JP JOURNAL OF PARAPSYCHOLOGY

Volume 73 Spring/Fall 2009

OBITUARY

BOOK REVIEWS

EDITORIAL

[*Editor's Note:* Back when J. B. Rhine was alive, he would occasionally publish editorials in the *JP*. Starting with the current issue, I am resurrecting this practice. The editorials will not appear in every issue, but neither will they be isolated events. I envision them as thought-provoking opinion pieces addressing fundamental issues facing contemporary parapsychology. Because I would like our "editorial page" to reflect a wide range of perspectives, most of the editorials will be guest editorials written by other prominent members of the parapsychological community. However, the first contribution should clearly be mine, hence the following.]

Winning Over the Scientific Mainstream

One of the most important goals of parapsychology over the years has been to convince the mainstream scientific community of the reality of psi, at least as a communications anomaly. I think it is useful to reflect periodically on what progress we have made in meeting this goal. My answer is, not much.

In the old days, we attempted to make our case through fool-proof (and magician-proof) "crucial experiments." This approach never succeeded, although the alternative explanations critics proposed were sometimes at least as implausible as psi itself (cf. Hansel, 1989). The whole rationale behind this approach is flawed, as even some critics have recognized (e.g., Hyman, 1981). Nowadays, the replicability of good-quality, if not perfect, experiments has taken over as the criterion of success. In the literature, at least, the battle has focused on the results of meta-analyses of groups of psi experiments of a common type. Success (and replicability) is claimed if the collective result of these experiments is statistically significant and the effect sizes of individual studies do not correlate with measures of methodological quality (Radin, 1997). Such an outcome has been claimed by parapsychologists in several instances. The most important of these is probably the ganzfeld, because it has been debated with critics in a very prominent mainstream psychology journal, *Psychological Bulletin* (e.g., Bem & Honorton, 1994; Hyman, 1994).

In a review of the ganzfeld debate, I concluded that the ganzfeld literature met the meta-analytic criterion for replicability (Palmer, 2003). However, I also noted that the large heterogeneity (variability) of the results illustrates that replicability is by no means universal, and that what replicability there has been is restricted to parapsychologists who are favorably disposed to the psi hypothesis. This latter point is particularly pertinent to the issue of mainstream acceptance of psi. In my view, even open-minded mainstream scientists (I am thinking especially of psychologists) will

not jump on board until a critical mass of mainstream scientists who have not been identified with parapsychology in the past obtain positive results in experiments they conduct. The more prestige these mainstreamers have in their own fields, the better. I don't think they need to be skeptics; benevolent neutrality is sufficient. Don't ask me to attach a number to this critical mass, but we clearly are nowhere near it at the present time.

The best recent example of such a mainstream contributor to our research literature is the prominent social psychologist from Cornell, Daryl Bem, who has repeatedly been successful in demonstrating psi in the context of the so-called "mere-exposure effect." As Bem has pointed out himself, his studies should carry additional weight with mainstream scientists because the methodological paradigm is one they use in their own field and is easy to implement. (Technically, Bem must be considered a parapsychologist more than an outsider, because he conducted these experiments after he had already taken a pro-psi position in his ganzfeld paper, and his psi research has not yet been published in a mainstream journal. However, he is close enough to an outsider that his research and general advocacy should carry some weight in the mainstream community.)

However, psi research by outsiders is very much a double-edged sword, as it can be exploited by researchers unsympathetic to psi to damage the credibility of the field even more effectively than the purely armchair critics of the past. Such an effort was recently published in a prestigious mainstream psychology journal, the *Journal of Cognitive Neuroscience* (Moulton & Kosslyn, 2008). I will discuss this experiment in some detail, partly because I want to get my criticisms on the record, but more importantly because it illustrates a powerful tactic that critics can use to damage parapsychology. The study is of particular interest to me because the first author is a former graduate of the Rhine Center's Summer Study Program (SSP). Its credibility is enhanced by the stature of the second author, Steven Kosslyn, Moulton's mentor at Harvard and a well-known and respected cognitive neuroscientist in his own right. He also is a member of the Scientific Board of the Bial Foundation, currently the major funder of psi research. The potential impact of the study really hit home when I found it cited as authoritative in my hometown newspaper.

The study compared brain reactions, as measured by fMRI, to psi vs nonpsi stimuli in unselected volunteer participants. The authors hypothesized that if psi is real, the fMRI should detect differences in brain responses to the psi and nonpsi trials. No such differences were found, leading the authors to boast that they had provided "the strongest evidence yet" against the existence of psi.

Although I have some quibbles with the experiment itself, the main problems are with the writeup and the authors' interpretation of its findings. First, the Moulton/Kosslyn paper was unbelievably deficient in reviewing previous research. The most serious problem was the failure to cite two psi-fMRI experiments that were quite similar to theirs (Achterberg et al.,

2005; Richards, Kozak, Johnson, & Standish, 2005). Both studies provided significant evidence of a psi-fMRI relationship. They were published in a peer-reviewed journal a little more than two years prior to the publication of the Moulton/Kosslyn paper, so there was plenty of time for the authors to cite them before submitting their report, and probably enough time to use them as guides for designing their own study. One thing the authors might have noted is that both these studies employed participants selected for presumed psi abilities—paranormal healers as senders (Achterberg et al., 2005) or successful participants in a pilot study (Richards et al., 2005)—rather than the ordinary volunteers they employed. However, the most important point for present purposes is that these two studies put a major dent in the authors' case for the nonexistence of psi, and the failure to refer to them makes their case appear to be stronger than it is.

The second major problem concerns the authors' rationale for claiming positive evidence against psi from negative (nonsignificant) results. They argue as follows:

> Although one can never affirm the null hypothesis, not all results are epistemologically equal. Because this paradigm uniquely minimizes assumptions about the source of knowledge, the kind of processing, or the nature of the mental content responsible for psi, any ensuing null results will be qualitatively more informative than those from behavioral methods. Moreover, we can compare any null results with positive results that reflect other aspects of the same stimuli; thus, conceptually, such null results can be considered part of an interaction, where one variable has effects but another does not. (Moulton & Kosslyn, 2008, p. 183)

Even if we grant the authors their metaphysical assumption of materialism, this argument is nonsense. The fact that the authors failed to find any significant physiological or behavioral evidence of psi simply means either that the psi process was not activated by their procedure or its manifestation was so weak as to be obscured by error variance. In either case, a crucial condition for testing the hypothesized psi-fMRI relationship was not met. This means that their study has the same impact on the psi controversy as any of the many other nonsignificant psi experiments—no more and no less. The existence of such a large number of negative results is precisely why parapsychologists have appealed to meta-analysis to draw conclusions based on the literature as a whole. That the editor and reviewers for a prestigious journal would accept such an argument is disturbing.

Not only is any argument of this type fundamentally flawed, but the authors' points are made in such a vague and abstract fashion that they cannot be readily understood. They do not even attempt to explain how

the methodological and conceptual virtues they attribute to their study answer the question at hand. Instead, they seem to be hiding behind a lot of sophisticated-sounding jargon. The average scientist reading the quoted paragraph is not likely to spend the time needed to grasp the argument and will simply assume it is valid because of who made it and where it was published.

When Moulton returned to Harvard after completing (with honors) the SSP, he obtained positive results in a psi experiment that appeared to be methodologically sound. In preparing this editorial, I consulted PsycINFO, the main search engine for finding psychological research, and could not find the experiment listed under his name. Thus, it appears he never published it. If he could not get it published in a mainstream psychology journal (a likely possibility given the results) he certainly could have published it in a parapsychological journal. Ironically, critics often accuse parapsychologists (unfairly) of only publishing results that support the existence of psi and leave the failures in the file drawer. It appears that Moulton has adopted the converse of this philosophy.

Can the behavior of Moulton and Kosslyn be explained by some combination of oversight and errors of judgment and logic, possibly mediated by their skepticism about psi, or by some other relatively innocent explanations I haven't thought of? Or does this behavior represent a deliberate attempt to mislead the scientific community and the general public about the status of the evidence for psi? I don't know—I'm not telepathic. In any case, the *effect* of this behavior is to misrepresent the status of the evidence for psi. All this could be cited by sociologists of science as a case study supporting the philosopher Feyerabend's (1993) cynical thesis that consequential science is less an objective search for truth than political game playing in the interest of promoting the prejudices and prestige of scientists and their institutions.

This lengthy diversion is an example of what we are up against in trying to convince the mainstream scientific community of the reality of psi. Moulton's student status should help dispel any illusions some might have that things will get better once the old generation of scientists dies out and a new generation takes over. As noted above, I think the key problem is the lack of reliability, or what Hyman (1989) properly calls the "elusiveness" of psi effects. The overwhelming majority of scientists (at least scientists of stature—the ones with the greatest stake in the theoretical status quo) will simply not accept statistical replicability as evidence for the existence of psi, however unjustified that conclusion may be. We need something pretty close to replicability on demand.

One thing that should be clear is that even the best psi practitioners are nowhere near up to this challenge. If they were, we could fund parapsychology for decades to come by sending them to Las Vegas and letting them loose in the casinos. The fact that we find the occasional strong psi effects we encounter so newsworthy simply underscores

their unreliability. Psychic healers are nowhere near replacing or even significantly assisting medical doctors for either diagnosis or treatment, to give just one of numerous examples of how far we have to go to make psi applicable in any meaningful sense.

Okay, so how do we get there? We can't wait for some super-talent to bail us out; that hasn't happened in the 100+ year history of our field, if it ever happened. Thus, we have to "grow our own," and that means psi training. Our literature records several attempts at training psi using feedback methods, but the results have been unspectacular at best (Palmer, 1978). However, the amount of training in these studies was so small that it would be astonishing had meaningful success transpired. College basketball players sometimes spend hundreds of hours practicing free-throw shooting, which involves nothing more than putting a ball through a hoop from a few feet away with no one guarding you. How much more complicated and delicate a process is developing ESP ability, especially given the high number of chance hits that must be mentally discriminated from the real ones? A better analogy of what is needed might be the years of rigorous discipline that the occupants of monasteries endure. Now, I realize that there are reasons to be pessimistic that such a long-term training program would lead us to the holy grail of reliable psi, even if we had promising participants to train. The answer to this challenge is simple: if we want to convince the scientific establishment of the reality of psi, psi training is the only game in town.

I continue to believe process-oriented psi research has a role to play in parapsychology by helping us to gain a theoretical understanding of psi. However, if we could find a way to make psi effects more reliable, it would make process-oriented research more efficient and productive, so we wouldn't have to do a meta-analysis or such every time we want to draw a conclusion about a relationship. Some of our process-oriented research also might provide guidance in how we should conduct psi training. I don't see any connection so far, but perhaps we could come up with something if we gave the matter some thought.

Finally, it should be noted that such a training program, particularly if it were to succeed, raises ethical issues. The development of more reliable psi will probably create stronger or more powerful psi as a by-product. Thus, the most important of these ethical issues is whether we would like to live in a world in which some of us, and maybe even all of us, had strong and reliable psi abilities. This is a complicated question, but, to put it metaphorically, before embarking on a journey it is a good idea to decide if you really want to reach the destination.

References

Achterberg, J. E., Cooke, K., Richards, T., Standish, L. J., Kozak, L., & Lake, J. (2005). Evidence for correlations between distant

intentionality and brain function in recipients: A functional magnetic resonant imaging analysis. *Journal of Complementary and Alternative Medicine*, **11**, 965–971.

Bem, D. J., & Honorton, C. (1994). Does psi exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin*, **115**, 4–18.

Feyerabend, P. K. (1993). *Against method* (3rd ed.). New York: Verso.

Hansel, C. E. M. (1989). *The search for psychic power. ESP and parapsychology revisited.* Buffalo, NY: Prometheus.

Hyman, R. (1981). Further comments on Schmidt's PK experiments. *Skeptical Inquirer*, **5**(3), 34–40.

Hyman, R. (1989). *The elusive quarry: A scientific appraisal of psychical research.* Buffalo, NY: Prometheus.

Hyman, R. (1994). Anomaly or artifact? Comments on Bem and Honorton. *Psychological Bulletin*, **115**, 19–24.

Moulton, S. T., & Kosslyn, S. M. (2008). Using neuroimaging to resolve the psi debate. *Journal of Cognitive Neuroscience*, **20**, 182–192.

Palmer, J. (1978). Extrasensory perception: Research findings. In S. Krippner (Ed.), *Advances in parapsychological research 2: Extrasensory perception* (pp. 59–243). New York: Plenum.

Palmer, J. (2003). ESP in the ganzfeld: Analysis of a debate. *Journal of Consciousness Studies*, **10**(6–7), 51–68.

Radin, D. I. (1997). *The conscious universe: The scientific truth of psychic phenomena.* New York: HarperCollins.

Richards, T. L., Kozak, L., Johnson, L. C., & Standish, L. J. (2005). Replicable functional magnetic resonance imaging evidence of correlated brain signals between physically and sensory isolated subjects. *Journal of Complementary and Alternative Medicine*, **11**, 955–963.

John Palmer

ABSTRACTS OF PRESENTED PAPERS FROM THE PARAPSYCHOLOGICAL ASSOCIATION 52ND ANNUAL CONVENTION SEATTLE, WASHINGTON, USA AUGUST 6–9, 2009

Editor's note: Abstracts of papers already accepted for publication in the *Journal of Parapsychology* are not included here.

ESP CONTRIBUTES TO THE UNCONSCIOUS FORMATION OF A PREFERENCE

JAMES CARPENTER[1], CHRISTINE SIMMONDS-MOORE[2], & STEVE MOORE[2]

ABSTRACT: This study is carried out as a test of some basic ideas drawn from first sight theory (FST). Some of the main ideas being tested include the assumption that extrasensory information has a part to play in the formation of all experience in a continuous, implicit, unconscious way. First sight theory also proposes that experience is created by unconsciously sampling all sources of potential information holistically, including psi information, and that all of these sources of information are treated in similar ways. And it also proposes that valuing the extrasensory domain of information, openness toward and interest in inward experience of the inadvertent sort that most implies unconscious processing (called liminal experience), being relatively free of fear, and being open to intimate communication with other people, also predispose one to make positive access to psi information.

The esthetic experience of preference is chosen as an everyday form of experience for study in terms of these propositions. Considerable research has demonstrated a tendency for persons to experience greater liking or attraction for things as a function of having been exposed to them previously. This is called the Mere Exposure Effect (MEE). This is often demonstrated especially strongly if the exposures are implicit, subliminally presented, and never available to awareness. The assumption is made here that an extrasensory MEE should obtain as readily as a subliminal one, and this study attempted to induce both in its participants.

Participants also responded to a number of psychological tests used here to assess aspects of unconscious motivation or intention. We measured whether or not one thinks ESP is possible in such situations, different aspects of openness to inner experience, tolerance of ambiguity, tolerance for interpersonal intimacy, creativity and fearfulness because FST predicts that each of these should relate to the utilization of extrasensory information. We also assessed the Need for Cognition, the Need for Structure, and Boredom-Proneness because these have been found to moderate the subliminal MEE.

Finally, we used a subliminal, implicit means to induce two different moods in our participants. Half were exposed to a stimulus aimed at enhancing a mood of symbiotic security and well-being, and consequent openness toward the situation, while the other half were exposed to a stimulus intended to evoke a mood of relative isolation, constriction, and vigilance.

Participants were subliminally exposed to a series of pictures, the mood manipulations, and a series of ESP targets (different pictures that were totally covered by an opaque block, such that they would convey no information even if seen supraliminally) in order to effect mood and induce the MEEs.

We expected to find an overall subliminal MEE but did not, and expected that participants would show similar directions of functioning in their subliminal and extrasensory MEEs, but they did not. We did find a number of predicted relationships with the motivational, attitudinal variables, particularly in the case of the extrasensory MEE, and particularly in the context of the induction of the open, positive mood. Discussion focuses on the development of a greater understanding of unconscious thought and how extrasensory information contributes to it.

[1]*Rhine Research Center*
Durham, NC, USA
[2]*Liverpool Hope University, Liverpool, UK*

EXPERIMENTAL PSI EFFECTS CAN BE PUT TO USE: TWO PILOT STUDIES

JAMES CARPENTER

ABSTRACT: I describe two studies that were designed to illustrate the potential applicability of laboratory-derived ESP effects in trying to predict events of practical consequence in the "real world." Both studies attempt to predict the behavior over a designated week in the future of some set of financial indices. The exact things used to represent targets for the studies were chosen secretly by experimenters not otherwise connected with the collection and analysis of data.

The studies followed up on earlier work on the prediction of scoring direction and scoring extremity using mood items, the California F-Scale, and Schmeidler's "sheep-goat" question. Predictors were empirically derived from all data that had been collected up to the time the current studies were conducted using stepwise multiple regression analysis. This program of studies may be viewed as an attempt to demonstrate the reliability of these predictive equations. The program can also be viewed as a series of efforts to make accurate predictions of events outside the laboratory that are yoked to the ESP targets with which participants are working. For example, a random number target might be picked in another laboratory using an REG and then those digits coded onto the ESP targets, or the rise or fall over a certain future period of the price of oil might be yoked to a certain target being guessed by study participants, using the binary alternatives + and -. Such efforts to predict future events have been part of this mood-scale

program of research from its inception. Participants are asked to guess over and over at the same set of targets, and then their responses are combined through a majority-vote analysis to generate a set of "best predictions" to be tested against the actual yoked outcomes.

In contrast to previous work in this program, analysis of these data was greatly facilitated by a scoring program written by Richard Broughton. Among other things, this program randomly reshuffles the same set of targets for each run of forced-choice guessing carried out by participants, thus avoiding the stacking effect due to positional preferences, and making statistical evaluation much simpler. The program also scores other predictors used in these studies (the California F-Scale and mood scales aimed at predicting ESP scoring direction and scoring extremity), sorts participants and runs into appropriate bins, and carries out prescribed operations on their guesses to permit a final tally of their rendered work. For example, a given run's performance might be expected to score above chance so its calls would be entered as they stand into a majority-vote analysis, whereas another run's performance might be expected to be below chance, so all its calls would be switched to their binary alternatives and these reversed calls added to the final tally. Predictions of scoring extremity were employed in a more complicated way involving the use of index samples derived from independently scoring responses to certain targets that have predetermined content. Then the scoring direction of the index calls was used to predict the scoring direction of the other, precognitive trials in the run, all following certain rules.

These studies were conducted partly to demonstrate the principles of ESP amplification procedures in planned lectures. Under the pressure of time, small samples were obtained in both studies, so reliability of the results was expected to be low. As it happened, both studies were incompletely analyzed at the time that they were conducted, and were only recently returned to for complete analysis.

The results of the first study were statistically significant and powerful enough in terms of amplification to have practical consequences. Eleven of twelve market indices and industry group comparisons were predicted correctly.

The second study was less effective. Results were nonsignificantly positive, with 7 of 12 similar entities being predicted accurately. Since this study also employed a different participant-soliciting experimenter, who took a more psi-facilitative attitude toward the groups with which she spoke, and since the study also engaged artistic persons as participants, it was deemed legitimate to conduct a secondary, unplanned analysis to see if the data as a whole, irrespective of the various predictors, might show psi-hitting. It did, in fact, to a significant degree, and a simple majority-vote procedure of the sort that would be done assuming psi-hitting was carried out and was fairly successful. Ten of the twelve indices were predicted correctly.

Discussion addresses possible concerns about the interpretation of these results in the case of the unplanned analysis of Study 2 hinging on the nonrandom nature of any real-world targets. One could imagine that some confluence of events might coincidentally occur such that targets might be highly correlated and happen by chance to covary with calls which might themselves coincidentally

have a predominance of one target type (for example, most market indices might happen to rise in value and be coincidentally yoked to a set of calls that happened to contain a predominance of the symbol "PLUS." In the particular analysis in question in Study 2 this would have potentially affected only half of the targets, and analysis makes it clear that this did not happen in fact. In order to eliminate this possibility for future research employing simple majority votes, a means is suggested that involves adding an additional randomizing step to the analysis. Then I address the potential implications of psi amplification using laboratory-based effects in regard to making parapsychology not only an interesting but also a useful branch of science.

Rhine Research Center
Durham, NC, USA

PSI AS FIRST SIGHT: A FORMAL STATEMENT OF THEORY

JAMES CARPENTER

ABSTRACT: The first sight model is intended to help understand the place of psi phenomena in human nature and to articulate a way of thinking about the mind in which psi could fit, and thereby to hasten the integration of parapsychology with general psychological science. The theory is aimed at helping to elucidate the rules governing the functioning of psi processes. Two fundamental postulates and 12 corollaries are presented. Several of the corollaries can be easily elaborated in testable directions and have already proven useful in helping to understand a great deal of published parapsychological research. While most of these ideas have been expressed before in other forms, here I further elaborate and organize them, and make clearer some of their empirical implications. The two postulates define the model. In them, I propose that organisms exist and transact continually in a nonlocal universe, that the mind thinks unconsciously about all of these transactions along with other unconscious transactions in a purposeful way, and that this unconscious thinking produces consciousness and other goal-directed experience. Nonlocal transactions (psi) are understood to be called upon actively and continuously in this process of unconscious work. The corollaries elaborate various aspects of the postulates and go beyond that as well in specifying various expectations about psi and other unconscious functioning that are consistent with the postulates.

Each corollary is named in a way that summarizes the gist of its conception. The Phenomenology Corollary asserts that unconscious mental functioning is best understood in terms of personal meaning rather than impersonal, biochemical process. The Personalness Corollary states that unconscious thinking is no less personal and volitional than conscious thinking; it is automatic, but not impersonal. The Ubiquity Corollary applies specifically to nonlocal (psi) apprehensions of reality and asserts that they are always active constituents of the plethora of potential meaning that the mind works on unconsciously. Since they are

always available, and always accessible prior to immediate sensory information, psi may be referred to as first sight. The Integration Corollary states that all sources of potential information (extrasensory, sensory, subliminal, memorial and intentional) are available together to unconscious thought and that the processing of these sources will generally follow the same patterns. The Anticipation Corollary states that unconscious thought seeks to adequately anticipate the content of experience, and behaviorally respond in an optimal way to developing circumstances, and that different streams of potential meaning offer varying suggestions about how experience might be best constructed and behavioral choices best guided. The Summation Corollary proposes that unconscious thought works by a kind of holistic summarizing across the many implications of meaning. The Bi-directionality Corollary says that in this summarizing process each element of potential meaning is either approached or avoided, and thence it either contributes additively or subtractively to the resultant experience or nonconscious volitional behavior. The Intentionality Corollary asserts that it is unconscious intention that guides this choice of assimilating or disassimilating the particular potential meaning. This corollary spells out many patterns by which this unconscious choosing is carried out (these proposed patterns constitute testable hypotheses for research to explore). The Switching Corollary states that the intentional posture toward a given element or source of meaning may be either relatively consistent or inconsistent over time. The Extremity Corollary goes further to say that if the intentional posture is consistent, a definite addition or subtraction to the ultimate experience or behavior will be evident. If it is inconsistent and switches rapidly, no reference to that element will be evident. The Inadvertency Corollary reminds us that psi processes are intrinsically unconscious, but that their action may sometimes be seen implicitly by virtue of the anticipatory cognitive and affective associations that the mind has aroused with them. Finally, the Liminality Corollary says that if a person consciously attends with interest to those aspects of experience that most imply such unconscious prompting, he or she will be more likely to express psi information either in terms of good conscious guesses or in terms of less conscious goal-directed behaviors. It also asserts that a person's emotional state influences the degree of openness to contextually implicit (liminal) information, including psi.

Rhine Research Center
Durham, NC, USA

PARANORMAL HEALING, PARANORMAL BELIEF, AND PHYSICAL AND PSYCHOLOGICAL WELL-BEING IN ARTHRITIS SUFFERERS: A SMALL-SCALE CLINICAL TRIAL

ALISON EASTER & CAROLINE WATT

ABSTRACT: Background and Objective: Although a number of studies have tested different healing techniques in controlled settings, few have investigated

the role of the patient's or "healee's" belief and expectancy in their response to the healing intervention. This small-scale randomized controlled clinical trial of distance healing with arthritis sufferers aimed to address this question by using a semimasked design in which half of the participants were aware of whether or not they were receiving distance healing.

Participants: Sixty patients were recruited from a rheumatology outpatient clinic, through online support networks and blogs and via word of mouth.

Intervention: Healers from various backgrounds were self-referred and chosen based on their self-reported experience (four were members of the National Federation of Spiritual Healers and two were certified Reiki masters). Healers were expected to practice healing for each participant at least once a week and to keep a log of the frequency and duration of healing. Although healing frequency and duration varied, each participant received between 30 minutes and an hour of healing each week.

Outcomes: The primary outcome measures were the General Health Questionnaire (GHQ-12) and the Short-form McGill Pain Questionnaire. In addition, the Paranormal Belief Questionnaire and a measure designed to assess belief in distance healing were given in an effort to determine if this had an effect on self-reported change of physical and mental health. In addition to primary outcome measures, the IPIP personality scale, Spiritual Connection Scale, Satisfaction with Life Scale and a brief index of dietary habits were administered as exploratory measures.

Results: The study results found no significant main effects of healing or of knowledge of condition placement, and no significant interactions between the two. Results suggest that although generalized belief in healing seems to have little effect on self-reported pain, health, and well-being, specific knowledge about whether or not one is receiving distance healing appears to be associated with improved outcomes for those who are in the healing group. This point is shown in the effect size of GHQ change scores for the healing and no-healing groups not masked to their condition ($d = 0.76$). The effect size suggests that the expectancy or knowledge of receiving distance healing may have a positive effect on participants' general health. However, due to the low statistical power of this study, this difference was not statistically significant in the ANCOVA. For those who were unaware of whether or not they were receiving healing, there was no evidence of improved outcomes for the healing group.

Conclusions: The hypothesis that there can be health gains through distance healing alone was not supported. One area identified for future research concerns individual differences in healer efficacy. In this study healer allocation added significance to the statistical model, but due to small sample size this point calls for further investigation.

Koestler Parapsychology Unit
University of Edinburgh, UK

EVIDENCE FOR SUBCONSCIOUS BUT NOT CONSCIOUS PSI IN REMOTE STARE DETECTION AND PRECOGNITION TASKS

JULIA A. MOSSBRIDGE, MARCIA GRABOWECKY, & SATORU SUZUKI

ABSTRACT: To more completely examine the relationship between conscious and subconscious psi performance, data for both kinds of performance would ideally be collected simultaneously. However, studies examining two replicable forms of subconscious psi perception, remote stare detection and presentiment, are generally performed in the absence of a concurrent behavioral task. This choice is probably based on the conviction that physiological evidence for these phenomena is most easily obtainable when participants are not being asked to engage conscious psi abilities. The few studies that have simultaneously gathered both behavioral and physiological data have generally found null results for both conscious and subconscious psi.

To address this gap in knowledge, we set out to test three hypotheses: (1) conscious remote stare detection is possible and evidence for it can be obtained via the performance of a two-interval forced-choice (2IFC) task during the concurrent measurement of physiological data, (2) remote staring produces physiological changes in the individual being stared at; these changes are apparent as tonic and/or phasic effects in pulse periods and/or skin conductance across individuals, and they are present even when individuals are simultaneously asked to consciously determine when they are being stared at, and (3) stimuli that produce different levels of arousal after their occurrence also produce different levels of arousal before their occurrence; these differences are apparent as tonic and/or phasic effects in pulse periods and/or skin conductance across individuals, and they can be measured even when individuals are simultaneously asked to consciously predict the identity of a future stimulus.

To test these hypotheses, we collected behavioral and physiological data from a group of 20 Northwestern University undergraduates who each participated in a 1.5 to 2-hr session in which they performed three conditions. We referred to these conditions as the remote stare-detection (RSD), precognition, and remote-stare-detection control (RSD control) conditions; these were performed in the order listed. The *RSD condition* consisted of 30 two-interval forced-choice (2IFC) trials in which the participant was asked to distinguish the 10-s interval during which s/he had been remotely stared at through a video camera (the staring period) from the 10-s interval in which s/he had been unobserved (the nonstaring period). The *precognition condition* consisted of 25 single-interval trials in which the participant was asked to guess which of four images would later be revealed as the "target" image on that trial. The *RSD control condition* was the same as the RSD condition, except the subject was unobserved throughout the entire condition. Intervals in the RSD control condition are referred to as "staring" vs. "nonstaring" periods (in quotes), to indicate that although the computer software marked the intervals as such, there was no actual observation of the participant during this condition. Note that in this condition, participants received false feedback so that

to the participant, in all respects this condition was the same as the RSD condition. Electrodermal activity (skin conductance or SC) data and pulse period (interbeat interval or IBI) data were recorded throughout all three conditions. All analyses used planned two-tailed comparisons at $\alpha = .05$.

Confirmatory and exploratory analyses supported only the two hypotheses regarding physiological or subconscious effects. Neither individual nor group data revealed behavioral or conscious performance above chance in any of the three conditions.

In terms of confirmatory analyses of physiological data, an examination of tonic effects in the *RSD condition* revealed that interbeat intervals were, on average, significantly higher during staring than nonstaring periods in the first interval, $t(16) = 2.96$, $p = .009$; $d = 0.72$, and significantly lower during staring than nonstaring periods in the second interval, $t(16) = -2.27$, $p = .037$; $d = 0.55$. Examination of tonic effects in skin conductance data revealed no significant differences between average skin conductance in staring versus nonstaring periods in either the first or second intervals (both $ps > .244$) in this condition. Analysis of phasic effects in the RSD condition revealed that average difference traces based on IBI traces from staring versus nonstaring periods showed several contiguous regions of statistical significance, including the region between about 6 and 8.5 s in the first interval (stare IBI > nonstare IBI) and two regions in the second interval: between about 0 and 1 s and between about 9 and 10 s (stare IBI < nonstare IBI). Difference traces derived from skin conductance data revealed no significant regions.

Support for these results may arise from a true staring effect. Confirmatory analyses of data from the *RSD control condition* revealed no significant physiological tonic or phasic effects. Averaged IBI and SC data did not differ between staring and nonstaring periods in either the first (IBI: $p = .995$; SC: $p = .686$) or second (IBI: $p = .194$; SC: $p = .656$) intervals. Further, no regions of statistical significance were found in the average difference traces derived from either pulse period or skin conductance data recorded in the RSD control condition.

In a follow-up exploratory analysis, an algorithm based on the differences observed in physiological data in the RSD condition was applied to the pulse period and electrodermal data for each participant on a trial-by-trial basis. Suggesting that the physiological differences observed between staring and nonstaring periods were consistent across individuals, this algorithm predicted which interval was most likely to be the staring interval at a rate that was, across participants, better than chance in the RSD condition, but not in the RSD control condition. RSD condition: $t(16) = 3.89$, $p = .001$; $d = 0.94$; RSD control condition: $p = .536$; paired $t(15) = 3.69$; $p = .002$; $d = 0.92$.

Regarding the *precognition condition*, confirmatory analyses revealed no evidence of tonic prefeedback effects in either pulse period or skin conductance data; paired t tests between collapsed averages across prestimulus periods from correct versus incorrect trials were not significant for either measure (both $ps > .272$). However, confirmatory analyses of phasic prefeedback effects revealed a >

500 ms region in which IBI values were significantly higher on correct trials than on incorrect trials; this region was temporally centered around 5.5 seconds preceding feedback. Supporting the validity of this result, no regions of significance were found in the control analyses of the same pulse period data sorted by correctness of the previous trial. No regions of significance were found via phasic analysis of skin conductance data.

We used exploratory analyses to examine whether such presentiment-like responses might also have occurred on correct versus incorrect trials of the RSD and RSD control conditions and found no evidence for such responses in the RSD condition. However, exploratory analyses of phasic prefeedback effects in the RSD control condition revealed two regions during which skin conductance values were significantly higher preceding feedback on correct versus incorrect trials (~7.75-8.25 and ~6.25-6.75 ms before feedback). Again suggesting a genuine presentiment-like effect, albeit different from that found in the precognition condition, there were no prefeedback differences when the same data were reanalyzed after sorting according to correctness on the previous trial.

Overall, the results do not provide evidence for conscious behavioral performance of remote-stare detection or precognition, but they do support the existence of subconscious psi effects that occur during the performance of remote-stare detection and precognition tasks. The present experiments, performed in an independent perceptual neuroscience laboratory in a mainstream scientific context, provide replications of a subset of data focusing on remote staring and presentiment and offer novel avenues for the investigation of the mechanisms underlying these and other psi phenomena. A replication attempt is currently under way and will be briefly discussed along with the results described here.

Visual Perception, Cognition and Neuroscience Laboratory
Department of Psychology, Northwestern University
Evanston, IL, USA

THE HAZARDS OF REDUCTIONISTIC NEUROSCIENCE INTERPRETATION: REVISITING ANOMALIES AND SUBJECTIVE EXPERIENCE LINKED WITH THE BRAIN

VERNON M. NEPPE

ABSTRACT: Several recent neuroscience research publications on alleged psi abilities and their brain correlates are examined. Superficial evaluations of these studies could support the model of materialistic reductionism, namely, that specific psi experiences derive solely from brain physiology. Detailed analyses in this paper dispute this conclusion.

Five different objective or subjective psi-brain situations are evaluated:

1. How stimulating focal areas of the brain produces "out-of-body experiences" (OBEs). These are in marked contrast to descriptions of spontaneous out-of-body experiences.

2. How physiological symptom complexes correlate with near-death experiences (NDEs). Specifically, the Nelson group in 2006 reported how NDEs may actually be symptoms relating to the physiological mechanism of REM intrusion.

These studies (#1-2) evaluated retrospective subjective paranormal experiences (SPEs), not psi itself, because the reports involved purely subjective experiences.

3. How functional magnetic resonance imaging (fMRI) can be used to evaluate the existence of extrasensory perception. In the well-publicized Moulton study of 2008, the objective of evaluating the neural correlates of ESP was not tested because ESP was not demonstrated. This study contrasts with several lesser known, but methodologically sound, related fMRI studies: These are all positive for the interactive role of nonconscious processes (including presentiment research) between two individuals.

These studies all involved attempted brain imaging of actual psi (OPE—objective paranormal experience).

Detailed analyses of the stimulation of focal brain areas and "OBE" (in #1), of the REM intrusion / "NDE" study (in #2) and of Moulton's fMRI brain study (in #3) all reveal flawed methodology and/or misinterpretations.

The author suggests two other brain models. These could highlight a more appropriate, productive approach (#4 and #5):

4. How the study of reported subjective paranormal experiences (SPEs) can be correlated with detailed clinical analyses of symptoms reflecting focal brain functioning. The example used involves analyzing the links of the temporal lobe of the brain with SPE. This was done in the 1983 Neppe research and the subsequent Palmer and Neppe study performed in 2002. Effectively, these generated similar findings in retrospective controlled SPE studies with both Subjective Paranormal Experients and Temporal Lobe Seizure patients. Subjective Paranormal Experients have significantly more "state" and "trait" temporal lobe symptoms than Nonexperients; and temporal lobe dysfunction patients have more SPEs than an appropriate comparative population. These findings are therefore "bidirectional." (They involve A linked with B, and B linked with A.)
5. How future brain-psi research can be facilitated by examining anatomical and functional brain areas. As an example, I hypothesize in this paper that the frontal lobe is a brain area of relevance for motor psi phenomena (psychokinesis) and that more speculative hypotheses would evaluate whether unconscious afferent psi is linked with the thalamus, whether hormonal measures of psi are correlated with hypothalamic function, and whether unconscious motor phenomena may involve the basal ganglia.

The approaches reflected in all five of these areas suggest that analysis of SPEs constitutes a major phenomenological methodological approach to brain research. Phenomenological analyses have variable levels of application: They may be less relevant in studies where there are direct attempts at inducing psi (as in #3). Even then, detailed descriptive analyses are still relevant.

Such phenomenological analyses allow nonprejudicial evaluation of subjective interpretations of ostensibly anomalous, psychic or intuitive experiences. Effectively, they apply the medical history-taking approach in assessing psychopathology (e.g., as in analyzing auditory hallucinations phenomenologically).

The approach neither confirms nor denies the validity of the SPE itself, suspends judgments of "pathological" versus "normal," emphasizes differentiation by phenomenological detail, and may, if extended to the lab situation, correlate subjective with objective empirical research. Specifically, detailing SPEs facilitates correlating epiphenomena with brain localization: Dichotomous descriptions of subjectively interpreted experiences require careful phenomenological differentiation because different origins and etiologies could be inappropriately interpreted as one.

In 2003, Neppe suggested eight methods to facilitate analyzing phenomenological data descriptions in the brain, namely, detailed data analysis, appropriate nosological subtype comparisons, specific pathophysiological context, limitations of single cases, literature comparisons, examination for unified brain localizations, correlations not implying causality, and the unproven origins of phenomena.

New principles can be espoused from the above research:

- The absence of demonstrating correlates of psi in the brain could imply insufficient or inappropriate methodology to elicit psi, not its nonexistence.
- The actual source of a subjective experience is not currently provable—SPEs could partly derive from exogenous (outside the brain) or purely endogenous (inside the brain) origins. These are unproven fundamental philosophical mind-body concepts.
- Correlations of SPEs neither confirm nor deny the veridicality of psi.
- Correlations are not necessarily causal, and specific interpretation guidelines for SPEs should exist.
- The medically tested diagnostic bidirectional approach can be usefully applied. Utilizing two converse SPE populations may clarify their links: Positive results would, nevertheless, fall short of implying causality but would further strengthen the link between the two events more than a unidirectional assessment when only correlations are implied.
- Methodology including anticipated interpretations of findings should be predefined: Unexpected results should not lead to rule changes.

- Methodology must be carefully justified including recognition of parapsychological theory (e.g., experimenter effects, psi conduciveness, the sheep-goat effect, and signal-to-noise ratios).
- The subjective experience model allows correlation with objective events (e.g., by EEG, fMRI, PET, clinical tests).
- Ostensible causal links reflect just one necessary but insufficient requirement for SPEs to be expressed in the brain. A functioning brain still requires other organs such as legs to walk.
- Adequate controls have to be set up to make appropriate sense of the results. These controls are sometimes very difficult to implement.
- Interpretations should not go beyond what is tested.
- Subjective experience studies have retrospective descriptive elements.
- Ecological validity is difficult to attain as each situation is different; the materials and setting of psychological, neuroscience and parapsychological studies seldom approximate the equivalent real-life situations. This information should be recognized in detailed phenomenological analyses as, when replicated, "like" may not be "like."

By applying these principles to the above research (#1-3) the limitations of brain reductionism are demonstrable in these SPE and OPE studies. By contrast, approach #4, using phenomenological detail in controlled planned research protocols, demonstrates advantages for such clinical studies, producing more robust results. This is so as these studies can more easily:

- involve larger sample size
- utilize more clinical measures
- be performed as the costs are far lower, and very little, if any machinery, is required
- emphasize phenomenological neuroscience, examining symptoms and clinical features using a well-established medical model
- obtain state (objective examination or simultaneous SPE correlates) and trait (historical) data
- utilize mechanisms of modifying psi performance by manipulating the variables through, for example, biofeedback or drugs.

Neuroscience and psi evaluations are complex and the rules should be as fair for studies of psi and possible anomalous phenomena as they are for all other scientific disciplines: A baseball analogy can demonstrate how farcical misinterpretations can be, but such comparisons have their limitations when referring to psi research.

Pacific Neuropsychiatric Institute
Seattle, WA, USA, &
Department of Psychiatry, St. Louis University
St. Louis, MO, USA

PHENOMENOLOGICAL ANOMALISTIC PSYCHOLOGY: ENSURING HOMOGENEOUS DATA COLLECTION FOR PRESENT AND FUTURE RESEARCH ON POSSIBLE PSI PHENOMENA BY DETAILING SUBJECTIVE DESCRIPTIONS, USING THE MULTIAXIAL A TO Z SEATTLE CLASSIFICATION

VERNON M. NEPPE

ABSTRACT: Spontaneous apparently anomalous experiences are often classed together on the basis of brief common descriptions, when they may be phenomenologically and etiologically divergent. Heterogeneous results can lead to mistaken pooling together of dissimilar events with hypothesized single etiologies, even though, in actuality, they do not reflect the same type. An example is "out-of-body experiences" induced by brain stimulation in epileptic patients compared with spontaneous events in "subjective paranormal experients."

This paper motivates detailed multiaxial evaluations of spontaneous, experimental and induced anomalous experiences. It is modeled on the successfully applied multiaxial psychopathology classification of the American Psychiatric Association, namely the Diagnostic and Statistical Manual (DSM), currently DSM IV-TR.

The same approach can be valuably and successfully applied to parapsychological research and to all subjective or objective spontaneous, induced, or experimental biopsychophysical phenomena. This implies a conceptual shift away from the attempted, and at times, impossible objectification of psi, to the detailed analysis of specific characteristics and events based on a multisystems biopsychofamiliosociocultural, anatomicophysiological and detailed physical models that allow for applying detailed criteria and descriptions.

This nonprejudicial approach of examining spontaneous and experimental "subjective paranormal/psi experiences" (SPEs) makes ostensible but unproven psi phenomena easier to tame and far less threatening. The shift in emphasis is from objectification and proof of truly paranormal phenomena to commonalities of, for example, specific cerebral function. Locating a correlative brain area or mechanism or chemical for processing of such subjective experiences becomes a legitimate alternative correlative approach to conceptualise psi. By such means, SPEs, like hallucinations, delusions, or déjà vu, can be measured and scientifically phenomenologically subtyped.

This detailing of anomalistic psychology research has allowed the author to extend the discipline of parapsychology from the objective approach into a second major school, phenomenological parapsychology.

Examples of the early origins of phenomenological parapsychology (1977-on) include the diagnostic entity of Subjective Paranormal Experience Psychosis; links of the physiological features of temporal lobe functioning to SPEs; demonstrable plurality of the déjà vu phenomenon where four different nosological subtypes were demonstrated; and biological measures of outcome such as pharmacological responsiveness and toleration differences in Subjective

Paranormal Experients versus schizophrenics. These demonstrate that the empirical phenomenological analytical parapsychological approach is valuable.

Specifically, such research allows fruitful hypotheses that not all subjective paranormal experiences derive from or are associated with the same brain locus or are predisposed to by the same specific psychopathological or psychological conditions, states, or traits. They empirically justify the need for a consistent multiaxial classification system. This allows interpretations of like with like to occur, not like with unlike. Detailed description of subjective phenomena produces interpretable results; its neglect could produce inappropriate generalizations of the key basic range of parapsychological experiences. These principles have been critical, guiding sources for a flexible, detailed multiaxial classification analysis of alleged psi experiences.

Rare nonartifactual positive original results in experimental psi research may not be replicated because tightened or different experimental controls might remove special environmental, interpersonal, and psychological psi-conducive effects. Contradictory results become a norm as replication attempts produce declining psi phenomena, possibly because of different biopsychophysical circumstances. This leads to the paradox of the inherent nonreplicability of psi because of subtle experimental changes. In reality, epiphenomena may reflect vastly different origins: documentation of differences in experimental protocol is key for the future of the discipline. Similarly, we must detail the second major domain of parapsychology, namely investigation of spontaneous phenomena. An example of detailed phenomenological analysis—namely, the Neppe déjà vu study—illustrates the real-life research application of such phenomenological analyses.

Like psychiatry, parapsychology needs a multiaxial classification system. I propose a tentative 26-level description of anomalous experience called the Subjective Experience of Anomalous Trait Typology Evaluation (SEATTLE). The SEATTLE was developed from and involves a significant modification of the author's original 1985 10-axis Multiaxial System for Anomalous Events. This did not include many key features, therefore requiring amplifications.

The SEATTLE ensures that the possible errors of classifying heterogeneous phenomena into single subgroups can largely be eliminated. There are 26 SEATTLE axes running from A to Z, in a workable order, not too contrived, allowing researchers to classify in order from A to Z. The first 10 letter Axes (A-J) detail Specific Features pertaining to the data given by the subjects. Thereafter, more General Factors (Axes K-Q) examine general biopsychosocial subject characteristics and pertinent factors. Finally, attempted possible Interpretations of Phenomena (R-Z) by experts (clinicians, statisticians, parapsychologists) follow. This multidisciplinary approach can be applied to both the experimental and the spontaneous. Even retrospective data can be classified, even when there are gaps in the data so that not all of Axes A to Z will be complete, as these are still better than no comparative data.

The SEATTLE classification allows for potential worldwide collaborations and a major new funding direction. Most importantly, a more

unified multiaxial database can arise with developments like those that occurred in psychiatry with DSM can occur; neologisms will happen as they did with this process in psychiatry. Every A to Z axis is special and has ongoing developing pertinent subdivisions.

We have accumulated large amounts of data over the past 5 years of individual subjective precognitive impressions, though frequently there are information gaps. An example is given of a complex "precognitive dream" with profound dynamic elements, with the application of the classification to this.

The SEATTLE can be applied to every subtype of SPE and objective experience, whether spontaneous, experimental, or induced. Ultimately, SEATTLE data analyses allow research and clinical meta-analyses of anomalous events where important phenomenological commonalities and differences could allow significant theoretical, paradigmatic, and research advances.

The SEATTLE axes involve preliminary, novel attempts to clarify developing a multiaxial system for describing subjective paranormal experiences. Until we consistently document each and every experience in detail, we will create nonreplicability and heterogeneity, even in our experimental protocols. The SEATTLE allows us to consistently document attitudes and expectations of the experimenters, subjects, and observers and to realize that a supposed replication was not a true replication, because significant data sets were different.

Dialing a complex telephone number produces entirely divergent results when one digit is in error: In psi, we realize that no-replicability may be because exactly the same phenomenology was not researched. The SEATTLE now requires empirical testing and routine use in research.

Pacific Neuropsychiatric Institute
Seattle, WA, USA, &
Department of Psychiatry, St. Louis University
St. Louis, MO, USA

CONSCIOUSNESS AND THE QUANTUM WAVE-FUNCTION: EXPERIMENTS WITH AN OPTICAL DOUBLE-SLIT SYSTEM

DEAN RADIN[1], PAUL WENDLAND[2], & ROBERT RICKENBACH[3]

ABSTRACT: An optical double-slit system was used to study an interpretation of the quantum measurement problem—that consciousness collapses the quantum wave function. In randomly counterbalanced periods, participants focused their attention toward or away from an optical double-slit apparatus while the photon interference pattern was continuously recorded by a high-resolution line camera. The measurement of principal interest was the ratio between the spectral power of the double-slit versus single-slit patterns. This value was predicted to decrease during periods of remote observation by the "mind's eye."

Phase I of this project involved 40 test sessions contributed by 12 unselected participants. Each session consisted of 40 randomly counterbalanced

observation versus no-observation periods of 15 s each. Analysis of the data showed that the spectral ratio measure decreased as predicted (p = .005, one-tailed, determined by randomized permutation analysis). Forty additional sessions conducted with no observers present to test the hardware and analytical procedures produced results consistent with chance (p = .734).

A planned analysis found that the spectral ratio measure in the observation condition dropped over 12 standard errors below the grand (z-score normalized) baseline mean of zero, whereas the same measure in the no-observation condition dropped 0.1 standard errors. This indicates that the differential effect obtained in this study was principally due to a statistically robust decline in double-slit spectral power during the observation condition, in accord with the hypothesis that consciousness influences the quantum wave-function.

Phase II followed a similar design with 20 preplanned sessions, 30-s observation versus no-observation periods, and 5 unselected participants. It also provided real-time audio feedback of the results to allow the entire experiment to be conducted with eyes closed. The outcome again was in the predicted direction (p = .017).

Across Phase I and II, 43 of the 60 test sessions (72%) produced results in alignment with the hypothesis, suggesting that the overall results were not due to a few outlier sessions. Of 11 sessions contributed by 6 participants with an active daily meditation practice, 10 (90%) were in the predicted direction. It should be noted that the results of this experiment could be interpreted as an experimenter psi effect, although the consistency of results across sessions argues against that interpretation.

A double-slit system appears to be a useful tool for studying the effects of consciousness on physical systems; it also offers the advantage of being directly relevant to an outstanding issue in mainstream physics—the quantum measurement problem. Further tests using this system are under way.

[1]*Institute of Noetic Sciences*
Petaluma, CA, USA
[2]*Thousand Oaks, CA, USA*
[3]*Micronor Inc.*
Thousand Oaks, CA, USA

EFFECTS OF PSYCHIC HEALING INTENTIONS ON PATTERNS OF COSMIC RAYS

GARY E. SCHWARTZ & MARK BOCCUZZI

ABSTRACT: As reviewed in G. Schwartz's 2007 book *The Energy Healing Experiments* and Tart's 2009 book *The End of Materialism*, a large body of evidence exists suggesting that human intention—both individual and collective—can have local and distal effects on living systems. Numerous psychic and spiritual healing traditions posit that some sort of "Universal Energy" is invited to enter

the consciousness and body of the healer; the healer is taught to use his or her intentions to direct this energy for "the best and highest good" of the patient or client. It is typically assumed that this energy or "higher power" is of a "high frequency"; however, to the best of our knowledge, no laboratory research has been conducted to address this assumption. Using a computer controlled low light CCD camera system cooled to -77 degrees centigrade, sporadic bursts of high energy, high frequency cosmic / gamma rays are detected; these gamma bursts are typically treated as "noise" and removed prior to statistical analysis. However, we theorized that patterns of cosmic ray activity, analyzed using fast Fourier transform (FFT) images generated by ImageJ analysis software (available from the National Institutes of Health), might reflect the presence of high-frequency energies purportedly generated during psychic healing practices. After pilot studies suggested that FFTs of cosmic ray patterns changed during the practice of psychic healing, a carefully controlled experiment was conducted. The camera's lens was focused on a stage containing white graph paper in a completely dark, light-tight metal chamber. The light-tight chamber was housed in a temperature controlled light-tight room. The computer and research assistant were housed in a separate room. Each run consisted of a 30-min baseline 512 x 512 pixel image that was subtracted from four 30-min data image trials. Eight runs involved a spiritual healing practitioner intending that the Universal Energy enter the "distant" light-tight chamber housed in the separate room; 4 runs involved the third 30-min data trial and 4 runs involved the fourth 30-min data trial. Eight matching runs had the same practitioner perform an intention meditation control where he focused his attention on imaging the stage in the chamber, but without inviting the Universal Energy to participate. Sixteen runs were conducted as "blank" trials to control for possible order and time effects. Cosmic ray images were generated for each of the 128 data trials. FFT images were calculated, and Plot Profile statistics provided by ImageJ software were performed. Analyses of variance revealed a highly significant ($p < .0000001$) condition by averaged pixel interaction for the Universal Energy compared to the Meditation and Blank Controls. Alternative interpretations of the findings, including experimenter intention and belief, are considered.

Laboratory for Advances in Consciousness and Health
University of Arizona, Tucson, USA

A STUDY TO ASSESS THE VALIDITY OF APPLIED KINESIOLOGY (AK) AS A DIAGNOSTIC TOOL AND AS A NONLOCAL PROXIMITY EFFECT

STEPHAN A. SCHWARTZ,[1] JESSICA UTTS,[2] S. JAMES P. SPOTTISWOODE,[3] CHRISTOPHER W. SHADE,[3] LISA TULLY,[4] WILLIAM F. MORRIS,[5] & GINETTE NACHMAN[6]

ABSTRACT: Applied Kinesiology (AK) is a diagnostic technique widely used within the Integrative Medical community. In essence it posits that a question can be mentally held in a person's mind, sometimes while they are holding a

substance like a vitamin, or a food sample, and by measuring relative muscular weakness an answer as to whether the substance or the condition represented by the question is good for that person can be obtained. This AK is presumed to have a diagnostic capability. That being presumed, this study asks: (1) Is there a difference in muscular strength when an individual holds a substance that is inimical to life processes (a poison solution), as compared to a substance that is essential for life (normal saline)? (2) Is this effect a transaction involving input from both the person being measured and the kinesiologist doing the measurement, or is it only the person being measured? (3) As an extension of question 2, is the result the same when different kinesiologists take the measurement, or when no kinesiologist is involved? (4). Does belief, expectation, gender, or time cognition play a role in determining the response? To answer these questions, which would help to define the parameters of the AK process, 51 participants were tested during three trials each, first by one kinesiologist, then by another and, finally, with no kinesiologist present by grip strength indicated using a hand dynamometer, grip strength being a self-administered AK test of relative muscular strength. For each trial a pair of randomly numbered sealed vials, each pair in a randomly numbered plastic bag, was used as the objects of the trial. In each bag one vial contained saline solution while the other was filled with a slightly smaller amount of saline solution to which had been added ionic hydroxlamine hydrochloride (NH3OH)+, producing a toxic solution of 9 mg/ml. Each trial consisted of a separate muscle test for each vial. All present at the trials were blind as to which vial contained the toxin. And all who prepared the vials were blind to the trials. The force used by the kinesiologists in each of their trials was measured via a pressure pad system. The hand dynamometer trials were conducted with no kinesiologist present.

Results: Of the 151 sets of trials, the toxic vial was identified correctly in 80 of them (53%), resulting in a one-tailed exact binomial p value of .258. Results for two of the kinesiologists were almost exactly at chance. For the third kinesiologist there was a one-tailed exact binomial p value of .18 (unadjusted for multiple testing). Results for the dynamometer were also almost exactly at chance. Testing whether there was a significant difference in proportions for whom the AK test worked based on belief about whether it would work resulted in nonsignificant chi-square values of 0.6 ($p = .439$) for the trials with one kinesiologist, and 2.222 ($p = .136$) for the hand dynamometer trials. The final variable examined was gender. While there was no significant difference in performance for males and females for the trials of the male kinesiologist or the hand dynamometer, the combined data for the two female kinesiologists did reveal a difference. Of the 33 sessions with females, only 15 were successful (45%) while for the 18 sessions with males, 14 were successful (78%) resulting in a chi-square statistic of 4.96, $p = .026$. However, given all of the chi-square tests performed in this section, the results must be interpreted with caution because of multiple testing. Results indicating belief in whether or not the AK test will work were not significantly related to whether or not it actually did work. A chi-square test of the relationship between time perception and correct vial choice showed no significant relationship. A chi-square test of the relationship between time perception and correct vial choice

showed no significant relationship. The chi-square statistic for the relationship using the hand dynamometer data was 0.927, $p = .629$.

The data in this study, particularly when seen in the larger context of a review of the literature from the AK field itself by Klinkoski and Leboeuf (1990), which considered 50 papers published between 1981 and 1987 by the International College of Applied Kinesiology, and the survey by Hall, Lewith, Brien, and Little, using standard evaluation criteria (QUADAS, STARD, JADAD and CONSORT) for research methodology, as well as six prior nonclinical studies, by Radin, Quintanar, and Hill; Braud, Arnett, Friedenberg, and Kendler; Ludtke, and Kendler; and Keating, all together suggest: The research published by the Applied Kinesiology field itself is not to be relied upon, and in the experimental studies that do meet accepted standards of science, Applied Kinesiology has not demonstrated that it is a useful or reliable diagnostic tool upon which health decisions can be based.

[1]Atlantic University, Virginia Beach, VA, USA, &
Cognitive Sciences Laboratory, Langley, WA, USA
[2]Department of Statistics, University of California, Irvine, USA
[3]The Resource Group, Beverly Hills, CA, USA
[4]Quicksilver Scientific, Boulder, CO, USA
[5]A.T. Still University – Mesa, AZ, USA
[6]Rhine Research Center, Durham, NC, USA

ASSOCIATIVE REMOTE VIEWING: THE NEXT CANDIDATE FOR THE PARADIGMATIC PSI EXPERIMENT?

PAUL H. SMITH

ABSTRACT: Ray Hyman has issued a challenge, that parapsychology has no "paradigm experiment" that can be replicated by students and others to reliably demonstrate some core finding of the field. Though various candidate experiments have been offered, many think that we still await a full answer to his challenge. In this paper I propose a likely candidate, the little-known associative remote viewing protocol (ARV), which embodies both cognitive information transfer and predictive elements. In the ARV protocol, each of two possible outcomes of a future binary event is associated with a standard-type remote viewing target (for example, an object, a geographical location, or a photo), thus yielding a target set of two orthogonal targets. This allows the remote viewer to use typical remote viewing procedures to predict the future event without having to rely on cognitive "guessing" strategies that generally lead to only chance results. In the ARV procedure, the viewer describes which target he or she will be shown at a future time after the event outcome has been decided. Before event culmination, a judge compares the remote viewing response to the two possible targets and decides which target best matches the response. The selected target is matched to the event outcome with which it is associated, thus indicating which of the event's outcomes

is predicted. In this paper I consider the requirements a paradigm experiment should be expected to satisfy. Among these requirements are (1) that it should require as little specialized equipment and training as possible; (2) that it should be relatively simple in concept, execution, and analysis; and (3) that it should produce reasonably reliable results on a consistent basis when executed correctly. I further consider how the ARV protocol meets these criteria. In the course of my discussion, I additionally explain how the ARV protocol is executed, and examine weaknesses and vulnerabilities in the process (for example, variations in perceptual skill between judges), while further entertaining considerations of its advantages and disadvantages. I point out factors that impact the quality of ARV results, and areas where it is not vulnerable to some of the conflating factors that affect other sorts of ESP research. My overall argument is supported by a survey of results from a collection of formal, informal, and pilot studies which show high statistical significance and demonstrate the value of the ARV protocol. Properly conducted ARV experiments have produced results ranging from 52% accuracy to 80%, when only 50% would be expected by chance. Two long-term experiments with many trials have, at a minimum, shown a consistent statistically significant hit rate. The first of these experiments produced odds against chance of one in 5,000; odds against chance for the second were one in 90,000.

Department of Philosophy
University of Texas, Austin, USA

CONTEXT, INDIVIDUAL DIFFERENCES, AND MEDIA TYPE IN THE EVALUATION OF PHOTOGRAPHIC ANOMALIES

ANNALISA M. VENTOLA[1] & DEVIN B. TERHUNE[2]

ABSTRACT: The causes and interpretations of putative photographic anomalies captured at reputedly haunted sites remain the subject of much debate. This study examined the roles played by photographic expertise, context, prior paranormal belief, tolerance of ambiguity, and media type in the identification of anomalies in photographs. Three groups of individuals (professional photographers, paranormal enthusiasts, and controls) were randomly assigned to one of two conditions in which they examined an online catalog of photographs consisting of prints from five different media types (black and white, color, infrared, Polaroid, and digital) taken from an alleged haunt site and a control site. In the informed condition, participants were told that the photographs were taken within the context of a parapsychological investigation of an allegedly "active" haunting and debriefed on the circumstances of the case prior to examining the catalog of prints. In the uninformed condition, participants received no such briefing. Participants rated the anomalousness of each print and completed measures of paranormal belief and tolerance of ambiguity.

The manipulation of context failed to influence anomaly ratings, though this may have been due to pre-experimental associations of "photographic

anomalies" and the paranormal rather than lacking the influence of the contextual informational prime. Anomaly ratings for prints taken at the target site were suggestively greater than ratings for prints taken at the control site, but only ratings for inactive areas of the target site (i.e., those areas in which no phenomena were reported: $M = 1.31$, $SD = 0.21$)—not active areas of the target site ($M = 1.26$, $SD = 0.12$)—were significantly greater than ratings at the control site ($M = 1.27$, $SD = 0.16$). Paranormal enthusiasts and controls exhibited comparable levels of paranormal belief, but controls assigned greater anomaly ratings to the print catalog than the other two groups (controls: $M = 1.40$, $SD = 0.38$; paranormal enthusiasts: $M = 1.24$, $SD = 0.25$; photographers: $M = 1.21$, $SD = 0.24$). Across all groups, anomaly ratings were most strongly predicted by endorsement of traditional paranormal beliefs. Although photographers were found to exhibit greater tolerance of ambiguity than controls and paranormal enthusiasts, this did not appear to influence their assessments of the prints. Years of photography experience correlated negatively with anomaly ratings, underscoring the importance of photographic expertise in the evaluation of putative anomalies. The research replicated previous findings that anomaly ratings covary with media type. Infrared prints yielded the highest anomaly ratings across groups and sites (infrared: $M = 1.56$, $SD = 0.10$; black and white: $M = 1.15$, $SD = 0.00$; color: $M = 1.26$, $SD = 0.04$; Polaroid: $M = 1.20$, $SD = 0.02$; digital: $M = 1.25$, $SD = 0.10$). Additionally, the type of anomaly identified by participants differed across media types. Density spots were found to be more common for infrared and digital prints, light streaks were more common for black and white and Polaroid prints, shadows were more common for color prints, and fogging was more common for infrared prints. The results are discussed within the context of the practical implications for the utilization of photographic equipment and expert consultants in haunting investigations.

[1]*Public Parapsychology*
Columbus, OH, USA
[2]*Department of Psychology*
Lund University, Sweden

THE PUBLIC TESTING OF AN ARTIFICIAL INTUITION DEVICE USING PICK 3 LOTTERY

MARK ZILBERMAN

ABSTRACT: This paper presents the results of public testing of the Artificial Intuition Device. Artificial Intuition is commonly viewed as a special algorithm or collection of algorithms capable of replicating some properties of human intuition. Within this approach, Artificial Intuition is a part of the Artificial Intelligence domain. Unfortunately, in spite of Artificial Intelligence research progress, the vast and intriguing area of human intuition (intuitive forecasting of future events, lottery predictions, stock market, etc.) cannot be replicated on software-simulated Artificial Intuition.

The Artificial Intuition Device (AID) employs a hardware solution to the problem of Artificial Intuition and replicates some predictive abilities of human intuition on specially designed scientific equipment. The results of AID tests performed in 2006 were presented at the 51st Convention of the Parapsychological Association in Winchester, UK, in August 2008. At that conference I also announced that AID testing was being made *public* because I programmed AID to predict results of daily Canadian Pick 3 lottery and to post the predictions on the Internet *3 hours prior* to the lottery draw (web page http://www.intuitiontester.com/summary.html).

The public availability of AID predictions makes experiment 100% clean and free from any possible manipulation. In addition, since Oct. 28, 2008 Goldsmiths College at the University of London has developed a daily download process from my website and has been downloading the AID's predictions onto their computer. This happens every day *after* predictions are posted on the Internet, but *before* the lottery draw happens. The public availability of AID predictions provides a great opportunity for everybody to check the ability of AID to predict the future and for me to prove that AID works even in such 100% controllable conditions.

Here is a summary of the results produced by the AID public testing prior to Mar. 25, 2009:

1. Artificial Intuition Device works and is profitable. Since the start of the public testing, AID has generated a profit of $1,440 with Return On Investment (ROI) = 42.9%. In the null hypothesis, when all AID's predicted numbers are random, the Return On Investment should be negative (-10%) and profit should also be negative and equal (-$336).

2. Significance of accumulated material during the public testing is $p < .036$.

3. The graph of profit accumulation is nonlinear. There are some periods when accumulated profit grows faster and other periods when it grows slower or even falls. This means that AID's performance is nonconstant.

4. The Return On Investment appears to grow when we consider groups with higher AID rank.

5. The Return On Investment is much higher on days with low Geomagnetic Activity. On geomagnetic quiet days (Ap < 5) ROI is 63%, as compared to ROI = 8.4% for the days with Ap > = 5.

6. AID's performance continued to be almost the same during the public testing as it was before in private lab conditions. What this means is that the presence of independent observers and downloading of predictions to independent computers did not influence the quality of AID predictions.

7. Many psychics state that they lose their abilities when they attempt to get profit from their predictions. Contrary to that, AID allows everybody to use its lottery predictions for profit and continues to work fine, predicts lottery outcomes, and generates a profit of 42.9% on all days and 63% on geomagnetic-quiet days.

8. All the observations above were confirmed in the independent material accumulated between the start of the experiment (May 23, 2006) and the start of public testing (before Aug. 13, 2008).

Between May 23, 2006 and Aug.12, 2008,

a. Estimated profit was $4,755 and Return On Investment (ROI) was 47.8%. In the null hypothesis, the ROI should be negative (-10%) and profit should also be negative and equal (-$994).

b. Significance of accumulated material is $p < .00037$.

c. The graph of profit accumulation is also nonlinear.

d. The Return On Investment also grows when we consider the groups with higher AID's rank.

e. On geomagnetic quiet days (Ap < 5) ROI is 100.9%, as compared to ROI = 20.7% for the days with Ap > = 5.

The results obtained in this stage of testing indicate that modeling artificial intuition on software and hardware is indeed possible. In addition to that, the testing results suggest that the AID can also be used to improve the quality of forecasting in other areas where people use intuition (planning, investments, stock market, etc).

Toronto, Ontario, Canada

PANEL: THE PHENOMENOLOGY OF THE OUT-OF-BODY EXPERIENCE

CHAIR: VERNON M. NEPPE

MODELS OF CAUSALITY FOR THE OUT-OF-BODY EXPERIENCE: THE MULTIETIOLOGICAL PHENOMENOLOGICAL APPROACH

VERNON M. NEPPE

ABSTRACT: Current out-of-body experience (OBE) models have attempted to explain OBE through utilizing a single major explanation or approach, though frequently recognizing the lack of generalizability for all OBEs. These approaches are here classified into four main groups of *unitary* hypotheses: psychological, brain, psychopathological, and experiential.

These several diverse models could imply different etiologies in different subpopulations. This logically leads to focusing particularly on a proposed new multietiological phenomenological approach that does not limit the model to any single etiology. By so doing, this differentiates OBE phenomenologically into the many unitary approaches.

A: The psychological models:

The most well-known psychological models include:

1. *Blackmore's reality distortion*—OBEs involve attempts to regain control of one's external realities and a subjective OBE may be a misperception that never occurred.
2. *Palmer's body concept model*—OBEs involve changed proprioceptive feedback; they threaten the self-concept and activate

unconscious processes by trying to reestablish the sense of identity. This uses both motivation and psychodynamic models.

3. *Irwin's psychological absorption model,* later *somatic dissociation model*—reflecting pathological dissociation or a nonpathological absorption with fantasy proneness, implying correlations of OBEs with a certain related trait or personality phenomena and somatic features.
4. *Murray's dissociation model*—OBEs differ along several dimensions, for example, somatoform dissociation, self-consciousness and body dissatisfaction.

B. The brain model empirical approaches:

1. *The pathology model of OBEs deriving from brain stimulation.* Rare empirically induced "OBE" descriptions on single epileptic subjects undergoing intracranial brain stimulation presurgery have produced nonidentical loci, for example, *Penfield* (temporal cortex), *Blanke* (right angular gyrus) and *De Ridder* (parieto-temporal area). Occurrence across anatomical loci and absence of state-specific OBEs are problematic in these tiny samples. Phenomenologically, naming them OBEs is disputable. These induced Subjective-OBEs (S-OBEs) variably produced distorted body image, depersonalization and derealization, visual perceptions of specific unchangeable loci, and associated other parieto-temporal state or trait features. These descriptions differ markedly from thousands of spontaneously reported S-OBEs in ostensibly "normal" individuals, as these frequently involve subjectively extracorporeal consciousness with locality dependent perceptual experiences, clear imagery, polymodal perceptions and profound cognitive awareness. These dichotomous epiphenomena of subjectively interpreted "out-of-body experiences" require careful phenomenological differentiation—the induced S-OBE apparently greatly differs from the spontaneous S-OBE. Using one term for both endpoint expressions could produce incorrect clustering of entirely different phenomena with different origins and etiologies, inappropriately interpreted as of common basis.
2. *The "psychocerebral" models* refer to explanations involving specific anatomical or physiological brain models. Three examples are:
   a. *Persinger's vectorial hemsiphericity*
   b. *Wettach's model* correlating near-death experiences with midbrain involvement, and
   c. *Nelson's physiological REM intrusion model:* Are NDEs relevant? A controversy.
   d. *Neppe's temporal lobe model* as the integrator of polymodal perceptual experience.

C. The psychopathological psychiatric perspective:

1. *Neppe's portrayal of psychiatric interpretations:* extreme ego splitting, with marked derealization and depersonalization, and delusional out-of touchness with reality.

D. The experiential descriptive scientific subjective paranormal experient approach is epitomized by:

1. *Whiteman's levels of separative experience,* based on 10,000 documented OBEs.
2. *Alvarado's correlation of OBEs with psi experiences* (not a model but emphasizing the psi base).

The presenter's proposed multietiological phenomenological model of OBEs accommodates the multiplicity of causes and different subpopulations. It motivates detailed multiquestion OBE screening. Like must be classified as like. Discrete population sample analysis of form, content, circumstance, and predisposed populations is an empirically viable method in many other related areas such as déjà vu, olfactory hallucinations, and temporal lobe symptomatology. Analyses by multidimensional scaling or correspondence analysis may not be attainable by a single screening question on OBEs.

Not all epiphenomena have common origins. Multifactorial etiologies and epiphenomena expressed could produce, for example, four nosological subtypes based on the four unitary perspectives above, namely:

A. subjective paranormal nonexperients reporting psychological experiences; the general population may deny spontaneous OBEs and have different perceptual input.

B. epileptics or those with brain pathology who may experience distinct distorted cerebral-linked OBEs.

C. the psychiatric population whose experiences may differ in content and process.

D. subjective paranormal experients reporting qualitatively distinct subjective paranormal experiences (SPEs) including S-OBEs.

The possible phenomenological distinctiveness of these populations should be studied and can be subjected to appropriate correspondence analysis, multidimensional scaling, or statistical review.

Pacific Neuropsychiatric Institute
Seattle, WA, USA, &
Department of Psychiatry, St. Louis University
St. Louis, MO, USA

A COMPARISON OF RECENT RESEARCH IN BRAIN STIMULATION AND VIRTUAL REALITY WITH PSYCHOLOGICAL AND PARAPSYCHOLOGICAL RESEARCH ON OUT-OF-BODY EXPERIENCES

ARTHUR HASTINGS

ABSTRACT: Recent research in brain stimulation and virtual reality (VR) has claimed to create elements of an out-of-body experience (OBE). Ehrsson et al.

induced a VR arrangement using goggles. When the experimenter touched the participant's chest with a rod, he simultaneously held another rod so it would appear in the goggles as if it were at the front of the illusory body. The participants felt the prodding as if the sensation was in the illusory body.

A study by Lenggenhager et al. also filmed participants from the back, and displayed the virtual figure in goggles as if it was 2 m in front of the participant. Experimental subjects were stroked similarly to the previous study. The participants were then blindfolded and moved backward. Asked to move back to where they were, the experimental participants moved closer to where the illusory body appeared to have been, compared with control participants. The research conclusion was that the sense of the self had moved outside of the body boundaries.

These studies were published in *Science* and an editorial heralded them as "Out-of-Body Experiences Enter the Laboratory." A third study by DeRidder et al. stimulated the posterior part of the superior temporal gyrus on the right side of the brain, with the patient reporting a state of disembodiment, and feeling that he was 50 cm behind the body and to the left. PET scans showed activity in the right angular, precuneus, posterior thalamus, and superior vermis. The paper was titled "Visualizing Out-of-Body Experience in the Brain."

This paper will compare the results of the above (and other such research) with reports from psychological and parapsychological studies, using phenomenological thematic analysis of the experiences. Without disregarding neuroanatomical correlations and dynamics of the experienced self, and acknowledging the intrinsic interest of the above results, the reported phenomenology of the "traditional" OBE as an exceptional human experience shows a coherent whole that is not accounted for by the research.

A preliminary phenomenological analysis shows several differences between the VR and brain stimulation results and studies of subjective elements of OBEs by Gabbard and Twemlow, and others. Elements of OBEs that are not found in the experimental research above include a visuospatial perspective from the apparent externalized location of the self, lack of awareness of the physical body, and voluntarily being able to move the externalized point of consciousness and perspective. There is also pilot research (e.g., by Krippner and Tart) that otherwise nonevident visual information can be obtained as if from the externalized point of self. An element that is found in the VR research, but not commonly in spontaneous OBEs, is that the person experiences physical sensations in the virtual body. The differences in subjective awareness of these elements suggest a need for the integration of qualitative phenomenological data in the studies of OBEs. Suggestions for the integration of qualitative method research on OBEs will be presented with the paper.

Institute of Transpersonal Psychology
Palo Alto, CA, USA

WHERE AM I? (WHO AM I?) THEORIES OF OUT-OF-THE-BODY AND IN-THE-BODY EXPERIENCES

CHARLES T. TART

ABSTRACT: Theories of what I have called "classical" out-of-the-body experiences (OBEs) (experiencing oneself as elsewhere than where one's physical body is, while concurrently having a clear, lucid state of mind) tend to take our ordinary experience of being located in our bodies for granted and then postulate either that OBEs are a hallucinatory artifact of brain functioning or that a "nonmaterial" something temporarily separates from the physical body. By considering the rapid and skillful ways mind adapts to modern computer-generated virtual reality (CGVR) environments or telepresence environments, however, the way we could say mind readily "incarnates," we see there may be a variety of types of both OBEs and in-the-body experiences (IBEs), mixtures of the degree to which some aspect of mind may really be "out" of the body interacting with the processes that create the feeling of being located somewhere.

The implicit or explicit feeling of being located somewhere means you have a consistent and practically useful schema for organizing incoming information and acting on it in consistent and useful ways. Mind and/or brain creates a virtual reality (VR), a world-simulation process (WSP), that represents what the world we are currently in is like, where we are in it, predicts likely consequences of our intentional motor actions, and gives feedback on the results of such actions. The purest form of the VR-WSP is a nocturnal dream: a world, a self located in that world, said self acting and being acted upon, and experienced consequences of such actions. Modern brain research suggests that essentially the same VR-WSP process is operating in our waking state, but now the experienced world the VR-WSP creates must account for massive sensory inputs from our exteroceptors and interoceptors, so the experienced virtual reality, the simulated world, matches the external one to a high degree. If the match is poor ("There's no cliff ahead of me in this fog.") you may die. Altered states of consciousness (ASCs) and OBEs are major pattern alterations in the VR-WSP. A classical OBE involves an experience of consciousness functioning pretty much as it normally does (seemingly not an ASC) except for lack of connection to one's physical body, but mental functioning feels as clear and lucid as ordinary consciousness or even clearer.

In the latter case, experients are tempted to think this is their true mind or self, freed from ordinary physical limitations, but since some OBEs seem to involve both correct psi perception of distant locations and errors about these locations, it is more likely that consciousness is still a VR-WSP, not a simple, straightforward perception of reality. With a high degree of distortion/alteration of the WSP construct from the actual reality where consciousness is now located, we might have a genuine OBE (in the sense of something potentially detectable by others or instruments at that distant location) with quite inaccurate perception of the distant location.

Theories of OBEs, then, must take account that our ordinary IBE is not as simple as it seems and the VR-WSP may function in OBEs that are just as complex as in IBEs.

Institute of Transpersonal Psychology
Palo Alto, CA, USA, &
University of California, Davis, USA

BEYOND THE BODY BUT NOT OVER THE LINE: EXPERIENTIAL DIFFERENCES BETWEEN THE NEAR-DEATH EXPERIENCE AND OUT-OF-BODY EXPERIENCE

KIMBERLY CLARK SHARP

ABSTRACT: Research has given scant interest to the differences between a near-death experience (NDE) and an out-of-body experience (OBE). What little exists on the subject in scientific publications is written entirely by authors without personal perspective in either phenomenon.

The exact definition of what constitutes an NDE is disputed. However, closeness to death is certainly a sufficient though arguably not necessary requirement. The presenter will differentiate how NDEs should be distinguished from what could be called "near-death-like experiences" occurring in non-near-death circumstances.

There is a literature on this, including that of Greyson, Ring, Alvarado and Gabbard. Based on the literature, the presenter is using the following operational criteria: NDEs are mainly subjective events that occur when our physical bodies are at or inexorably approaching clinical death, including coma. These events can include peace and a sense of well-being; the ability to hear but not communicate with one's surroundings; the sounds and sights of unearthly environments; finding oneself in a void or traveling through a tunnel; meeting others, including deceased loved ones and spiritual beings; encountering a brilliant light; a life review or preview; and, occasionally, frightening situations. Additionally, the International Association for Near-Death Studies (IANDS) describes four phases of an NDE: disassociation from the body, perception of the natural world, perception of a supernatural world, and return to the body.

Of course, every NDE is an OBE, which, simply put, is the ability to comprehend one's surroundings without benefit of our body's physical operating system. But it is the presenter's contention that differences abound between the NDE and the OBE.

This presentation compares the NDE to OBEs from the vantage point of personal experience, laced with the author's several decades' worth of clinical interviews from both adult and child populations.

Bases for comparison between NDEs and OBEs include:

- proximity to physical death
- sense of threat
- spontaneity

- perception of time
- perception of space
- sense of connection to the physical body
- architectural elements
- visual distractions
- integration of the experience
- lifestyle changes
- fear of death
- need for validation and inclusion

The presenter's personal experiences include her NDE during citizen CPR when the resuscitation was observed; multiple spontaneous OBEs since her NDE, including a veridical OBE while on EEG in an inpatient hospital setting; the validation of a tennis shoe on a remote hospital ledge observed by a patient in cardiac arrest; and anecdotal reports from people who have had NDEs and nonlife threatening OBEs.

Seattle International Association for Near-Death Studies
Seattle, WA, USA, &
University of Washington, Seattle, USA (Ret.)

PANEL: PSI, THE SOUL, AND PRESENTLY KNOWN LAWS OF PHYSICS

CHAIR: CHARLES T. TART

PSI AND PHYSICS: RECONCILIATION IN AN EXPANDED PHYSICS OR NEW PARADIGM?

CHARLES T. TART

ABSTRACT: Psychical research and parapsychology got their historical impetus from an interest in religion, in the widespread beliefs that there was some sort of spiritual world that our souls were destined (hopefully) to go to after death and which had real effects on psychological and physical events in the ordinary material world of the living. These ideas were rejected wholesale with the rise of science and its concurrent fight to liberate itself from the historically authoritarian control of religion. The founders of the SPR came up with the brilliant idea that instead of believing religion and ignoring science wholesale, or vice versa, why not apply the *method* of science (rather than being stuck in the current *corpus*, the accepted findings) to the phenomena of religion and see what had evidential backing and what didn't? As a result we now have, as I summarize in my recent book *The End of Materialism: How Evidence of the Paranormal Is Bringing Science and Spirit Together*, excellent evidence for five major phenomena—telepathy, clairvoyance, precognition, PK, and psychic healing—as well as highly suggestive evidence for more exotic phenomena like OBEs, postmortem survival, and reincarnation. I

argue that the evidence for these phenomena was obtained using the methods of essential science, but they do not fit in with the corpus of classical, materialistic Newtonian physics. Can they fit with modem quantum physics? Or what kind(s) of extension or paradigm shift in physics would be needed to accommodate the actual data?

I further ask the following questions: (1) To explain psi, does physics need to be extended beyond presently known laws? What is a "law," anyway? (2) Is there any way to empirically test any of the physics theories of psi? Could you predict, for example, that a good physics theory of psi should allow machines to be built which will produce or receive psi? (3) Have any physics theories of psi been of any practical use so far? Besides giving an experimenter general confidence through being a member of the esteemed community of physics, is this more than a psychological, experimenter effect? (4) Wouldn't any explanation of an immaterial soul necessitate a major addition to physics? What does an "explanation" mean in this sense?

Institute of Transpersonal Psychology
Palo Alto, CA, USA, &
University of California, Davis, USA

EXPLANATIONS OF PSI, SPECULATIONS ON SOUL

RICHARD SHOUP

ABSTRACT: The most critical concepts relating psi phenomena and fundamental physics are *causality* and *randomness*. I will argue that current quantum physical theory is sufficient to explain most if not all psi phenomena described by the (classic but outdated) terms telepathy, clairvoyance, precognition, and psychokinesis IF one crucial assumption in the theory is sensibly challenged and modified—that of the fundamentally random "collapse" of the wavefunction. Adopting instead the *decoherence* view of quantum mechanics allows complete time symmetry, and thus correlations due to past or *future* interactions. A simple canonical thought experiment (forced-choice, subject guess compared against randomly generated target) is described and analyzed in terms of possible paths that enable correlation or information flow. It is then shown how psi phenomena can and will manifest under this slightly revised quantum theory. Several new and modified actual experiments, both microscopic and macroscopic, are proposed to test this theory and the idea that the "big 4" above are all due to the same physical mechanism. I also discuss, but can only speculate about, some more-challenging phenomena such as hauntings, mediumistic communications, and apparent reincarnation. It is suggested that these will eventually be better explained by physical means than by invoking an immaterial "soul."

Boundary Institute
San Jose, CA, USA

PSI AND EXPANDED PHYSICS, SOUL AND NEW PARADIGM

JEAN BURNS

ABSTRACT: There does not appear to be any explanation for either ESP or PK within presently known physical laws. For instance, many examples of ESP are known in which an operator has obtained information from thousands of miles away. Because of such examples, information transfer in ESP is commonly considered to be independent of distance. Quantum nonlocality has the property that it lacks distance dependence, so it might be thought that it could provide an explanation for this aspect of ESP. However, it is well established in quantum theory that information cannot be transferred via nonlocality. (This result is independent of which interpretation of quantum theory is used.) Therefore, nonlocality, as described in presently known physical laws, cannot provide an explanation for information transfer in ESP.

Consciousness appears to be involved in psi, for instance, through the holding of ESP or PK targets in conscious intention, and this suggests that psi occurs as part of a general interaction of matter and consciousness. In that case one can reasonably expect that an explanation of psi would be incorporated into an extension to known physics that would describe the effects of consciousness on matter. An extension of some sort would be needed, because presently known physics was developed to describe the action of matter only and was never intended to incorporate any extra effects due to consciousness. However, such an extension might be fairly simple. For instance, in the case of ESP it is possible that conscious intention allows information to be transferred through quantum nonlocality, even though it cannot be transferred without such intention. If so, this principle could be viewed as part of such an extension.

Similar considerations apply to PK. It does not appear to be explained by presently known laws of physics because the latter allow only randomness or the determinism of known laws, and PK is usually conceived to be something different from either of these. However, a possible explanation for PK is that consciousness can produce the ordering of randomness in a way that correlates to conscious intention. The latter principle could similarly be viewed as part of an extension to known physical laws that takes the capacities of consciousness into account.

The explanation of ESP and PK could conceivably consist of only a few simple principles, such as the above. However, large-scale psi effects such as poltergeist or seance-room phenomena suggest that more additions to physics than just a few simple statements will be needed. Once the relationship of psi to physics is better understood, it is likely that a whole new realm of physics will open up that describes the interaction of consciousness with matter.

Consciousness Research
San Leandro, CA, USA

PANEL: SEPARATING THE SHEEP FROM THE GOATS: A TRIBUTE TO THE MEMORY OF DR. GERTRUDE R. SCHMEIDLER

CHAIR: JOHN PALMER

GERTRUDE SCHMEIDLER: PSYCHOLOGIST, PARAPSYCHOLOGIST AND MENTOR

RUTH REINSEL

ABSTRACT: This presentation will review the highlights of Dr. Gertrude Raffel Schmeidler's career, starting with her introduction to parapsychology by Gardner Murphy in 1942. Murphy, a past president of the American Psychological Association, offered her a job in the newly founded Psychology Department at the City College of New York, where he was the first chairman. Schmeidler remained at CCNY for the rest of her career, appointed later as a member of the doctoral faculty of the Social and Personality Psychology program of the City University of New York. She taught the required graduate course in Experimental Psychology for many years, as well as graduate courses in personality and perception, and parapsychology and altered states of consciousness. She retired in 1982 but continued to teach and publish as Professor Emerita for several years.

Gertrude, as she was known to all, mentored dozens of students for their M.A. and Ph.D. degrees in psychology. During her active career between 1939 and 1997, she published over a hundred papers in psychological and parapsychological journals. An analysis of these papers, many coauthored with her students, shows the scope and diversity of her contribution to both fields. Her careful experimental work provided some of the early evidence for the role of personality, attitudes, and social factors in ESP. Prominent in her later work is the groundbreaking paper on PK-induced temperature changes with psychic Ingo Swann, and her comprehensive reviews of research in psychokinesis. She developed objective methods for working with psychics and mediums, and for investigating hauntings. She published four books on parapsychology, with arguably her most important work being the careful review of correspondences between psi and normal perceptual processes contained in *Parapsychology and Psychology: Matches and Mismatches* (McFarland, 1988).

Schmeidler played a leadership role in parapsychology; present at the founding of the Parapsychological Association in 1957, she served as its first Vice President and twice as President (1959 and 1971), and she gave four years as President of the American Society for Psychical Research (1981–1985). She was unfailing in her encouragement of young investigators entering the field. She established the Award for Outstanding Student Contribution to Parapsychology, which the PA named after her. The PA honored her in 1988 with the Outstanding Career Award.

Dr. Schmeidler's legacy is her research work. Just how much she contributed to our field can only be appreciated by a thorough review of her many

published papers. Her thoughtful writings and carefully controlled experiments provide many of the building blocks on which the scientific integrity of our field still rests.

The NeuroPsience Laboratory
New York, NY, USA

GERTRUDE SCHMEIDLER AND THE SHEEP-GOAT EFFECT

JOHN PALMER

ABSTRACT: Although Gertrude Schmeidler's research covered a vast array of topics related to the psychology of psi, she undoubtedly will be best remembered for her pioneering research on the relationship between ESP test scores and belief in psi, which she euphemistically labeled the "sheep-goat" effect (SGE). This was the first psi research she conducted in her illustrious career. Her initial forays were conducted to satisfy her curiosity about the reality of ESP. The participants in this informal 1942 card-guessing experiment were colleagues in the Harvard psychology department and students in psychology classes at Harvard and Radcliffe. She observed that her participants who had a favorable attitude toward ESP and the experiment tended to score above chance while those with an unfavorable attitude toward ESP and the experiment tended to score below chance. She proceeded with more formal tests, first with participants tested individually ($N = 151$) and later with students from her own psychology classes over 14 semesters from 1946 to 1951 ($N = 1157$). In both sets of studies, the sheep scored significantly above chance, and the goats significantly below chance, although not to as great a degree as the sheep.

A couple points about this research are often misunderstood. First, sheep and goats were not distinguished by whether they believed in ESP but instead by whether they thought ESP was possible under the conditions of the experiment. Note that it is not whether the participants themselves would score well (those who answered this question yes were later dubbed "super-sheep") but whether the test was capable of evoking ESP in someone. Also, only firm skeptics were classified as goats; even those who doubted the existence of ESP were labeled sheep.

Schmeidler was not satisfied just demonstrating the SGE. Very much the psychologist, she wanted to know *why* sheep scored better than goats. She never accepted the simplistic view that belief was the crucial variable; it was a marker of something more fundamental. To get a handle on this something, she first took note of how individual participants reacted to the test procedure. These anecdotal data were not evidence, but they served as a rich source of hypotheses. Then she did something far too uncommon in personality-ESP research: she looked for interactions between belief and other psychological variables that she measured as predictors of ESP scores in her large classroom experiment. She found that the SGE was attributable to participants who (a) showed good social adjustment

and liveliness or over-responsiveness (as opposed to constraint or over-control) as measured by the Rorschach, (b) a theoretical value system according to the Allport-Vernon Study of Values and (c) were female.

In the final analysis, Schmeidler concluded that the SGE really reflected how participants reacted to the situation and to the person administering the test. This is very close to my own interpretation of the SGE, arrived at independently, as reflecting comfort in the test situation. I find this sophisticated quest for a deeper understanding of the psychology of psi to be her most admirable attribute as a psychologist and parapsychologist.

Rhine Research Center
Durham, NC, USA

GERTRUDE SCHMEIDLER: PSYCHOLOGY AND PARAPSYCHOLOGY

JAMES CARPENTER

ABSTRACT: For over 50 years Gertrude Schmeidler presided over her chosen field: the interface between general psychology and the renegade findings of parapsychology. Perhaps we should say that she was a psychologist who very intently studied parapsychology. She began with a good background in the rich mixture of humanistic approaches that characterized general psychology in New York in the 1940s. She knew gestalt psychology, and psychodynamic theory, and the New Look in perception, and Murphy's psychosocial field theory. She took the challenging findings of J. B. Rhine, as aridly conceived as any dustbowl S-R experiment out of the Midwest, and wrapped them in meaning. If ESP was real, it was something people *did*, not merely a mysterious thing that happened to them. It would have to be effected by, and be expressive of, their attitudes and beliefs and values and relationships. It would have to serve their situations and meet their needs. And it should be of a piece with all of their psychological functioning. She innovated several important lines of research, added to and enriched others, and gathered them all together and interpreted them together in light of what is known about human cognitive, affective, and social functioning. This review focuses particularly on her work on cognitive, affective, and interpersonal factors that influence the expression of ESP and PK, and summarizes some of her own conclusions about the state of the field and its relation to general psychology.

Rhine Research Center
Durham, NC, USA

CLEVER BEASTS AND FAITHFUL PETS: A CRITICAL REVIEW OF ANIMAL PSI RESEARCH[1]

By Diane Dutton and Carl Williams

ABSTRACT: While sharing key theoretical and methodological assumptions with human psi research, animal psi research poses its own unique conceptual and empirical challenges. We explore two main strands of animal psi research: (1) experimental research, which has tended to explain animal psi as an evolutionarily adaptive process of information transmission, and (2) fieldwork, which has emphasized the importance of the human-animal relationship for the expression of psi. Although there is some support for both explanatory frameworks in animal psi research, we argue that the evidence suggests that animal psi may function as an expression of relationship or "resonance" between individuals. We suggest that it may be more profitable to search for animal psi in the context of close human-animal relationships, and that parapsychology needs to seek methodologies that engender the expression of psi in relational contexts.

Keywords: animal psi, anpsi, human-animal relationship

Emerging Explanatory Frameworks in Animal Psi Research

It is fair to say that animal psi research is a relatively neglected area of investigation in present-day parapsychology. Theoretical debates about the nature of psi rarely make reference to findings from animal work, and conceptual and practical issues ensure that, with one or two notable exceptions, most researchers do not involve animals in their research programs. Yet an examination of the origins, underlying assumptions, and findings of animal psi research illuminates a number of conceptual and empirical debates that are pertinent to parapsychological research in general.

With its heyday in the experimental parapsychology of the 1960s and 1970s, animal psi research arose out of attempts to situate psi within a broader biological and evolutionary framework. In outlining the main theoretical aims and methodological issues involved in this emerging sub-discipline, Rhine (1950, 1951) suggested that the study of psi in animals may facilitate an understanding of the evolutionary foundations of psi, a position that has been echoed by later researchers (e.g., Taylor, 2003).

Early interest in animal psi arose out of attempts to explain unusual sensory abilities such as homing or "psi-trailing."[2] Although later research

[1] We would like to thank Fundação Bial for support during the preparation of this paper.

[2] Defined as "…those cases in which an animal, separated from a person or mate to whom it has become attached, follows the departed companion into wholly unfamiliar territory and does so at a time and under conditions that would allow the use of no conceivable sensory trail." (Rhine & Feather, 1962, p. 3)

programs began to identify non-psi sensory processes that might explain homing, the large numbers of anecdotal reports of psi-trailing behavior were less easily explained, and served to help shape early ideas about the specific form and function of animal psi abilities. Rhine had suggested that psi appeared relatively early in evolutionary history and in animals could best be considered as a "presensory mode of orientation" (1951, p. 244). Hence psi could be considered to be a largely unconscious cross-species ability. However, anecdotal reports of extraordinary animal sensory abilities suggested that animal and human psi may differ in some basic respects. Rhine and Feather (1962), for instance, suggested that abilities such as psi-trailing require such profound sensitivity that animal psi may be stronger, more consistent, and perhaps even *qualitatively* different from human psi.

Debate about the precise nature of animal psi has also shaped expectations about which species may be more likely to display various psi abilities and which contexts may best elicit animal psi. Some early writers were sufficiently impressed by the complex coordination of particular social species to posit the existence of animal "supersensory" processes, such as a "group mind" (Hardy, 1965), "group soul" (Selous, 1931), or a "spirit of the hive" (Maeterlinck, 1901). But it is in the context of the human-animal relationship that much of the early interest in potential psi abilities arose. Many lay accounts of unusual animal abilities focused on the potential for psi communication between animals and their owners or caretakers as an expression of the strong bonds that often form between humans and animals. These relationships tend to involve social species with highly developed communicative abilities and a high level of responsiveness to humans (such as dogs, cats, birds, and horses). Growing out of anecdotal accounts of the unusual abilities of companion animals, the field experiments of Sheldrake (e.g., Sheldrake & Smart, 1998, 2000a, 2000b) are pertinent examples of this type of research.

A contrasting strand of animal psi research has restricted work to those species that have traditionally informed much experimental psychological research, such as mice or rats. These animals are arguably less complex and more easily able to be manipulated in an experimental laboratory setting. In such settings, any bonds that form between animal and experimenter tend to be considered as detrimental to the investigation, and the focus is upon isolating the precise form of psi that may be operating (e.g., telepathy or clairvoyance). A widespread assumption within early animal psi research was that, whereas animal psi abilities were more evident in spontaneous settings, more rigorous investigation demanded the controlled environment of the laboratory (Morris, 1970; Davis, 1979). In this respect, the context within which animal psi research developed somewhat inevitably reflected the broad conceptual and methodological assumptions of the human psi research program. In an early review of animal psi research, Morris (1977) argued for the integration of parapsychology and biology, and a common language for both that could express the developing models

of communication and information transfer that characterized both fields. In doing so, he was expressing the predominant explanatory framework that has shaped much parapsychological research to date: namely, that psi may best be understood as the anomalous transfer of *information*, the process of which can best be studied in a carefully controlled experimental setting.

Animal psi research has therefore been shaped by many of the same assumptions as human psi research, and has struggled to overcome similar methodological challenges, such as individual differences in the ability of participants, decline effects, and the difficulty of disentangling experimenter and participant psi. In other respects animal psi research has posed its own unique problems and opportunities. The distinction between ordinary and nonordinary sensory abilities is arguably more difficult when we are working with nonhuman species, with different physical, cognitive, and social abilities. Issues to do with the motivation, mood, or levels of stress of participants are similarly more thorny when those participants are not able to communicate their thoughts and feelings to an investigator. And the assumption that animal psi may best be elicited in simple species in nonsocial settings proves to be problematic when the dynamics of animal psi studies are considered more closely.

We have chosen a broad distinction in the following appraisal of animal psi research. We examine firstly those studies which constitute the main body of formal animal psi research and which focus largely on experimental evidence. We then turn to consider research which has focused more closely on the expression of psi within the context of close human-animal relationships and which tends to be characterized mostly by anecdotal reports and field studies. We subsequently argue that the contrasting demands and opportunities of these different research contexts shape an assessment of the conceptual and evidential value of animal psi research.

The Experimental Animal: Animal Psi in the Laboratory

Much experimental animal psi research has been conducted within an operant or classical conditioning paradigm. Within this context, animals can be trained to respond to a particular stimulus (e.g., light, food, electric shock) and a baseline response obtained. Such conditions tend to be easily manipulated to measure any deviations from baseline behaviors that might be due to psi. The use of this type of paradigm for testing psi in animals is based on the assumption that psi in such contexts would act to *increase* favorable conditions for the animal.

The two earliest formal experimental animal psi studies focused on cats, and they will be explored in a little detail as they served to shape the design of later animal psi research by highlighting some key methodological challenges (Davis, 1979). In a study that explored the ability of a human agent

to influence a cat, Osis (1952) used six domestic kittens in a forced-choice paradigm. Kittens were tested on their ability to detect which of two metal cups containing food was the target cup chosen by a human agent. Efforts were made to minimize olfactory and other sensory cues. Variations in the general procedure included comparing trials in which the agent identified the target cup (by choosing a card from a randomly ordered pack) and also carried out all other experimental tasks (i.e., arranging the food cups, handling the kittens, and recording the results) with trials in which the agent was isolated in a cubicle and a separate experimenter carried out the other tasks. Kittens were also tested in the presence of negative stimuli, such as electric shock. Initial exploratory trials indicated above-chance scoring, but this seemed to be restricted to only some kittens. A second series of trials using an independent experimenter to record the results indicated little positive deviation from chance scoring but significant psi-missing in a condition with negative stimuli. Osis (1952) highlights the presence of decline effects for most kittens across trials, as well as the tendency for some animals to form "side-habits" in developing a tendency to consistently choose a particular cup (e.g., the one on the right-hand side). There were clear individual differences between animals in deviation from chance scoring, and these differences seemed to be mediated by the relationship between experimenter and animal. For instance, in the exploratory trials, the best-scoring kitten had formed a very strong attachment to the author (who acted as one of the experimenters) and his family, suggesting a possible interaction between human and animal psi.

In a follow-up study, Osis and Foster (1953) attempted to control for possible effects of experimenter psi, and also to further explore whether differences in handling of animals affected scoring. In this second study, a clairvoyance condition was obtained by placing food in only one cup; the kitten's task was to use psi to detect the correct cup, without the influence of an experimenter. Kittens which were handled affectionately scored significantly above chance, whereas kittens which were subject to unpleasant handling (e.g., rubbing their fur the wrong way or withholding food) scored significantly below chance. These early findings suggest that although there seems to be some evidence for the operation of psi in the animals' performance, the expression of this may be influenced by aspects of social interaction between human and animal. There is good evidence from social psychological research that positive expectation by experimenters can by itself affect behavioral responses in animals. Early work by Rosenthal and his colleagues showed that the performance of rats in an operant conditioning task was greater when experimenters were led to believe that these "maze-bright" animals had been bred to perform well in such tasks, compared to rats who had been categorised as "maze-dull" (Rosenthal & Fode, 1963). It seems likely that a key contributor to this effect was the tendency for experimenters to handle the "maze-bright" rats more gently and to attribute more positive qualities to them (such as tameness),

suggesting that it may be difficult to isolate effects on performance due to experimenter psi from general handling effects (see Grim, 1984 and Rosenthal, 1984 for a discussion of their possible equivalence). Of course, the social facilitation of success that a positive research atmosphere can produce is by now a well-known feature of research on human psi, utilizing settings such as the ganzfeld (Dalton, 1997; Sherwood, Roe, Holt, & Wilson, 2005).

It was not until the 1960s and 1970s that further substantial experimental work with animal psi was attempted. One strand of research in this period tested for precognition in situations where the animal was likely to anticipate future emotionally charged events. Morris (1967) showed that goldfish which were randomly chosen to be netted and briefly lifted out of their tank exhibited significantly more activity than fish which were not to be removed. However a later study, in which a person other than the experimenter was responsible for the random selection of the fish, found no effect, suggesting the possible presence of experimenter PK (Morris, 1977). A small number of studies explored precognition in rodents in situations where the animals faced possible death. Morris (1967) found that rats which were to be terminated showed more tendency to freeze in an open-field test (though this was only the case if they were familiar with the open field). However, Schmidt (1970; cited in Morris, 1977), using a similar procedure, failed to replicate this effect, and Craig (1974, 1975) and Craig and Treurniet (1974) found that rats which were to be terminated the next day were more, not less, active. Morris (1977) suggested that this discrepancy may have been due to differing time intervals between testing and death in Craig's study, but Davis (1979) concludes that the difference is probably due to experimenter psi. For obvious ethical reasons, later research moved away from this type of testing situation, but these studies serve to demonstrate that situations with strong emotional involvement may be more likely to elicit *both* experimenter and animal psi.

Animal psi research after the mid 1960s was characterized by a greater attention to developing more rigorous experimental procedures using automated testing systems. In an automated shock-avoidance paradigm, Duval and Montredon (1968a, 1968b) tested whether mice could anticipate which side of a light- and sound-shielded cage was to be shocked, by moving to the "safe" side of the cage before the shock was administered. A random number generator determined which side of the cage was shocked. The analysis was complicated by the formation of stereotyped behaviors in the mice, such as routinely jumping to the opposite side of the cage after being shocked, or freezing due to fear. But when the stereotypical responses were removed from the analysis, significant results were obtained. These studies embodied a more rigorous experimental design and so could be considered to represent reasonable evidence for psi in animals (Morris, 1970; Davis, 1979) although later studies using a similar design were not generally indicative of a psi effect (e.g., Levin, 1975, cited in Morris, 1970; Terry, 1976).

However, these later studies were not direct replications of the Duval and Montredon work; Levin's study used gerbils rather than mice, and as Davis (1979) notes, the experimental apparatus used in Levin (1975) and Terry (1976) differed in some important respects from the Duval and Montredon apparatus. The large number of precognition experiments performed by Levy and his colleagues in the 1970s should also be briefly mentioned here, as some of these attempted to replicate the Duval and Montredon findings. Unfortunately, this body of work cannot contribute to an evaluation of this testing paradigm, due to reported fraud (see Rhine, 1974).

One clear problem with the Duval and Montredon experimental design was that the negative effects of the electric shock introduced nonrandom behavior, as the mice attempted to avoid the shock. Schouten (1972) attempted to improve this design by substituting a positive reinforcer for electric shock to test whether mice could determine by psi which of two randomly selected levers would produce water. Both a clairvoyance and a telepathy condition were run; in both conditions the experimenter was blind to which lever was the target. In the telepathy condition, pairs of cage-mates were separated, such that while one mouse was choosing a lever, its mate was aware of which lever led to water. The results were marginally significant in favor of psi, though only when nonhabitual responses were evaluated. There appeared to be no significant difference between the telepathy and clairvoyance conditions, although some pairs showed behavior suggestive of telepathy. Schouten (1972) interprets his results as representing confirmation of Duval and Montredon's (1968a, 1968b) findings, though a later study failed to replicate these results (Schouten, 1973, cited in Morris, 1977). It seems that the tendency for psi to occur mainly in nonrandom (and so more labile) responses is an important characteristic of animal psi.

Using a similar design to Schouten's, Terry and Harris (1975) obtained significant findings in a precognition study with rats, but only when stereotypical behaviors were excluded from the analysis. However, in a clairvoyance test using gerbils, Parker (1974) found that both random and nonrandom behavior trials achieved significance, although the scoring rate was slightly higher for the random trials. There were clear individual differences between gerbils, with some performing better on random behavior trials and others performing better on nonrandom behavior trials. The question of individual differences in psi ability was explored directly in a precognition study using rats (Eysenck, 1975). Using a similar apparatus to Duval and Montredon (1968a, 1968b), rats were tested for their ability to predict which side of a box would be electrified. Although electric shock seemed to inhibit psi in some rats, others responded by psi-missing. Eysenck (1975) suggested that the difference was due to the strain of rats used, with some strains being low in arousal and thus likely to seek out stimulation, even if it was aversive. Other strains were high in arousal and likely to be inhibited by higher levels of electric shock. However, in a

study explicitly designed to tease out possible effects of strain of rat and level of shock (Hewitt, Fulker, & Eysenck, 1978) no differences were found, and there was no evidence of psi.

The difficulty of determining whether experimenter psi is operating alongside, or instead of, animal psi is illustrated most clearly in those animal psi studies that have tested for psychokinesis. Such studies have typically tested possible influence on the behavior of a random event generator (RNG) that controls either a positive or negative stimulus. A watershed study of this type was Schmidt's (1970) investigation of the ability of animals to influence an electronic RNG. In one set of trials, a cat was placed in a cold room together with an RNG connected to a heat lamp. An above-chance scoring rate was obtained on the first five testing sessions but declined thereafter, possibly due to the cat's developing an apparent aversion to the lamp. In a second set of trials, cockroaches were tested to see if they could influence an RNG that controlled the amount of electric shock administered. The results showed that cockroaches received significantly more shocks than would be expected by chance. In explaining this psi-missing effect, Schmidt (1970) suggests a possible influence of experimenter psi, perhaps due to his admitted dislike of cockroaches.

As with ESP animal psi studies, PK studies are also complicated by decline effects and the problem of apparent individual differences in psi ability. A sophisticated study by Watkins (1971) attempted to tease out the relative influence of environmental factors on the expression of possible PK in lizards. An RNG was linked to a light that warmed the air in the chamber in which the lizards were kept; it was assumed that psi would function to maintain an optimum level of heat. The results showed that the amount of time the light stayed on was related to the amount of relative atmospheric pressure, which seemed to be the most important variable in maintaining an optimum environment. Watkins also found evidence for individual differences in success, with the strongest effect for females and for those animals which were dominant in the social hierarchy. Animals which were subordinate showed evidence of psi-missing.

In his study of PK in fish, Braud (1976) found a clear decline effect over four experimental series. In the first three series, fish appeared able to influence an RNG to produce mirror images (a positive reinforcer for some species of fish), but this effect appeared only in the first trial runs and declined consistently across trials. It is difficult to determine whether such decline effects in animals are due to habituation to stimuli or to other unknown motivational aspects of the animal, or are in fact evidence of the involvement of experimenter psi. Such effects can sometimes interact with individual differences; for instance, an exploratory study by Kennedy (1979) found clear differences in the ability of three rats to influence an RNG connected to a light stimulus, with one rat actually showing an *increase* in psi-hitting as trials progressed.

Since the 1970s there has been little experimental work exploring animal psi, with the exception of a handful of studies that have investigated PK using a mobile RNG device called the tychoscope. The researcher, Chauvin, who published early animal psi work under the name of Duval, tested the ability of mice to influence the movement of a tychoscope (Chauvin, 1986). Earlier tests had indicated that if the device was presented close to the mice, and to the rear of the cage, they became afraid of it. Results suggested that mice could significantly influence the device to move away from the cage; however, this effect appeared to be specific to certain animals and was not replicated in subsequent trials.

Later work using the tychoscope focused on whether instinctual imprinting behaviors in young animals may function as a vehicle for possible psi. Imprinting is a much-studied phenomenon whereby young animals develop an attachment to the earliest stimulus they are exposed to, which is usually the parent. Peoc'h (1988) imprinted young chicks to a tychoscope and tested whether, if prevented from following the tychoscope, they could use PK to influence the machine to spend more time near their cage. To control for effects of experimenter PK, trials were analyzed only after the completion of the whole experiment, and the experimenter was not present during any trials. The results showed that the tychoscope spent significantly more time close to the cage when imprinted chicks were present, compared to when nonimprinted chicks were present. In an attempt to replicate Peoc'h's findings, Johnson (1989) tested chicks imprinted on a different object, a rotating red box. An RNG controlled whether the box was activated or not. Despite evidence that chicks were strongly imprinted onto the box, the object was not activated at significantly more than chance levels. As Johnson (1989) notes, the lack of effect may have been partly due to methodological differences between these two studies, such as a smaller sample size and less overall exposure to the stimulus in the Johnson (1989) study.

In a much later study, Green and Thorpe (1993) attempted to improve upon Johnson's (1989) method by using larger numbers of chicks and controls for experimenter effects. In the first of four trials, a rotating light controlled by an RNG remained on for significantly longer when chicks which had been imprinted onto the light were present. However, a subsequent trial showed no effect when the experimenter was absent, suggesting the operation of experimenter PK. Additional trials to further test the effect of the presence of the experimenter and the possible influence of the chicks' locomotor activity on the RNG output found no evidence of either animal or experimenter PK, and no further evidence of level of activity influencing the RNG. Green and Thorpe (1993) suggest a possible explanation for the discrepancy between the findings of themselves and Johnson (1989), compared to Peoc'h's (1988) positive results. In the earlier Peoc'h study, the source for imprinting (and the PK target) was a moving rather than a static object. As Green and Thorpe (1993) note, a situation in

which a moving object is the target is more likely to elicit psi in imprinted animals, as this most closely mimics the imprinting process, which primes animals to maintain proximity to a (moving) parent.

In an interesting follow-up to his earlier work, Peoc'h (1995), tested whether chicks kept in the dark were able to influence an RNG that controlled the movement of a tychoscope. A candle was attached to the tychoscope so that the closer the robot was to the chicks, the more light they received. In this test, chicks were not imprinted onto the tychoscope. The machine spent significantly more time near the chicks' cage, compared to a control condition where no chicks were present. A possible problem, however, with this experiment is that no test was made without a human observer present, thus making it difficult to assess the impact of experimenter psi.

As Davis (1979) has noted, the expectation that psi could be more easily examined in less complex species studied in the laboratory has not been borne out by the evidence. The experimental findings reviewed here suggest a psi effect that is small, inconsistent, and not easily repeatable, even in the same species. Researchers have struggled with defining and eliciting nonstereotypical behavior in experimental conditions that are likely to be impoverished or even sometimes stressful for the animal. Although more rigorous attention to experimenter effects may have resulted in better designed studies in this area, there has been little success in adequately controlling for, or explaining, the role of individual differences in psi ability in animals. In addition, the potential interaction of animal and human psi in these contexts has been an intriguing but poorly understood feature of many experimental studies. In aiming to remove the experimenter-animal relationship from the testing situation by more automated procedures, experimental animal psi research may have inadvertently removed an element crucial to the expression of animal psi. We turn now to research that focuses more directly upon the human-animal relationship as a potentially rich context in which animal psi may occur.

The Companion Animal: Animal Psi in the Human-Animal Relationship

In setting out recommendations for the investigation of animal psi, Rhine (1951) and Morris (1970) suggested that the examination of spontaneous accounts of animal psi were useful mainly as a precursor to more rigorous experimental research. We will argue, however, that the focus on motivation, behavior, and interaction in field studies may facilitate a more fine-grained analysis of some of the characteristics of animal psi within the human-animal relationship. We will discuss first those cases of "clever" animals; reported as showing unusual conceptual or numerical abilities assumed to be mediated by psi. As with the studies discussed in the previous section, the interpretive framework underlying much of this work is a model of psi as the anomalous transfer of information not obtainable by normal sensory means.

An early example was the investigation by J. B. and L. E. Rhine of a horse called Lady, claimed by her owner to display "mind-reading" abilities (Rhine & Rhine, 1929a, 1929b). The horse could apparently answer questions related to numerical problems and past and future events correctly if the questions were written down and showed to her owner. Reports of Lady's ability in the popular press claimed that she had successfully located missing children (Candland, 1993). Rhine and Rhine (1929a) conducted a series of tests using various methods of screening the horse from possible facial or bodily cues from the owner. Lady was tested when her owner was blindfolded, had her movements restricted, or was behind a screen. Initial trials indicated that Lady could perform above chance even if her owner did not know the correct response to the test questions, as long as someone present knew the answer, suggesting a possible telepathy effect. Although there appears to be some evidence of the operation of psi in this case, this was heavily dependent on certain aspects of the horse-owner relationship. Although Lady did obtain some success on trials when her owner was placed behind a screen or was absent, her motivation was noticeably worse during these trials. In a few trials conducted toward the end of this testing period, just before the family was due to move from their house, Lady's performance dropped below chance. At this time, Lady's owner appeared to be under some stress and had less control over the horse.

In a follow-up investigation a year later, Lady performed below chance unless her owner knew the answer to the question *and* was in full view. Only by imitating the owner's movements and gestures could other observers obtain a result, suggesting that any success in these later trials was due wholly to cueing (Rhine & Rhine, 1929b). However, an interesting aspect of this later investigation was the fact that the relationship between Lady and her owner had deteriorated somewhat since the earlier trials. The horse's owner had become noticeably more nervous and impatient with Lady, scolding her and using a whip with greater frequency. Rhine and Rhine (1929b) conclude that the earlier trials represent good evidence for psi; however, they note that the best performance in the earlier trials occurred when the horse was in a passive, relaxed, and almost sleepy state. This state was not observed in the later, less successful trials. Research suggests that states in which human participants are more passive and relaxed are more conducive to the expression of psi (Braud, 1974, 2002; Sherwood, 2002); the Rhines' report suggests tentative but intriguing evidence that this kind of state may also be psi-facilitative in animals.

Due to their long association with humans, and their highly developed social skills, it is perhaps unsurprising that dogs have been featured as potential agents for telepathy. An early investigation by Bechterev (1949) explored the abilities of two circus-trained dogs to obey mental commands issued by their trainer, Durow. By imagining the task that the dog was expected to perform, such as fetching a book from a table, or running and sitting on a particular chair, Durow claimed to be able to

telepathically direct the dogs' behavior. Bechterev conducted a number of trials designed to control for sensory cueing by Durow. The dogs performed well, even when Durow was blindfolded and behind a screen. Trials in which other individuals acted as the agent were also largely successful. Of particular interest in this case is the assumption that physical proximity to the dog facilitated telepathy, as the trainer Durow described:

> I take his head between my hands as if I am symbolically inculcating in him the thought that he is entirely in my power ... I fix my eyes upon his ... I pull together all my nerve power and concentrate so that I entirely forget the outer world, impressing upon myself mentally the outlines of the object in which I am interested ... I turn the dog towards myself with an imperious gesture and look into his eyes, somewhere into his interior. I fix into his brain what I just before fixed into my own ... Then I mentally give him the command, or rather the mental push: "Go!" (Bechterev, 1949, pp. 167-168)

The dogs were often observed to become restless and eager to get free while this visualization process was happening; upon release, the response was usually to go rapidly to the desired object/area without hesitation. From the few trials which Bechterev reports, it is clear that this process of holding the dog's head and gazing into its eyes, even if done by individuals other than the trainer, produced greater success than those trials in which the trainer or experimenter was behind a screen. However, some trials were successful even when the trainer or experimenter was restricted to merely *imagining* looking into the dog's eyes from behind the screen.

Considered by both White (1964) and Morris (1977) to be the best study of its type, the investigation of Chris, the "Wonder Dog" was prompted by the dog's apparent ability to give the correct response to arithmetical problems by pawing the appropriate number of times on his owner's arm. Wood and Cadoret (1958) conducted a number of trials in which Chris was trained to associate each card in a standard ESP deck with a number, and to identify face-down cards by pawing the appropriate number. Trials conducted with Wood (Chris's owner) presenting the cards to the dog were highly significant, even in clairvoyance trials in which Wood did not know the correct card. However, when Cadoret observed the trials, performance was significantly below chance. Wood and Cadoret (1958) consider whether psi-missing in this case may be due to the often-cited effects of stress on performance in observed trials. To the extent that the source of psi in this study is unclear, the process by which possible stress affected performance (i.e., whether psi-missing was due to the mental strain of the dog, the owner, or both) is also difficult to elucidate.

The difficulty of disentangling human from animal psi in these kinds of trials is discussed by White (1964) in a review of ESP in dogs. White (1964) reports on her investigations of five domestic dogs, all of which were reported to be able to bark the appropriate number of times when asked what number their owners were thinking of. Three of the five dogs were found to score above chance, even when controls were introduced, such as separating the owner and dog by a screen, a door, or even placing them in separate rooms. However, White (1964) points out that complete control of all possible sensory cues was not possible in these trials and that their main utility lies in delineating important issues for future research, such as managing the motivation of both dog and owner in order to maximize performance. This account also usefully discusses more qualitative aspects of behavior that impact upon performance, such as the tendency for the best performances to occur when the owner-dog relationship was a close one.

The potentially crucial role of the human-animal relationship to potential animal psi in dogs is also considered by Rhine (1971) in his report of trials conducted in 1952 for the U.S. defense department, exploring whether dogs could detect underground dummy mines. Two dogs were used that had been previously trained to detect underground objects, and the close nature of the trainer-dog team was considered crucial to the possible success of the trials. The dogs were trained to indicate the presence of a buried box by sitting near one of a number of markers. Trainers were blind to where the boxes were buried, and efforts were made to conceal potential visual clues by raking and smoothing the sand after concealment of the boxes. Significant results were obtained, even in trials where the boxes were buried in sand under shallow water; however, performances declined substantially over time.

In discussing the identity of the psi agent in these trials, Rhine (1971) questions "whether it was perhaps the team and not either man or dog alone that produced such results" (p. 20). In an unpublished account of these trials produced for the ERDL defense laboratory, Rhine (1953) notes aspects of the changing dynamics of the trainer-dog relationships that may potentially have affected performance. In one series of trials, the dog Binnie performed particularly poorly, and Rhine (1953) noted that Binnie had been roughly treated by the trainer, Simpson, in the previous tests. A reason for this can perhaps be found in Rhine's observation that over the course of the trials Simpson had become "manifestly nervous, slightly irritable with his dogs, and, judging by little signs which one learns to look for, not enthusiastic about going on with the project" (p. 10). Although the closeness of the trainer-dog relationship had been initially seen as important to possible success, Rhine (1971) admits that he later considered this factor as a methodological weakness, clouding the identity of the psi-source. However, in trying to explain the findings, he concludes that this close coordination of human and animal may be necessary: "The man-dog

team was built up with such a tightly bound common need to locate mines that a functional integration of man and dog mentalities existed" (p. 31).

The closeness of the relationship between human and animal is particularly pertinent when considering whether animal psi can facilitate the location of a distant owner, or the prediction of an owner's return. In their early survey of psi-trailing, Rhine and Feather (1962) discuss a range of criteria for assessing this kind of evidence, such as obtaining evidence from independent witnesses, and searching for physical and behavioral characteristics of the returning animal (e.g., scars, unique deformities, unusual behavior patterns). They conclude by suggesting that naturalistic field experiments may assist in the investigation of more spontaneous aspects of animal psi that may be difficult or impossible to reproduce in a laboratory setting.

An example of such spontaneous behavior is the purported ability of some animals to predict their owner's return from a distant location. Typically, the animal is observed to wait by a door, gate, or window even when the owner is distant enough that no normal visual, olfactory, or auditory cues indicate their approach. There are many popular accounts of such behavior, which usually, though not always, involves domesticated dogs and cats (Sheldrake, 1999a). In surveys of the general public, about half of respondents tend to believe that their pet dogs can anticipate arrivals, whereas between about one tenth and one third of cat owners believe so (Sheldrake & Smart, 1997; Brown & Sheldrake, 1998; Sheldrake, Lawlor, & Turney, 1998).

A series of studies conducted over the last 10 years by Sheldrake and his colleagues provides interesting, if contentious, evidence that animal psi may feasibly be investigated in a naturalistic setting. To be adequate, such investigations need to be grounded in careful observations of the animal's spontaneous behavior, so that testing permits a full range of behavior to be sampled. To this end, Sheldrake and Smart (1998) conducted a set of preliminary observations investigating the ability of Jaytee, a dog belonging to Pamela Smart (PS), to anticipate her arrival home. Jaytee was usually left with PS's parents in her absence, and informal observations over a number of years suggested that the dog seemed to wait at the window for PS at about the same time that she departed from her destination to return home. This occurred in the absence of any sensory cues, or cues from PS's parents (who usually did not know at what time she would return). Detailed records were kept over a period of nine months recording the time at which PS left the house, and the time she started on her return journey, the means of return (e.g., taxi, her own car, friends' cars, etc.) and the times at which Jaytee went to the window. Additional information was also recorded by PS's parents, who noted any other distracting events that appeared to affect Jaytee's usual behavior at the window, such as other animals outside the window, or Jaytee's being ill. The results indicated that Jaytee seemed able to predict PS's return on 86 out of 100 occasions, and that there was a

highly significant relationship between the length of time that the dog spent waiting and the duration of PS's return journey, suggesting that the dog was detecting the time at which PS began to set off on her return journey.

In a later study, Sheldrake and Smart (2000a) tested Jaytee in two different environments: PS's parents' flat and PS's sister's flat. Blind coding of Jaytee's videotaped behavior was carried out by an independent observer. The results showed that the dog spent significantly more of the time at the window when PS was returning than at other times. In PS's sister's flat, Jaytee showed the same general pattern of behavior, but the difference between time periods was not significant. One possible contributing factor to this difference was the fact that in PS's sister's flat Jaytee had to balance on the back of a sofa to see out of the window, which resulted in less time overall being spent at the window. Graphical representations of the pattern of behavior shown in each 10-min period for each trial indicate a sharp increase in the time spent at the window in the "pre-return" period, which appears clearly distinct from the behavior observed in the rest of each period.

The criterion by which an animal's performance should be judged in these kinds of trials has been subject to some debate in the literature. Following a television program featuring Jaytee's behavior, Wiseman, Smith, and Milton (1998) tested the dog's performance in four experiments, lasting 3 hr each. Each testing period was broken down into 10-min periods, and Jaytee's behavior was videotaped throughout. In each experiment, the time at which PS set off to return from a distant location was randomly allocated by one of the experimenters. The criterion by which Jaytee's performance was measured differed in these trials from Sheldrake and Smart's (1998) earlier criterion. Wiseman, Smith, and Milton (1998) classed the first time that the dog visited the window inexplicably for more than 2 min as his "signal" that he was detecting PS's intention to return. They concluded that, based upon this criterion, Jaytee did not show evidence of being able to detect PS's intention to return.

In a reply to Sheldrake's (1999b) criticism of their use of this criterion, Wiseman, Smith, and Milton (2000) insist that testing Jaytee's ability to anticipate PS's return "did not require plotting our data and looking for a pattern, but instead simply involved determining whether Jaytee's 'signal' matched the time that PS started to return home" (p. 2). The question of how particular behaviors are interpreted is central to field experiments, where great quantities of often-complex behaviors are displayed. In such settings, careful preliminary observations are needed to determine which aspects of a dynamic pattern of behavior are central to testing a hypothesis. The detailed observations of Jaytee's behavior by Sheldrake, Smart, and PS's family, as well as the large evidence base of anecdotal reports of similar anticipatory behavior by other animals (e.g., Sheldrake, 1999a) indicate that the behavior of interest is the *sustained attention* of the animal over a period that coincides with the owner's intention to return and the initial homeward

movements. It follows that a single frequency measure of attention (rather than a duration measure) could not be expected to capture the complexity of the purported behavior in these cases.

Sheldrake (1999b) has argued that by examining the duration of time spent at the window during the whole of each experimental period (rather than just the first 2 min spent at the window) the pattern of behavior in Wiseman et al.'s (1998) results mirrors that found in his own trials. Wiseman et al. (2000) suggested that this pattern of behavior might be found if Jaytee was merely spending increasing amounts of time at the window during the period of PS's absence. However, the findings of Sheldrake and Smart (1998, 2000a) indicate that the amount of time at the window did not increase through the absence period; rather there was a sharp increase in the period immediately prior to PS's starting her homeward journey.

Sheldrake and Smart (2000b) have also tested similar behavior in another dog-owner pair and found a corresponding pattern of results, with the dog, Kane, spending significantly more time at the window when his owner was on her way home than at other periods of each trial. However, unlike Jaytee, Kane appeared to respond only when his owner had actually started her homeward journey, rather than responding just prior to this. The elimination of normal sensory cues in this set of studies suggests the operation of animal psi. It seems unlikely that in these trials the dogs were obtaining information telepathically from individuals other than the owner, since in most cases these individuals were not aware of the time of the owner's return; Sheldrake and Smart (2000b) have suggested that the most parsimonious explanation for the findings is direct telepathy between animal and owner.

A more recent study by Sheldrake and Morgana (2003) has pioneered the use of a language-trained animal to investigate animal psi. Morgana's African Grey parrot N'kisi was the subject of the study; the bird had been trained to acquire more than 700 words and was able to use grammar appropriately to speak in sentences. N'kisi was tested to see if the bird could respond to Morgana's thoughts and intentions. Care was taken to construct a testing paradigm that reflected the bird's natural behavioral responses and provided a context in which possible psi responses might operate. In informal observations, Morgana had established that N'kisi's utterances seemed to reflect her own thoughts, mostly at "moments of discovery" when Morgana had been engaged in looking for something, or noticing something for the first time. Instead of using Zener cards, which were assumed to be too repetitive, the tests involved Morgana's opening sealed envelopes containing different photographs. Both Morgana and the bird were filmed in separate rooms and the behavior of both was later transcribed blindly by independent witnesses. The findings were highly significant, suggesting that the words uttered by N'kisi upon Morgana's opening of each envelope reflected the bird's, presumably telepathic, awareness of each photograph.

In considering some of the trials in which N'kisi did not score a hit (i.e., did not utter the word most appropriate to the photograph), analysis of the utterances within the context of the way language was used is instructive. For example, on one trial, N'kisi failed to respond with the word "car" to a photograph in which a driver in a stationary car had his head out of the window. Instead the bird said, "Uh-oh, careful, you put your head out" (Sheldrake and Morgana, 2003, p. 613). Although counted as a miss, this illustrates the complexity of responses possible in animals that have acquired a greater level of language ability within a social context, and suggests interesting questions regarding the most fruitful contexts in which to search for animal psi.

Discussion: Critiquing the Evidence for Animal Psi

Taken as a whole, the evidence from animal psi research points to a psi effect that is small, not easily replicable, subject to fluctuations and decline effects, and whose source is largely unclear. Like humans, animals appear to vary in terms of possible psi ability, and as with human psi research, animal psi research indicates the central importance of the dynamics of the experimenter-participant relationship. It might be argued, however, that this relationship becomes both more important and more complex in animal psi work, due to the extra layer of interpretation necessary to understand nonhuman responses.

The difficulty of interpreting behavior even in those species assumed to display relatively simple responses illustrates this point. For example, Morris (1977) highlights the difficulty of predicting the responses of rats in a precognitive test featuring impending euthanasia. He suggests that the tendency of rats to freeze in response to an immediate predator can explain his findings that rats to be euthanised 10 min hence were less active (Morris, 1967). At the same time, the tendency of rats to take evasive action when faced with a more distant predator may explain Craig's (1973) findings that rats to be euthanised in 24 hours time were *more* active. Thus assumptions about likely responses need to be informed by a detailed knowledge of typical species-specific behaviors, as well as a recognition of the role of motivational factors and even personality differences in shaping responses (see Gosling, 2001 for a review of animal personality research).

The philosopher Thomas Nagel famously speculated about the types of subjective awareness experienced by other species, and pointed out that the often vast interspecies differences in sensory and perceptual experiences make the task of understanding animal awareness challenging (Nagel, 1974). In assessing the evidence for psi in animals, particular attention must be paid to those behaviors that look like psi but may be expressive of as-yet undiscovered normal sensory abilities. Echolocation in bats and dolphins, electrical sensitivity in a number of species of fish, and unique sensitivity to movement in insects all provide challenges in

precisely distinguishing between normal and anomalous sensory detection (see Hughes, 1999 for a review of some of the more extraordinary animal sensory abilities). In some cases, scientific research in this area has shed new light on traditional folk assumptions. For instance, long-held assumptions about the ability of many animal species to detect early earthquake activity have been borne out by more formal trials (Kirschvink, 2000), and a new strand of medical research has revealed that the tendency of some dogs to be more attentive to their owners at times of stress or illness may be at least partly related to canine ability to olfactorily detect cancer cells (McCulloch, 2006). As well as being an important precursor to designing adequate animal psi experiments, gaining a sound understanding of animal sensory, perceptual and behavioral worlds is crucial in interpreting subtle behavioral signals. This is especially true for nonmammalian species, whose social and communicative behaviors are much more difficult to interpret (e.g., Bowers & Burghardt, 1992).

A related problem concerns unexamined assumptions about the cognitive and emotional bases of animal psi. In fact, to speak of a more or less unitary ability called "animal psi" is perhaps too simplistic; there are many different species of animals, with varying levels of social, mental, and emotional complexity. In a broad generalization, Griffin has suggested that whereas humans display more "expressive psi" (e.g., telepathy, poltergeists, PK) animals may excel at more "passive" aspects of psi (e.g., psi-trailing, anticipation of danger) (Griffin, 1993, cited in Armstrong, 1996). Armstrong (1996) speculates that this might be because "The animal psyche is more closely bound to the immediate situation—to what is going on in that place and at that time" (p. 144). The value of such broad conceptualizations, however, lies mainly in their use as starting points for a more detailed examination of species-specific abilities. It is problematic, for instance, to assume that all animal awareness is mostly unconscious and nonfigurative. There is now good evidence of previously unrecognized levels of complexity in animal social intelligence (Whiten & van Schaik, 2007) emotional awareness (Panksepp, 1998, 2005a, 2005b; Preston & de Waal, 2002) and abstract thinking (Aust, Range, Steurer, & Huber, 2008; see also Bekoff, Allen, & Burghardt, 2002 for a useful general review of work in animal cognition). These findings permit a more precise understanding of some of the characteristics of awareness in different species.

A methodological challenge that is more acute in studies of animal compared to human psi is the relative difficulty of understanding and predicting the motivation of an animal in a particular testing situation. The effects of handling and emotional stress on animal behavior are now well charted (McMillan, 1999) and highlight the difficulty of producing natural behavior patterns in artificial testing environments. The importance of motivation to successful performance is especially clear from animal psi research in more naturalistic settings, such as Rhine's (1971) work with the dog-handler teams, and Wood and Cadoret's (1958) investigation of

the dog Chris. The evidence suggests that motivation is closely tied up with the quality of the relationship between animal and owner or experimenter, such that in some cases animal psi seems to be directly facilitated by close emotional bonds with a human. Indeed, in some contexts, such as psi-trailing or anticipation of an owner's return, this close bond is a necessary precursor of animal psi.

From a traditional objectivist perspective, the development of a relationship between experimenter and animal is considered to be a potentially confounding factor. So Schmidt (1970) laments the fact that he is unable to rule out an effect of experimenter psi due to his dislike for his cockroach subjects, whereas Osis (1952) wonders whether the affectionate relationship between himself and one of his experimental kittens is responsible for the animal's unusually high scoring rate. It is clear that experimenter psi can be considered a function of the type of relationship between the experimenter and the animal and may be elicited accordingly. But it may play a more central role in the expression of psi. In discussing his dog-handler clairvoyance trials, Rhine (1971) suggests that instead of seeing experimenter psi as an unwelcome confound, future experimental designs might foster "a deliberate attempt by the experimenter to *invade and augment* the subject's mental system and undertake to help or hinder him in his psi efforts" (p. 32, italics added). In the dog-handler team that Rhine tested, the close connection between dog and human, and the common goal of both may have functioned to reinforce psi in both participants. From this perspective, the experimenter effect perhaps signifies that in close relationships psi is more usefully conceptualized as a joint expression of the intentions or needs of *both* participants (Reed, 1994; see also White, 1976a, 1976b).

If psi is especially apparent in close relationships then it could be assumed to serve an adaptive function. The findings of Peoc'h (1988) that PK appears to be augmented by the imprinting process suggest that psi may be an evolutionarily based need-serving function. This was the central assumption in Stanford's (1974) psi-mediated instrumental response (PMIR) model, which holds that organisms unconsciously use psi to scan the environment for information relevant to their biological needs. Cases such as psi-trailing, the anticipation of an owner's return, and unexplained detection of danger to the owner at a distance are all examples of how psi may benefit the animal. However, the assumption that psi in such cases is analogous to the detection and processing of an information signal is still open to debate. A more recent extension of the original PMIR model, the decision augmentation theory (DAT), assumes that the anomalous acquisition of information serves to help the decision making process (May, Utts, & Spottiswoode, 1995). According to this model, PK effects may be a precognitive sampling of future information, such as RNG sequences, rather than a modification of such processes. In his later rejection of the PMIR model, Stanford (1977, 1978) proposed instead a systems-based model,

the "conformance behavior model," which explained psi as a tendency for organisms to bring about (in a teleological way) a goal which serves a need. In this model, both experimenter and participant become part of the same system: a system that is predisposed to bring about a goal: the success of the experimental task (Edge, 1978). This model more easily accounts for the evidence that the type, quality, and intensity of the human-animal relationship affect psi performance in animal psi research. Moreover, it may be that the desire to maintain and reinforce the human-animal relationship may function as a need in itself.

The evidence from animal psi research therefore points to the necessity of reconsidering the traditional general information transmission models of psi in favor of more specific predictive models (e.g., DAT) and of contemplating the possibility of frameworks that more readily encompass the interconnectedness that seems characteristic of many psi experiences (Williams, 1996). This suggests that future research needs to focus more on the relational aspects of psi within a much broader social and environmental context. For instance, Radin (2002) has suggested that the ability of animals to track their owners' whereabouts may be due to a psi-mediated ability to process nonlocal orientational information. It is likely that, as with humans, this ability may fluctuate in response to changes in the geomagnetic field (GMF). Radin (2002) compared trials in which the dog Jaytee failed to anticipate his owner's return to levels of GMF and other environmental variables during the experimental period. The findings showed that Jaytee's performance was better on days with a lower GMF flux, suggesting psi ability in both animals and humans may be strongly affected by environmental factors.

In generating appropriate explanatory frameworks within which to explore and understand psi, animal psi research highlights some of the methodological debates that characterize parapsychological, and psychological, research in general. Early animal psi researchers focused their energies in the laboratory, assuming that animal psi could be better isolated and controlled in this traditional setting. Although much of the evidence for a psi ability in animals stemmed from anecdotal or lay reports of spontaneous behavior, the fact that such reports often concerned a single animal (or were based on very small samples) and were difficult to verify and replicate meant that their evidential status was considered problematic. In Rhine and Feather's (1962) survey of psi-trailing, for instance, the authors argued that an assessment of the reliability of such cases should ideally focus on defining physical, rather than behavioral, characteristics of the returning animal. Yet the recognition of a returning pet inevitably involves assessing whether the animal behaves in the same way as the lost pet, whether it interacts in a similar way with the owner and family. Consider the case of the dog King, for example, who disappeared from his home in Idaho and turned up 3 months later in front of the door of the family's new home in California (Rhine & Feather, 1962). The new arrival was the size, shape,

and color of King, and even had a scar in the same place, but also displayed the same idiosyncratic behaviors that the family remembered, such as a dislike of hissing noises and a tendency to always shake hands with the left paw. Through their interactions with the dog, the family were convinced that the newcomer was in fact King. It is only in the context of interaction and relationship that these kinds of judgments can be made, yet they are often assigned low evidential status. Rhine and Feather (1962) suggested, for instance, that future investigation of ostensible psi-trailing cases might more fruitfully seek even more reductionistic proof of identification, such as blood tests, as more desirable criteria.

The present debate within the social sciences concerning the validity of different types of evidence is of central importance to efforts to understand and explain psi phenomena. In the current positivist paradigm, quantitative evidence from traditional experimental settings is privileged over more qualitative accounts of spontaneous experiences. Substantial effects using large samples are sought, and smaller, more intimate, studies of the complex behavior of individuals are neglected. Yet evidence from animal psi work indicates that a richer account of animal psi may be gleaned from protracted observations in more naturalistic study settings, in which the relationship between animal and researcher is seen as an integral part of research design. For such an approach to be considered adequate, more sophisticated methodological frameworks may be required that accept the valuable role of direct observation, phenomenological experience, and intersubjective, process-oriented explanations of psi (e.g., White, 1990; Braud, 1994; Braud & Anderson, 1998).

Essentially the same tension between experimental and naturalistic forms of enquiry has characterized recent debates about the difficulties of studying animal consciousness. In contemporary behaviorist and cognitive accounts of animal awareness, thinking is assumed to be a mode of information processing and manipulation, which is best studied using abstract cognitive tasks in the laboratory (Dutton & Williams, 2004). Concurrent with this view is the more insidious assumption that alternative accounts of animal mind that feature subjective awareness, motivation, intention, or emotion are necessarily biased, anthropomorphic and perhaps a little naïve. Like some debates in parapsychology, the debate about the existence of animal awareness has often been polarized, being portrayed as involving "believers" and "skeptics," as involving "killjoy" interpretations waging war on "sentimental" or "anthropomorphic" accounts, and as being a battle between lay assumptions and more "hard-nosed" scientific explanations. Parapsychologists will be familiar with the tenor of such debates, which often reflect rhetorical aims rather than informed criticism (see Zingrone, n.d., particularly her discussion of the rhetoric involved in the Wiseman et al. (1998, 2000) critique of the Sheldrake studies, and also Zingrone, 2006).

A useful development in the comparative cognition field has been a call for a more "critical anthropomorphism" (Burghardt, 1985;

Mitchell, Thompson, and Miles, 1997). This approach rejects both uncritically anthropomorphic and excessively mechanistic approaches in favor of a more rigorous comparison of human and animal awareness that is grounded in the knowledge of the behavioral and sensory world of a particular species. It also incorporates a reflective awareness of our own role in that world, in our relationships with animals and the assumptions and attributions we make about their experience (Davis & Balfour, 1992). The models of animal psi that we construct and the methodological approaches we use for its investigation are similarly structured by prevailing conceptual and methodological assumptions about animal awareness, motivation, and the role of the human-animal relationship in research. A more complete understanding of animal psi demands greater awareness of such assumptions, and calls for a more participatory context in which to study potential psi abilities in animals.

References

Armstrong, S. J. (1996). Souls in process: A theoretical inquiry into animal psi. In M. Stoeber & H. Meynell (Eds.), *Critical reflections on the paranormal* (pp. 133–158). New York: SUNY.

Aust, U., Range, F., Steurer, M., & Huber, L. (2008). Inferential reasoning by exclusion in pigeons, dogs, and humans. *Animal Cognition,* **11**, 587–597.

Bechterev, W. (1949). "Direct influence" of a person upon the behavior of animals. *Journal of Parapsychology,* **13**, 166–176.

Bekoff, M., Allen, C., & Burghardt, G. M. (Eds.) (2002). *The cognitive animal: Empirical and theoretical perspectives on animal cognition.* Cambridge, MA: MIT Press.

Bowers, B. B., & Burghardt, G. M. (1992). The scientist and the snake: Relationships with reptiles. In H. Davis and D. Balfour (Eds.), *The inevitable bond: Examining scientist-animal interactions* (pp. 250–163). Cambridge, UK: Cambridge University Press.

Braud, W. G. (1974). Relaxation as a psi-conducive state. *Bulletin of the Psychonomic Society,* **3**, 115–118.

Braud, W. G. (1976). Psychokinesis in aggressive and nonaggressive fish with mirror presentation feedback for hits: Some preliminary experiments. *Journal of Parapsychology,* **40**, 296–307.

Braud, W. G. (1994). Honoring our natural experiences. *Journal of the American Society for Psychical Research,* **88**, 293–308.

Braud, W. G. (2002). Psi-favorable conditions. In V. G. Rammohan (Ed.), *New frontiers of human science: A festschrift for K. Ramakrishna Rao* (pp. 95–118). Jefferson, NC: McFarland.

Braud, W. G., & Anderson, R. (1998). *Transpersonal research methods for the social sciences: Honoring human experience.* London: Sage.

Brown, D. J., & Sheldrake, R. (1998). Perceptive pets: A survey in northwest California. *Journal of the Society for Psychical Research,* **62**, 396–406.

Burghardt, G. M. (1985). Animal awareness: Current perceptions and historical perspective. *American Psychologist,* **40**, 905–919.

Candland, D. K. (1993). *Feral children and clever animals: Reflections on human nature.* New York: Oxford University Press.

Chauvin, R. (1986). A PK experiment with mice. *Journal of the Society for Psychical Research,* **53**, 348–351.

Craig, J. G. (1973). The effect of contingency on precognition in the rat [Abstract]. In W. G. Roll, R. L. Morris, & J. D. Morris (Eds.), *Research in Parapsychology 1973* (pp. 154–156). Metuchen, NJ: Scarecrow Press.

Craig, J. G. (1975). The effect of the experimenter on precognition in the rat [Abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in Parapsychology 1974* (pp. 97–102). Metuchen, NJ: Scarecrow Press.

Craig, J. G., & Treurniet, W. (1974). Precognition in rats as a function of shock and death [Abstract]. In W. G. Roll, R. L. Morris, & J. D. Morris (Eds.), *Research in Parapsychology 1973* (pp. 75–78). Metuchen, NJ: Scarecrow Press.

Dalton, K. (1997). Is there a formula to success in the ganzfeld? Observations on predictors of psi-ganzfeld performance. *European Journal of Parapsychology,* **13**, 71–82.

Davis, J. (1979). Psi in animals: A review of laboratory research. *Parapsychology Review,* **10**, 1–9.

Davis, H., & Balfour, D. (Eds.). (1992). *The inevitable bond: Examining scientist-animal interactions.* Cambridge, UK: Cambridge University Press.

Dutton, D., & Williams, C. (2004). A view from the bridge: Subjectivity, embodiment and animal minds. *Anthrozoös,* **17**, 210–224.

Duval, P., & Montredon, E. (1968a). ESP experiments with mice. *Journal of Parapsychology,* **32**, 153–166.

Duval, P., & Montredon, E. (1968b). Further psi experiments with mice [Abstract]. *Journal of Parapsychology,* **32**, 260.

Edge, H. L. (1978). A philosophical justification for the conformance behavior model. *Journal of the American Society for Psychical Research,* **72**, 215–231.

Eysenck, H. J. (1975). Precognition in rats. *Journal of Parapsychology,* **39**, 222–227.

Gosling, S. D. (2001). From mice to men: What can we learn about personality from animal research? *Psychological Bulletin,* **127**, 45–86.

Green, P. R., & Thorpe, P. H. (1993). Tests for PK effects in imprinted chicks. *Journal of the Society for Psychical Research,* **59**, 48–60.

Griffin, D. R. (1993). Parapsychology and philosophy: A Whiteheadian postmodern perspective. *Journal of the American Society for Psychical Research,* **87**, 217–288.

Grim, P. (1984). Psi phenomena and the Rosenthal effect. *New Ideas in Psychology,* **2**, 35–45.

Hardy, A. (1965). *The living stream: Evolution and man.* New York: Harper & Row.

Hewitt, J. K., Fulker, D. W., & Eysenck, H. J. (1978). Effect of strain and level of shock on the behavior of rats in PSI experiments. *Psychological Reports,* **42**, 1103–1108.

Hughes, H. C. (1999). *Sensory exotica: A world beyond human experience.* Cambridge, MA: MIT Press.

Johnson, M. H. (1989). Imprinting and ANPSI: An attempt to replicate Peoc'h. *Journal of the Society for Psychical Research,* **55**, 417–419.

Kennedy, J. E. (1979). Exploratory PK experiments using light as the stimulus. *Journal of Parapsychology,* **43**, 358–359.

Kirschvink, J. L. (2000). Earthquake prediction by animals: Evolution and sensory perception. *Bulletin of the Seismological Society of America,* **90**, 312–323.

Levin, J. (1975). Unpublished manuscript.

Maeterlinck, M. (2006). *The life of the bee.* New York: Dover Publications. Original work published 1901.

May, E. C., Utts, J. M., & Spottiswoode, J. P. (1995). Decision augmentation theory: Toward a model of anomalous mental phenomena. *Journal of Parapsychology,* **59**, 195–220.

McCulloch, M. (2006). Diagnostic accuracy of canine scent detection in early- and late-stage lung and breast cancers. *Integrative Cancer Therapies,* **5**, 30–39.

McMillan, F. D. (1999). Effect of human contact on animal well-being. *Journal of the American Veterinary Medical Association,* **215**, 1592–1598.

Mitchell, R. W., Thompson, N. S., & Miles, H. L. (Eds.). (1977). *Anthropomorphism, anecdotes, and animals.* New York: SUNY.

Morris, R. L. (1967). Some new techniques in animal psi research. *Journal of Parapsychology,* **31**, 316–317.

Morris, R. L. (1970). Psi and animal behavior: A survey. *Journal of the American Society for Psychical Research,* **64**, 242–261.

Morris, R. L. (1977). Parapsychology, biology and Anpsi. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 687–715). Jefferson, NC: McFarland.

Nagel, T. (1974). What is it like to be a bat? *Philosophical Review,* **83**, 435–450.

Osis, K. (1952). A test of the occurrence of a psi effect between man and the cat. *Journal of Parapsychology,* **16**, 233–256.

Osis, K., & Foster, E. B. (1953). A test of ESP in cats. *Journal of Parapsychology,* **17**, 168–186.

Panksepp, J. (1998). *Affective neuroscience: The foundations of human and animal emotions.* New York: Oxford University Press.

Panksepp, J. (2005a). Beyond a joke: From animal laughter to human joy. *Science,* **308**, 62–63.

Panksepp, J. (2005b). Affective consciousness: Core emotional feelings in animals and humans. *Consciousness and Cognition,* **14**, 30–80.

Parker, A. (1974). ESP in gerbils using positive reinforcement. *Journal of Parapsychology,* **38**, 301–311.

Peoc'h, R. (1988). Chicken imprinting and the tychoscope: An ANPSI experiment. *Journal of the Society for Psychical Research,* **55**, 1–9.

Peoc'h, R. (1995). Psychokinetic action of young chicks on the path of an illuminated source. *Journal of Scientific Exploration,* **9**, 223–229.

Preston, S. D., & de Waal, F. B. M. (2002). Empathy: Its ultimate and proximate bases. *Behavioral and Brain Sciences,* **25**, 1–72.

Radin, D. (2002). A dog that seems to know when his owner is coming home: Effect of environmental variables. *Journal of Scientific Exploration,* **16**, 579–592.

Reed, H. (1994). Intimacy and psi: A preliminary exploration. *Journal of the American Society for Psychical Research,* **88**, 327–360.

Rhine, J. B. (1950). Parapsychology and biology. *Journal of Parapsychology,* **14**, 85–94.

Rhine, J. B. (1951). The present outlook on the question of psi in animals. *Journal of Parapsychology,* **15**, 230–251.

Rhine, J. B. (1953). Unpublished report retrieved 11/15/2007 from http://www.thememoryhole.org/mil/animal_esp.htm.

Rhine, J. B. (1971).Location of hidden objects by a man-dog team. *Journal of Parapsychology,* **35**, 18–33.

Rhine, J. B. (1974). Comments: A new case of experimenter unreliability. *Journal of Parapsychology,* **38**, 215–225.

Rhine, J. B., & Rhine, L. E. (1929a). An investigation of a "mind-reading" horse. *Journal of Abnormal and Social Psychology,* **23**, 449–466.

Rhine, J. B., & Rhine, L. E. (1929b). Second report on Lady, the "mind-reading" horse. *Journal of Abnormal and Social Psychology,* **24**, 287–292.

Rhine, J. B., & Feather, S. B. (1962). The study of cases of "psi-trailing" in animals. *Journal of Parapsychology,* **26**, 1–22.

Rosenthal, R. (1984). Interpersonal effects and PSI: Some communalities and differences. *New Ideas in Psychology,* **2**, 47–50.

Rosenthal, R., & Fode, K. L. (1963). The effect of experimenter bias on the performance of the albino rat. *Behavioral Science,* **8**, 183–189.

Schmidt, H. (1970). PK experiments with animals as subjects. *Journal of Parapsychology,* **34**, 255–261.

Schouten, S. A. (1972). Psi in mice: Positive reinforcement. *Journal of Parapsychology,* **36**, 261–282.

Schouten, S. A. (1973). Psi in mice: Role of target, spatial position of target and response preferences. Unpublished manuscript.

Selous, E. (1931). *Thought transference in birds.* London: Constable and Co.

Sheldrake, R. (1999a). *Dogs that know when their owners are coming home and other unexplained powers of animals: An investigation.* New York: Crown.

Sheldrake, R. (1999b). Commentary on a paper by Wiseman, Smith and Milton on the "psychic pet" phenomenon. *Journal of the Society for Psychical Research,* **63**, 306–311.

Sheldrake, R. (2000). The "psychic pet" phenomenon. *Journal of the Society for Psychical Research,* **64**, 126–128.

Sheldrake, R., Lawlor, C., & Turney, J. (1998). Perceptive pets: A survey in London. *Biology Forum,* **91**, 57–74.

Sheldrake R., & Morgana, A. (2003). Testing a language-using parrot for telepathy. *Journal of Scientific Exploration,* **17**, 601–616.

Sheldrake, R., & Smart, P. (1997). Psychic pets: A survey in north-west England. *Journal of the Society for Psychical Research,* **61**, 353–364.

Sheldrake, R., & Smart, P. (1998). A dog that seems to know when its owner is returning: Preliminary investigations. *Journal of the Society for Psychical Research,* **62**, 220–232.

Sheldrake, R., & Smart, P. (2000a). A dog that seems to know when his owner is coming home: Videotaped experiments and observations. *Journal of Scientific Exploration,* **14**, 233–255.

Sheldrake, R. & Smart, P. (2000b). Testing a return-anticipating dog, Kane. *Anthrozoös,* **13**, 203–212.

Sherwood, S. J. (2002). Relationship between the hypnagogic/hypnopompic states and reports of anomalous experiences. *Journal of Parapsychology,* **66**, 127–150.

Sherwood, S., Roe, C., Holt, N., & Wilson, S. (2005). Interpersonal psi: Exploring the role of the experimenter and the experimental climate in a ganzfeld telepathy task. *European Journal of Parapsychology,* **20**, 150–172.

Stanford, R. G. (1974). An experimentally testable model for spontaneous psi events. I. Extrasensory events. *Journal of the American Society for Psychical Research,* **66**, 34–57.

Stanford, R. G. (1977). Are parapsychologists paradigmless in psiland? In B. Shapin & L. Coly (Eds.), *The philosophy of parapsychology* (pp. 1–16). New York: Parapsychology Foundation.

Stanford, R. G. (1978). Toward reinterpreting psi events. *Journal of the American Society for Psychical Research,* **72**, 197–214.

Taylor, R. (2003). Evolutionary theory and psi: Reviewing and revising some need-serving models in psychic functioning. *Journal of the Society for Psychical Research,* **67**, 1–17.

Terry, J. C. (1976). Continuation of the rodent precognition experiments [Abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in Parapsychology 1975* (pp. 11–14). Metuchen, NJ: Scarecrow Press.

Terry, J. C., & Harris, S. A. (1975). Precognition in water-deprived rats [Abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in Parapsychology 1974* (p. 81). Metuchen, NJ: Scarecrow Press.

Watkins, G. K. (1971). Possible PK in the lizard *Anolis sagrei*. *Proceedings of the Parapsychological Association,* **8**, 23–25.

White, R. A. (1964). The investigation of behavior suggestive of ESP in dogs. *Journal of the American Society for Psychical Research,* **58**, 250–279.

White, R. A. (1976a). The influence of persons other than the experimenter on the subject's scores in psi experiments. *Journal of the American Society for Psychical Research,* **70**, 133–166.

White, R. A. (1976b). The limits of experimenter influence on psi test results: Can any be set? *Journal of the American Society for Psychical Research,* **70**, 333–369.

White, R. A. (1990). An experience-centered approach to parapsychology. *Exceptional Human Experience,* **8**, 7–36.

Whiten A., & van Schaik, C. P. (2007). The evolution of animal "cultures" and social intelligence. *Philosophical Transactions of the Royal Society: Biological Sciences,* **362**, 603–620.

Williams, C. (1996). Metaphor, parapsychology and psi: An examination of metaphors related to paranormal experience and parapsychological research. *Journal of the American Society for Psychical Research,* **90**, 174–201.

Wiseman, R., Smith, M., & Milton, J. (1998). Can animals detect when their owners are returning home? An experimental test of the "psychic pet" phenomenon. *British Journal of Psychology,* **89**, 453–462.

Wiseman, R., Smith, M., & Milton, J. (2000). The "psychic pet" phenomenon: A reply to Rupert Sheldrake. *Journal of the Society for Psychical Research,* **64**, 46–49.

Wood, G. H., & Cadoret, R. J. (1958). Tests of clairvoyance in a man-dog relationship. *Journal of Parapsychology,* **22**, 29–39.

Zingrone, N. L. (n.d.). Failing to go the distance: On critics and parapsychology. *Skeptical Investigations.* Retrieved 3/27/2008 from http://www.skepticalinvestigations.org/exam/Zingrone_critics.htm.

Zingrone, N. L. (2006). Complicating the conversation: Rhetoric, substance, and controversy in parapsychology. *Journal of Parapsychology,* **69**, 3–21.

Liverpool Hope University
Hope Park
LiverpoolL16 9JD, UK
DUTTOND@hope.ac.uk

Acknowledgments

We would like to thank two anonymous reviewers for their helpful comments on the manuscript.

Abstracts in Other Languages

Spanish

BESTIAS INTELIGENTES Y MASCOTAS FIELES: UNA REVISIÓN CRÍTICA DE LA INVESTIGACIÓN PSI CON ANIMALES

RESUMEN: Aunque la investigación psi con animales comparte algunas premisas teóricas y metodológicas con la investigación psi con seres humanos, la investiugación con animales presenta sus propios retos conceptuales y empíricos. Exploramos dos aspectos principales de la investigación psi con animales: (1) investigación experimental, la cual tiende a explicar psi animal como un proceso evolucionario de adaptación de transmisión de información, y (2) estudios de campo, los cuales han enfatizado la importancia de la relación humana-animal en la expresión de psi. Aunque hay algun apoyo para ambos acercaminetos, nos parece que la evidencia sugiere que el psi animal puede funcionar como una expresión de relaciones o de "resonancia" entre individuos. Sugerimos que quizás sería más provechoso buscar el psi animal en el contexto de relaciones humano-animales cercanas, y que la parapsicología necesita buscar metodologias que produzcan la expresión de psi en contextos relacionales.

French

BETES INTELLIGENTES ET ANIMAUX FIDELES: UN EXAMEN CRITIQUE DE LA RECHERCHE SUR LE PSI ANIMAL

RESUME: Bien que partageant les principaux postulats théoriques et méthodologiques de la recherche sur le psi humain, la recherché sur le psi animal pose des défis conceptuels et empiriques uniques. Nous explorons deux grands volets de la recherche sur le psi animal: (1) la recherche expérimentale qui tente d'expliquer le psi animal comme un processus évolutif d'adaptation de la transmission d'informations, et (2) la recherche de terrain qui pointe l'importance de la relation homme-animal pour l'expression du psi. Bien qu'il y ait quelques éléments favorables à ces deux cadres explicatifs dans la recherche sur le psi animal, nous soutenons que les preuves suggèrent que le psi animal puisse fonctionner comme une expression d'une relation ou d'une « résonance » entre individus. Nous suggérons qu'il serait peut-être plus profitable de chercher du psi animal dans le contexte de relations homme-animal étroites, et que la parapsychologie a besoin d'envisager des méthodologies qui permettent l'expression du psi dans des contextes relationnelles.

German

KLUGE VIECHER UND TREUE HAUSTIERE: EINE KRITISCHE ÜBERSICHT ÜBER PSI-FORSCHUNG BEI TIEREN

ZUSAMMENFASSUNG: Obwohl die Psi-Forschung bei Tieren theoretische und methodologische Kernannahmen mit der Psi-Forschung bei Menschen teilt, bietet sie eigene begriffliche und empirische Herausforderungen. Dargestellt werden zwei Bereiche der Psi-Forschung bei Tieren: (1) die Experimentalforschung, die dazu tendiert hat, Psi bei Tieren als einen evolutionär bedingten Anpassungsprozeß der Informationsübertragung zu erklären, und (2) die Feldforschung, die die Bedeutung der Mensch-Tier-Beziehung für die Entfaltung von Psi herausgestellt hat. Wenn es auch einige unterstützende Befunde für beide Erklärungsmodelle gibt, vertreten wir den Standpunkt, daß das Beweismaterial eher dafür spricht, Psi bei Tieren als Ausdruck einer Beziehung oder als "Resonanz" zwischen Individuen aufzufassen. Wir schlagen vor, daß es erfolgversprechender sein könnte, Psi bei Tieren im Kontext enger Mensch-Tier-Beziehungen zu untersuchen und daß sich die Parapsychologie eher an solchen Methodologien orientieren sollte, die für die Entfaltung von Psi in Beziehungskontexten förderlich sind.

ADDRESSING THE SURVIVAL VERSUS PSI DEBATE THROUGH PROCESS-FOCUSED MEDIUMSHIP RESEARCH

By Julie Beischel and Adam J. Rock

ABSTRACT: Over a century of mediumship research concludes that skilled mediums are able to report accurate and specific information about the deceased loved ones (termed discarnates) of living people (termed sitters) during anomalous information reception (AIR); that is, without any prior knowledge about the discarnates or sitters, in the absence of sensory feedback, and without using deceptive means. However, this historical body of proof-focused research does not directly address which parapsychological mechanisms are involved in AIR by mediums. The data, in and of themselves, support multiple hypotheses including the super-psi and psychic reservoir (collectively "psi") models as well as the survival of consciousness hypothesis (or simply "survival"). However, by restricting research to proof-focused studies, investigators neglect important phenomenological mediumship processes underlying AIR and how those processes might address the survival hypothesis. This process-focused investigation of mediums' experiences may lead to a better understanding of the source of the information mediums report during AIR. This paper briefly summarizes the results from recent process-focused studies of modern-day, American mental mediums' experiences during ostensible communication with discarnates. We also suggest areas for future process-focused mediumship studies to more fully address the question at the root of mediumship research: Is there life after death?

Keywords: mediumship, survival psi, somatic psi, phenomenology, survival of consciousness, anomalous information reception

Although parapsychological research most often involves the Big Four—telepathy, clairvoyance, precognition, and psychokinesis—with only "an occasional nod toward survival and afterlife topics" (Braud, 2005, p. 40), the continued investigation of the latter issues is pivotal for our understanding of consciousness, the potential of the mind, and the nature of life in general. One of the main methods for scientifically addressing life after death involves studying mediums—individuals who report regular communication with the deceased.

Skilled mediums are able to report information that is both accurate and specific about the deceased loved ones (termed discarnates) of living people (termed sitters[1]) using anomalous information reception

[1] Mediums performing readings with proxy sitters provide information for living people who are not present at the reading. Consequently, "sitter" would be more completely defined as a living person who requested a reading from a medium and who has a desire to receive information about one or more deceased people with whom s/he had an emotionally close relationship, irrespective of whether or not s/he is present for or hears the reading as it takes place. Conversely, a "proxy sitter" is a living person who is present for the reading but is not the person for whom the information reported during a reading is intended. A proxy sitter may or may not have knowledge about the absent sitter or the deceased persons contacted during the reading.

(AIR); that is, without any prior knowledge about the discarnates or sitters, in the absence of any sensory feedback, and without using deceptive means (e.g., Beischel & Schwartz, 2007). However, after over a century of research (reviewed by Braude, 2003; Fontana, 2005; Gauld, 1983), mediumship findings en bloc do not directly address which parapsychological mechanisms are involved in AIR by mediums. That is, the data, in and of themselves, support multiple hypotheses including: (a) the survival of consciousness (i.e., life after death; the continued existence, separate from the body, of at least portions of an individual's consciousness or personality after physical death), (b) the psychic reservoir hypothesis (i.e., that all information since the beginning of time is stored somehow and somewhere in the universe and mediums are accessing that cosmic store rather than communicating with the deceased; reviewed in Fontana, 2005), and (c) super-psi (also called super-ESP; discussed in detail in Braude, 2003, and reviewed in Fontana, 2005). Super-psi,[2] the retrieval of information through telepathy with the living, clairvoyance, and/or precognition, is deemed *super* by its ostensible requirement of "more refined and extensive psychic functioning than we discover in controlled laboratory studies" (Braude, 2003, p. 11). In the super-psi explanation, a medium may receive information through multiple psi processes:

> telepathically from the mind of the sitter (even though the latter may not be consciously thinking about the information at the time), telepathically from the minds of people elsewhere, clairvoyantly from the environment, or even precognitively from the future moment when the sitter checks on the facts given in the communications and finds them to be correct. (Fontana, 2005, p. 104)

However, it is important to note that the survival hypothesis also requires some form of psi in order for information to be transferred from the discarnate to the medium. Indeed, either "the medium acquires her knowledge of discarnate minds by telepathically scanning their minds or ... the discarnate person is telepathically sending information to a medium's mind. In either case, living agent telepathy is operative" (Sudduth, 2009, p. 177). Sudduth (2009) terms this "survival psi" and describes it as "a highly refined and efficacious sort of psi functioning ... indistinguishable from the degree or kind of psi required by the super-psi hypothesis" (p. 184).

Because mediums are ostensibly using psi regardless of the source of information—living persons or a cosmic database in the super-psi and psychic reservoir theories, respectively, and deceased persons in the survival hypothesis—in our current discussion of the "survival versus psi debate," we

[2] Detailed discussions of the controversies surrounding the definitions and assumptions of the super-psi hypothesis can be found in Braude (2003) and Sudduth (2009). The survival and super-psi models are also briefly compared in Irwin and Watt (2007, pp. 143–144).

are using the term "somatic[3] psi" to describe telepathy with living persons, clairvoyance (including of a psychic reservoir), and precognition on the part of the medium but not including survival psi.

To determine which of these parapsychological hypotheses—survival psi or somatic psi—best accounts for AIR by mediums, further research is needed. This work is important for reasons that are academically important as well as those that are socially relevant. First, an understanding of the process mediums use may aid in determining which mechanisms are at work during the processing of nonlocal, nonsensory information. Second, mediumship research findings provide unique evidence for an issue central to consciousness science: the relationship between the mind/consciousness and the brain. That is, is consciousness (a) a localized product of the brain as theorized by materialist neuroscientists (e.g., Crick & Koch, 2003) or is consciousness (b) nonlocal and mediated, transmitted, transformed, guided, or arbitrated by the brain (e.g., Clarke, 1995)?

In the social arena, this research is significant beyond just addressing society's growing interest in mediumship and the survival of consciousness. First, mediums may be able to find missing persons or contribute to criminal investigations, but in order for the information mediums provide to be sensibly utilized by society, the processes by which it is acquired need to be better understood. In addition, the information mediums provide may include knowledge or wisdom beneficial to scientific, technological, and/or social progress. Furthermore, scientific evidence for life after death may alleviate the anxiety felt by hospice and end-of-life patients and their families and alter the way allopathic physicians perceive death. Mediumship readings may also be helpful in grief counseling and recovery. Finally, evidence for immortality may affect individual and group behavior. For example, research in Terror Management Theory has found that belief in an afterlife may liberate people from "the compulsion to continually prove our value and the correctness of our beliefs" (Dechesne et al., 2003), an impulse that can manifest in extreme cases as radical actions that defend or propagate the dominance of one's beliefs, religion, nation, and so on (which provides the individual with the psychological comfort of symbolic immortality). For these academic and socially relevant reasons, the continued investigation of mediumship and how the phenomenon of AIR relates to survival remains important.

The aim of this paper is to discuss how the examination of mediums' *experiences* during ostensible communication with the deceased adds an important facet to the field of mediumship research and its contribution to our understanding of the survival of consciousness hypothesis. Here, we briefly summarize the results from our recent research on the experiences of modern-day, American mental mediums during mediumship readings

[3] Here, the term somatic is used in reference to the physical body of the living client in psychic readings as well as the "body" of information described by the psychic reservoir hypothesis.

and discuss the implications of recent findings for the survival psi versus somatic psi debate. We also formulate suggestions for future mediumship studies intended to more fully address the survival question. However, it may be prudent to first discuss the distinction between proof-focused and process-focused research.

Proof- Versus Process-Focused Research

The majority of previous and historic mediumship research was *proof*-focused; that is, it tested mediums' claims that they could report accurate information about the deceased. Studies do, of course, exist in which investigators attempted to directly address the survival hypothesis in mediumship research, though these studies were in the minority. For example, in the paper "Linkage Experiments with Mediums," Karlis Osis (1966) described a series of experiments in which "the question of survival after death [was] the problem under investigation" (p. 92). The methods used were designed "to separate information obtained by [telepathy] from living sources from that obtained from the deceased" (p. 92). In these linkage experiments, a chain of individuals is placed between the medium and the sitter in an attempt to "block" telepathy between them. For instance, the sitter "could ask his acquaintance, Jones, to contact an experimenter, Smith, who turns to an assistant, Brown, who then conducts a sitting with [a medium]" (p. 94). However, because we do not understand the limits of somatic psi, introducing even an infinite number of links in a proof-focused study cannot eliminate somatic psi as an explanation for AIR. The results from Osis's study indicated "no significant phenomena" (p. 117).

This use of proxy sitters during mediumship readings to block the flow of information (though usually through "normal" means) is nearly as old as mediumship research itself (e.g., reviewed by Kelly, in press; Schmeidler, 1958; Thomas, 1932–1933; West, 1949). Other theoretical experiments historically suggested as "ideal" in differentiating survival from other explanations for mediums' accuracy involve the retrieval of the combination to a lock (or other code) during a reading that only the discarnate knew; asking the medium to respond to a language (in that language) that the discarnate spoke but that the medium does not (i.e., xenoglossy); obtaining information during a reading from a discarnate unknown to the sitter, medium, or experimenter (i.e., drop-in communicators); and acquiring information that cannot be fully understood until information from another reading is obtained (i.e., cross-correspondence) (Braude, 2003, pp. 283–88; Irwin & Watt, 2007, pp. 138–42). These suggestions involve uncorroborated conjecture about a discarnate's ability and motivation to convey specific information and about a medium's ability to receive and report it; these errors have been previously discussed elsewhere (Beischel, 2007/2008).

Most previous—especially recent—mediumship research has been primarily concerned with empirically demonstrating a particular and replicable effect (i.e., AIR by mediums) without specifically addressing the survival hypothesis. For example, in their paper "Results of the Application of the Robertson-Roy Protocol [RRP] to a Series of Experiments with Mediums and Participants," the third in a series, Roy and Robertson (2004) describe the RRP as a "practical, repeatable, and useful procedure in assessing the ability of mediums to transmit relevant information to recipients" (p. 18). Researchers O'Keefe and Wiseman (2005) also claimed to have developed "a practical, straightforward, and methodologically sound way of testing [mediums'] claims" (p. 175). Indeed, the "primary purpose" of the first author (JB)'s own previous research "was to acquire novel evidence concerning the possibility that accurate information about a sitter's deceased loved ones could be reliably obtained from research mediums under highly controlled experimental conditions that effectively eliminated conventional (classical) explanations" (Beischel & Schwartz, 2007, p. 24). All of these studies were primarily concerned with gathering evidence (either for or against) the claims of AIR that mediums make, and none directly addressed the survival hypothesis.

This type of proof-focused research alone discounts the mediums' *actual experiences* of communication with the deceased. Several authors have noted the importance of these types of experiences. Cardeña, Lynn, and Krippner (2000) propose that "some anomalous experiences may have much to offer science in terms of clarifying its current boundaries and identifying how psychology, the neurosciences, and the social sciences can join hands to explain [the variety] of life" (p. 10). In addition, in his book *Exploring Unseen Worlds*, George William Barnard (1997) describes how William James "insists that mystical experiences are more than simply an amalgam of physiological, psychological, or sociological factors" (p. 18). Barnard expresses that, for James, anomalous cognitive states such as mystical experiences "are important sources of data on the existence of realms of reality or dimensions of consciousness that exceed (even while interpenetrating) our everyday 'natural' reality or our typical waking consciousness" (p. 18). Thus, the evaluation of mediums' experiences may be important for our understanding of the boundaries of science and reality.

Proof-focused mediumship research fails to examine the phenomenological processes employed by mediums during AIR and how those processes might address the survival hypothesis. Phenomenology "is a term that refers to a philosophy, a research approach, and, in a more general way, the study of experience" (Pekala & Cardeña, 2000, p. 59). The phenomenological investigator engages in *process*-focused research investigating "the way things are experienced by the experiencer, and … how events are integrated into a dynamic, meaningful experience" (Hanson & Klimo, 1998, p. 286). With this type of analysis, the researcher is

able to identify the essential aspects of the experience under investigation (Fischer, 1998). Previous phenomenological research has investigated, for example, the experience of meditation (Gifford-May & Thompson, 1994), being unconditionally loved (Matsu-Pissot, 1998), and shamanic-like journeying (Rock, 2006). It is noteworthy that process-focused research may be qualitative (e.g., Gifford-May & Thompson, 1994) or quantitative (e.g., Pekala, 1991), and numerous scholars argue that these two approaches are reconcilable and may inform one another (e.g., Abussabha & Woelfel, 2003; Burke-Johnson & Onwuegbuzie, 2004; Crawford, Weever, Rutter, Sensky, & Tyrer, 2002; Foss & Ellefsen, 2002). Process-focused investigations of mediums' phenomenology during mediumship and psychic readings may aid in defining the source(s) of information for each.

Any future proof-focused research in the absence of a process-focused component may continue to provide support for AIR, but it will also continue to overlook addressing the survival hypothesis and go on failing to differentiate between survival and the other parapsychological explanations. By combining mediums' reports that they are communicating directly with the deceased; their alleged ability to differentiate between that communication and their use of telepathy, clairvoyance, and precognition; and experimental evidence that the mediums' two experiences are in fact different under controlled conditions, it may be possible to arrive at an empirically driven distinction between survival versus super-psi or the psychic reservoir (collectively "somatic psi").[4] Thus, the integration of proof-focused research working to better establish AIR with process-focused studies addressing mediums' experiences of communication with the deceased will begin to provide a more complete picture of both AIR and the possibility of life after death.

Recent Process-Focused Mediumship Research

To date and to the best of our knowledge, there has been no published systematic research addressing modern-day, "nondenominational," American mental[5] mediums' phenomenology pertaining to AIR. It is nonetheless important to recognize that numerous—arguably unsystematic—qualitative studies were conducted during the first

[4] It is, of course, possible that if, for example, mediumship readings and psychic readings are associated with different phenomenological effects, then this may not be the result of the percipient accessing different information sources (i.e., discarnates versus living persons, respectively). Indeed, it is plausible that the phenomenological differences are due to, for instance, demand characteristics, false memory impressions, and the percipient's different expectations.

[5] Mental mediumship (also called "clairvoyant" mediumship) "occurs in a conscious and focused waking state" (Buhrman, 1997, p. 13). In contrast, during trance mediumship, which involves an "unconsciousness of surroundings," "the normal personality is ... completely dispossessed by the intruding intelligence" and the medium "retains little or no recollection of what has been said or done in her 'absence'" (Gauld, 1983, p. 29).

half of the twentieth century[6] and that several investigations of mediums' experiences "from within a variety of academic disciplines" (Cousins, 2008, p. 334) have since then been reported (e.g., Emmons, 2000). Below, we briefly summarize the results from our recent process-focused investigations.

This research evaluated the experiences of certified research mediums (CRMs); that is, participants screened over several months using an intensive multi-step screening and testing procedure (described briefly at http://www.windbridge.org/mediums.htm and in detail in Beischel, 2007/2008). These CRMs remain conscious and aware during readings, and their abilities to report accurate and specific information have been repeatedly demonstrated under controlled conditions in the laboratory. Thus, the sample of participants in our research is not representative of claimant mediums in general or of the extensively observed historical trance and physical mediums, but rather of modern-day, American mental mediums whose abilities have been documented.

One recent process-focused study conducted by Rock and Beischel (2008) aimed to *quantify* the phenomenological differences that arose psychologically during a discarnate reading task versus a control task. It is important to note that this study was not concerned with whether any phenomenological differences were a result of variations in the source of the information (i.e., a discarnate versus a living person), but rather whether phenomenological differences arose psychologically as a result of different task demands. Rock and Beischel administered seven CRMs[7] counter-balanced sequences of a discarnate reading and control condition. The discarnate reading condition consisted of a phone reading including questions about a target discarnate in which only a blinded medium and a blinded experimenter were on the phone. The control condition consisted of a phone conversation between the medium and the same experimenter in which the medium was asked similar questions regarding a living person s/he (i.e., the medium) knew. Mediums' phenomenology during each condition was retrospectively assessed using the Phenomenology of Consciousness Inventory (PCI; Pekala, 1991), a questionnaire designed to quantify different phenomenological elements associated with exposure to a particular stimulus condition.

It was found that the CRMs provided scores for the reading condition that were significantly *higher* than those for the control condition with

[6] Schouten (1994) states: "The first extensive studies of verbal statements of mediums appeared about 100 years ago in the publications of the British and American psychical research societies. These studies were purely descriptive. Hundreds of pages were devoted to transcripts of readings of mediums and discussions of interpretations and the validity of the mediums' statements ... The subjective estimation of the significance of data became less acceptable and was gradually replaced by the application of quantitative and statistical evaluations" (pp. 222–223).

[7] The participants in this study ranged in age from 43 to 54 years (mean = 46.71, SEM = 1.77, median = 44, SD = 4.68) and included six females and one male.

regard to phenomenological elements including Negative Affect, Altered Experience (e.g., alterations in time sense, body image, and perceptions of objects in the external world), and Altered State of Awareness (i.e., one's subjective sense of an unusual state of consciousness). In contrast, the discarnate reading condition scored significantly *lower* than the control condition with regard to: Self-Awareness, Volitional Control, and Memory. Consequently, Rock and Beischel's (2008) quantitative findings allow one to more fully appreciate the phenomenological processes associated with mediumship readings for discarnates.

The results also begin to make reference to the CRMs' experience of the discarnates as autonomous beings. For example, the finding of lower volitional control during the reading condition leads us toward a better understanding of the CRMs' experience of ostensible discarnate communication. Granted, the CRMs were not blinded to the conditions in this study and Rock and Beischel were not making claims beyond the phenomenological differences that arose psychologically according to the task demands of the reading and control condition.

In a subsequent process-focused study, Rock, Beischel, and Cott (in press) began to specifically address the source of the information CRMs receive by *qualitatively* investigating CRMs' experiences of purported communication with discarnates as compared to their experiences during psychic readings for the living in which somatic psi (i.e., telepathy, clairvoyance, and/or precognition, be it "super" or not) was ostensibly used. Six CRMs[8] were e-mailed two open-ended questions in counter-balanced sequences that requested detailed descriptions of the phenomenological effects of: (1) mediumship readings and (2) psychic readings. A thematic analysis using various principles of phenomenological methodology yielded the comprehensive constituent themes and representative verbatim comments from participants regarding mediumship and psychic reading experiences displayed in Tables 1 and 2, respectively.

A comparative analysis of the essential aspects of mediumship readings versus psychic readings revealed several similarities. The *multimodal*, *visual*, *auditory*, *tactile*, and "*just knowing*" themes were considered essential aspects of both the mediumship reading and psychic reading experiences, and the extracted significant statements that constituted these themes are similar. One significant difference, however, is that psychic reading themes tended to pertain primarily to the individual client, whereas mediumship reading themes pertained to the discarnate, the sitter, and other friends and relatives of the discarnate. Indeed, as one CRM stated: "In a psychic reading, the information that comes through usually has to do with life issues and often does not continually make references back to family members."

[8] The participants in this study ranged in age from 44 to 56 years (mean = 48.63, SEM = 1.96, SD = 4.81) and included five females and one male.

Table 1
Comprehensive Constituent Themes and Verbatim Comments for Mediumship Experiences

Comprehensive constituent themes	Verbatim comments
(1) Verificatory "signs" of contact with the discarnate	*Another really exciting communication I have with the deceased is hearing high pitched kind of rings or whines. I have asked for communication and suddenly hear inside my ears, a high ringing sound. I am thrilled to know this is contact.*
(2) Partial "merging" with the discarnate	*It almost feels like my energy is "merging" (often referred to as "blending") with the energy of the discarnate. Sometimes it feels like being in two places at the same time.*
(3) Apparent independence of the discarnate from the medium	*Now you would think being a medium I would want to look and connect with them sitting on the edge of my bed. What really happens is they startle me which makes me freak out!*
(4) Multiple modalities functioning simultaneously (e.g., auditory and olfactory)	*In mediumship readings, communication comes through in a variety of ways, and often it is a combination of methods that go beyond the normal senses.*
(5-8) Visual, auditory, tactile, and olfactory imagery pertaining to the discarnate and their loved-ones	*I often see in my spiritual vision, symbols such as a favorite piece of jewelry.* *I have also heard music inside me for a favorite song the deceased person loved. I have "heard" piano music in my ears or in my mind and had that identified by the sitter.* *I will also go through feelings of how they passed. If a heart attack my heart will beat really fast, drowning I will get lots of fluid in my throat.* *I have smelled violets and then found out Violet was the deceased person's name for the sitter.*

Comprehensive constituent themes	Verbatim comments
(9) The experience of "just knowing" (i.e., the spontaneous manifestation of knowledge about, or connected to, a discarnate without the medium experiencing its acquisition).	*It is just "knowing." When this happens, it is always correct, and can even surprise me. In some cases I just look at them and start saying names and giving validations in rapid fire sequence.*

Table 2
Comprehensive Constituent Themes and Verbatim Comments for Psychic Reading Experiences

Comprehensive constituent themes	Verbatim comments
(1) Multiple modalities functioning simultaneously	*All the above aspects of psychic ability overlap during a reading so that one minute I may be seeing something in my mind's eye while seconds later, I might be feeling something or hearing something.*
(2-4) Visual, auditory, and tactile information pertaining to the client	*I may see beautiful colors in the aura or deep red angry colors or even black if there is a health issue.* *I also have been able to hear things like blood coursing through the veins or circulatory system of a person.* *My hands are sensitive, and I experience the physical feeling of touching a spirit form which is the living person's spirit. I also use my hands to detect health issues in the spirit form. So I can feel cancer with my hands or the clothes someone is wearing, or their hair. This touch and feel kind of thing is communicated back to me spiritually.*
(5) Empathy with the client	*As soon as I hear them say hello I can tell if they are skeptical, open, excited and happy, nervous, sad … I can feel their energy, therefore, know what they are feeling.*

Comprehensive constituent themes	Verbatim comments
(6) The experience of "just knowing"	*I may just suddenly "know" something without any rational explanation.*
(7) Ostensible precognition	*...a psychic reading is like reading a book, which represents your aura and energy field that contains past, present, and future information.*

The similarities between psychic and mediumship experiences are not entirely surprising considering that mediums, in fact, must be employing similar techniques in both discarnate and psychic readings, albeit to receive information from ostensibly different sources. That is, in a psychic reading, the medium uses telepathy to acquire information from the living client's mind, and during a discarnate reading, s/he also uses telepathy to seemingly receive information from the discarnate's mind.[9] In addition, just as the target is "sent" from the sender to the receiver during telepathy studies, the information reported in mediumship readings may be "sent" from the discarnate to the medium. Thus, the two experiences may indeed appear similar even if the two sources are different. Similarities are also apparent between the *empathy* theme of the psychic readings and the *partial* "*merging*" theme of the mediumship readings; that is, both themes included an experience of the target's emotions (i.e., the client and the discarnate, respectively). However, it may be noted that during mediumship readings, CRMs tended to strongly *experience* the discarnate's emotions, whereas during psychic readings, CRMs were merely *aware* of the client's emotions.

There are, however, several themes that emerged during the mediumship analysis but were absent from the psychic readings analysis (i.e., the *olfactory*, *verificatory* "*sign*," and *independence* themes). The finding that these themes were not considered essential aspects of the psychic reading experience is due, at least in part, to the fact that the target entity during psychic readings was a living person rather than a discarnate. For example, the CRMs clearly did not require a verificatory sign of contact from the living client who had solicited the psychic reading. In contrast, the nebulous nature of ostensible discarnate communication calls for corroborative evidence that contact has been established. Furthermore, the *independence* theme may have surfaced during the mediumship analysis and not the psychic analysis simply because it is obvious that the living clients of psychics are independent from the psychics themselves.

[9] We use the term "mind" here in a general sense to refer to the discarnate's ostensible survived consciousness. We are not making claims that a discarnate mind is identical to a living mind.

Finally, the finding that the *precognition* theme was an essential aspect of the psychic reading experience but not the mediumship reading experience emphasizes their different functions. That is, the function of a psychic reading includes conveying to the living client information regarding future events, whereas the function of a mediumship reading is to facilitate discarnate-sitter communication.

These process-focused findings suggest that CRMs have the ability to differentiate between ostensible discarnate communication and their use of somatic psi during psychic readings. Indeed, one CRM succinctly made the following distinction: "a psychic reading is like reading a book ... and a mediumship reading is like seeing a play." Thus, further research is required to better understand the differences that occur in CRMs' phenomenology during mediumship and psychic readings.

The Implications of Process-focused Mediumship Research for the Survival Psi Versus Somatic Psi Debate

As previously stated, historically, parapsychological researchers have been unable to discern whether the accurate and specific information that mediums report comes from the deceased in the afterlife (i.e., survival psi) or instead arises from mediums' use of telepathy, clairvoyance, and/or precognition (i.e., somatic psi). By simply asking mediums about the source of their information, however, it becomes apparent that they experience information arising from those two sources as quite different and easily discernable.

The first study described briefly above (Rock & Beischel, 2008) provided us with a general understanding of mediums' experiences during a mediumship reading task (i.e., ostensible communication with the deceased) versus their experiences during a control task. The finding of lower volitional control during the reading condition speaks to a medium's experiences of a discarnate as a separate individual communicating with him/her. This finding supports the idea that during a mediumship reading, the information "comes in" to the medium rather than the medium "reaching out" to acquire the information.

The second study summarized above (Rock, et al., in press) serves as an important first step in specifically addressing the survival psi versus somatic psi debate through process-focused research. Specifically, the presence of the comprehensive constituent theme "apparent independence of the discarnate from the medium" begins to directly address the differences between CRMs' experiences during mediumship conditions and situations in which information is acquired from the proposed psychic reservoir. The CRMs experience discarnate-based information as coming from independent, volitional beings separate from themselves and not as knowledge obtained from a dormant, inert source.

It is also noteworthy that CRMs are apparently engaged in *super* (somatic)-psi (i.e., retrieving information through more extensive than "normal" clairvoyance, precognition, and/or telepathy with the living) during "regular" psychic readings. Namely, the thematic analysis of mediums' experiences during psychic readings revealed that they receive visual, auditory, and tactile information pertaining to the client; that is, they ostensibly use telepathy and/or clairvoyance to acquire information from the living. The analysis also included their use of reported precognition. However, in the CRMs' descriptions of communication with discarnates, the precognition theme was not present. Granted, this may simply be because it is not possible to report precognitive information about a deceased person's actions in the future either because the consciousness no longer exists (i.e., the survival hypothesis is false) or because of the nebulous nature of the afterlife and our access to it as physically-based beings. As one CRM stated: "One cannot predict the future of a discarnate; unless of course, we'd like to predict what they are going to be doing 'in the afterlife'—a concept that is totally subjective and completely unverifiable." Regardless, the presence of precognition in one condition and not the other does raise an interesting issue worthy of further investigation as it reveals a specific distinction between mediumship and psychic readings.

In addition, the similarities discovered between the CRMs' experiences during mediumship and psychic readings may relate to the CRMs' regular reported contact with "spirit guides," "angels," discarnates known to them, and similar "friendly" ethereal entities. If, for example, the auditory information associated with the living client during a psychic reading is acquired through communication with other-worldly beings, the mediums' experiences might be similar under the two conditions (i.e., discarnate and psychic readings) as the sources are relatively analogous. Though this may be speculative, it is important to entertain all possible explanations for similarities and differences when comparing mediums' experiences during mediumship and psychic readings. Accounting for overlap between the two experiences is essential to our understanding of either.

Furthermore, it is important to keep in mind that the source of information reported during mediumship readings may in fact include combinations of all three proposed parapsychological hypotheses (i.e., survival, super-psi, and psychic reservoir). Indeed, the existence of psi abilities and/or the presence of a cosmic store of information do not preclude the possibility of the survival of consciousness or the capacity of mediums to communicate with the deceased. In the future, a closer systematic examination of the processes mediums use to acquire information—by both mediums and researchers—will lead to a more complete understanding of the source of the anomalous information reported during mediumship readings. In the next section, we propose a future study that may allow researchers to achieve this objective.

The Future of Process-focused Mediumship Research Concerning the Survival Psi Versus Somatic Psi Debate

As stated above, proof-focused mediumship data alone cannot distinguish between the survival psi and somatic psi hypotheses. Mediums, however, claim that they are able to make that distinction based on the nature of their experiences. Future studies aimed at distinguishing between survival psi and somatic psi might include a modified version of Rock and Beischel's (2008) experimental design in which CRMs complete the PCI concerning their experiences during different stimulus conditions: (1) a mediumship reading for a deceased target, (2) a psychic reading for a living target, and (3) a control condition in which no use of somatic psi or discarnate communication is requested. For the first two conditions, the first name of the target would be provided to the CRM but s/he and the participating experimenter would be kept blind to all other details about the targets, specifically whether each target is deceased or living. This may address whether the underlying phenomenological processes associated with discarnate communication are quantitatively different from those used during somatic psi (i.e., telepathy, clairvoyance, and/or precognition).

It would also be useful to standardize the question set across conditions (e.g., not including questions about the cause of death), which would serve to eliminate phenomenological differences due to differential task demands. In addition, this study could include a placebo or "sham" reading condition, whereby mediums are blinded to the fact that they are instructed to communicate with a fabricated, rather than a factual, target. However, if the blind is compromised and the medium determines that the target has been fabricated, then the source of this determination could include telepathy with the living (e.g., the experimenter) or communication with a factual discarnate that is presumably knowledgeable regarding the sham reading condition. Indeed, it would be difficult, if not impossible, to control for which skills or abilities a medium is using to acquire information. It is also possible that if the blind is *not* broken, this may be due to the medium communicating with a mischievous factual discarnate who is masquerading as the fabricated discarnate.

Including a sham condition in the design would be advantageous if the mediums' phenomenology associated with the sham condition is significantly different compared to, for example, the mediumship reading; this, then, may be due to differences between the informational sources (i.e., fictional versus deceased). However, if similarities in phenomenology are seen between the sham condition and *either* the psychic reading condition *or* the discarnate communication condition, due to the blinding issues stated above, this result would not aid in discriminating between somatic psi and survival psi as it is not possible to determine the source of information (i.e., friendly or mischievous discarnate, telepathy or super-psi, other,

etc.) acquired during the sham condition. Consequently, the "slippery" dynamics of mediumship and the unknown limits of either form of psi make any attempt at blinding the medium to the reading condition problematic. In addition, ethical issues arise regarding asking the participants to "open up" to a sham discarnate; because the source of information obtained during AIR is unknown, opening up without a "known" entity with which to communicate may not be psychologically or spiritually safe.

In the future, researchers may wish to forego the sham condition and simply blind the mediums to whether a target individual is alive or deceased and provide instructions requesting that the mediums do not enlist the assistance of entities besides target discarnates during the readings. Differences discovered under these conditions may allow for the further discrimination between mediums' experiences of survival psi and somatic psi.[10]

Conclusion

Although previous evidence for AIR alone cannot differentiate between survival and psi, by adding (1) mediums' spontaneous reports that they are communicating directly with the deceased as well as (2) their alleged ability to differentiate between that communication and their use of somatic psi, and including (3) experimental evidence that the two experiences are in fact different under controlled conditions, it may be possible to arrive at an empirically driven distinction between survival psi and somatic psi. Issues (1) and (2) were addressed by Rock et al. (in press) and may assist researchers in identifying the phenomenology of ostensible communication with discarnates versus the phenomenology of somatic psi. Furthermore, Rock and Beischel (2008) indicated how a modified experimental design may allow researchers to address (3), which may assist in determining the source of the information mediums receive. This determination will, in turn, bring the field closer to addressing the question at the root of mediumship research: Is there life after death?

References

Abussabha, R., & Woelfel, M. L. (2003). Qualitative vs. quantitative methods: Two opposites that make a perfect match. *Journal of the American Dietetic Association*, **103**, 566–569.

Barnard, G. W. (1997). *Exploring unseen worlds: William James and the philosophy of mysticism.* New York: State University of New York Press.

Beischel, J. (2007/2008). Contemporary methods used in laboratory-based mediumship research. *Journal of Parapsychology*, **71**, 37–68.

[10] Investigating these differences in the future may also include using non-phenomenologically based methods such as electroencephalography (EEG), functional magnetic resonance imaging (fMRI), and/or physiological monitoring of participants during psychic and mediumship experiences.

Beischel, J., & Schwartz, G. E. (2007). Anomalous information reception by research mediums demonstrated using a novel triple-blind protocol. *EXPLORE: The Journal of Science & Healing*, **3**, 23–27.

Braud, W. (2005). The farther reaches of psi research: Future choices and possibilities. In Thalbourne, M. A. & Storm, L. (Eds.), *Parapsychology in the twenty-first century: Essays on the future of psychical research* (pp. 38–62). Jefferson, NC: McFarland.

Braude, S. E. (2003). *Immortal remains: The evidence for life after death.* Lanham, MD: Rowman & Littlefield.

Buhrman, S. (1997). Trance types and amnesia revisited: Using detailed interviews to fill in the gaps. *Anthropology of Consciousness*, **8**, 10–21.

Burke-Johnson, R., & Onwuegbuzie, A. J. (2004). Mixed methods research: A research paradigm whose time has come. *Educational Researcher*, **33**, 14–26.

Cardeña, E., Lynn, S. J., & Krippner, S. (2000). Introduction: Anomalous experiences in perspective. In Cardeña, E., Lynn, S. J., & Krippner, S. (eds.), *Varieties of anomalous experience: Examining the scientific evidence* (pp. 3–21). Washington, DC: American Psychological Association.

Clarke, C. J. S. (1995). The nonlocality of mind. *Journal of Consciousness Studies,* **2**, 231–240.

Cousins, W. E. (2008). Ireland the anomalous state: Writing in the Celtic twilight zone. *Proceedings of Presented Papers: The Parapsychological Association 51st Annual Convention*, 333–337.

Crawford, M. J., Weever, T., Rutter, D., Sensky, T., & Tyrer, P. (2002). Evaluating new treatments to psychiatry: The potential value of combining qualitative and quantitative research methods. *International Review of Psychiatry*, **14**, 6–11.

Crick, F., & Koch, C. (2003). A framework for consciousness. *Nature Neuroscience*, **6**, 119–126.

Dechesne, M., Pyszczynski, T., Arndt, J., Ransom, S., Sheldon, K. M., van Knippenberg, A., & Janssen, J. (2003). Literal and symbolic immortality: The effect of evidence of literal immortality on self-esteem striving in response to mortality salience. *Journal of Personality and Social Psychology*, **84**, 722–737.

Emmons, C. F. (2000). On becoming a spirit medium in a "rational society." *Anthropology of Consciousness*, **12**, 71–82.

Fischer, C. (1998). Being angry revealed as self-deceptive protest: An empirical phenomenological analysis. In Valle, R. (Ed.), *Phenomenological inquiry in psychology* (pp. 111–122). New York: Plenum Press.

Fontana, D. (2005). *Is there an afterlife? A comprehensive overview of the evidence.* Oakland, CA: O Books.

Foss, C., & Ellefsen, B. (2002). The value of combining qualitative and quantitative approaches in nursing research by means of method triangulation. *Journal of Advanced Nursing*, **20**, 242–248.

Gauld, A. (1983). *Mediumship and survival: A century of investigations.* Chicago: Academy Chicago Publishers.

Gifford-May, D., & Thompson, N. (1994). "Deep states" of meditation: Phenomenological reports of experience. *Journal of Transpersonal Psychology*, **26**, 117–138.

Hanson, D., & Klimo, J. (1998). Toward a phenomenology of synchronicity. In Valle, R. (Ed.), *Phenomenological inquiry in psychology* (pp. 281–307). New York: Plenum Press.

Irwin, H. J. & Watt, C. A. (2007). *An introduction to parapsychology* (5th ed.). Jefferson, NC: McFarland.

Kelly, E. W. (in press). Mediumship and survival research. *Proceedings of the 2005 Parapsychology Foundation International Conference.*

Matsu-Pissot, C. (1998). On the experience of being unconditionally loved. In Valle, R. (Ed.), *Phenomenological inquiry in psychology* (pp. 321–334). New York: Plenum Press.

O'Keeffe, C., & Wiseman, R. (2005). Testing alleged mediumship: Methods and results. *British Journal of Psychology*, **96**, 165–179.

Osis, K. (1966). Linkage experiments with mediums. *Journal of the American Society for Psychical Research*, **60**, 91–124.

Pekala, R. J. (1991). *Quantifying consciousness: An empirical approach.* New York: Plenum Press.

Pekala, R. J., & Cardeña, E. (2000). Methodological issues in the study of altered states of consciousness and anomalous experiences. In Cardeña, E., Lynn, S. J., & Krippner, S. (eds.), *Varieties of anomalous experience: Examining the scientific evidence* (pp. 47–82). Washington, DC: American Psychological Association.

Rock, A. J. (2006). Phenomenological analysis of experimentally induced visual mental imagery associated with shamanic journeying to the lower world. *International Journal of Transpersonal Studies*, **25**, 45–55.

Rock, A. J., & Beischel, J. (2008). Quantitative analysis of research mediums' conscious experiences during a discarnate reading versus a control task: A pilot study. *Australian Journal of Parapsychology*, **8**, 157–179.

Rock, A. J., Beischel, J., & Cott, C. C. (in press). Psi vs. survival: A qualitative investigation of mediums' phenomenology comparing psychic readings and ostensible communication with the deceased. *Transpersonal Psychology Review*, **13**.

Roy, A. E., & Robertson, T. J. (2004). Results of the application of the Robertson-Roy protocol to a series of experiments with mediums and participants. *Journal of the Society for Psychical Research*, **68**, 18–34.

Schmeidler, G. R. (1958). Analysis and evaluation of proxy sessions with Mrs. Caroline Chapman. *Journal of Parapsychology*, **22**, 137–155.

Schouten, S. A. (1994). An overview of quantitatively evaluated studies with mediums and psychics. *Journal of the American Society for Psychical Research*, **88**, 221–254.

Sudduth, M. (2009). Super-psi and the survivalist interpretation of mediumship. *Journal of Scientific Exploration*, **23**, 167–193.
Thomas, C. D. (1932–1933). A consideration of a series of proxy sittings. *Proceedings of the Society for Psychical Research*, **41**, 139–185.
West, D. J. (1949). Some proxy sittings: A preliminary attempt at objective assessment. *Journal of the Society for Psychical Research*, **35**, 96–101.

The Windbridge Institute
3938 E. Grant Rd. #407
Tucson, AZ 85712, USA
beischel@windbridge.org

Acknowledgments

Portions of the research described were supported by the Peter Hayes Fund.

Abstracts in Other Languages

Spanish

SOBRE EL DEBATE DE SOBREVIVENCIA VERSUS PSI A TRAVÉS DE ÉNFASIS EN LA INVESTIGACIÓN DE PROCESO DE LA MEDIUMNIDAD

RESUMEN: Más de un siglo de investigación concluye que médiums talentosos pueden presentar información certera y específica sobre seres queridos que han muerto (llamados desencarnados) relacionados a personas vivas (llamados asistentes) durante la recepción de información anómala (RIA); esto es, sin tener conocimientos previos sobre los desencarnados o los asistentes, en ausencia de información sensorial, y sin en uso de decepción. Sin embargo, la investigación histórica que ha enfatizado la prueba no ha sido enfocada hacia cuales mecanismos parapsicológicos están involucrados en la RIA con médiums. Los datos de por si apoyan hipótesis múltiples que incluyen los modelos super-psi y la reserva psíquica ("psi" colectivo) al igual que la hipótesis de la sobrevivencia de la conciencia (o simplemente "sobrevivencia"). Sin embargo, al limitar la investigación a estudios que enfatizan prueba, los investigadores han ignorado importantes procesos fenomenológicos de mediumnidad subyacentes a la RIA y como estos procesos pueden estar relacionados a la hipótesis de la sobrevivencia. La investigación de las experiencias de los médiums enfatizando proceso podría llevar a un mejor entendimiento de la fuente de información de los médiums durante la RIA. Este artículo resume brevemente los resultados de estudios recientes enfatizando proceso, las experiencias de médiums mentales americanos durante supuesta comunicación con desencarnados. También sugerimos areas para futuros estudios enfatizando el proceso para considerar de forma más completa la pregunta básica de la investigación de médiums: ¿Hay una vida después de la muerte?

French

REGLER LE DEBAT SURVIVALISTE VERSUS PSI A TRAVERS LA RECHERCHE ORIENTEE SUR LES PROCESSUS DE LA MEDIUMNITE

RESUME: Plus d'un siècle de recherche sur la médiumnité a conclu que des médiums doués étaient capables de rapporter des informations précises et spécifiques à propos des proches décédés (appelés désincarnés) et des personnes vivantes (appelées séanciers) au moyen d'une réception anomale d'information (AIR); c'est-à-dire sans aucune connaissance antérieure sur les désincarnés ou les séanciers, en l'absence de feedback sensorial, et sans utiliser des moyens frauduleux. Cependant, le corpus historique de recherche orientée vers la preuve ne permet pas de savoir quels mécanismes parapsychologiques sont impliqués dans les AIR par des médiums. Les données en elles-mêmes permettent de soutenir de multiples hypothèses dont les modèles (dits « psi ») du super-psi et du réservoir psychique aussi bien que l'hypothèse de la survie de la conscience (dit simplement « survivaliste »). Toutefois, en restreignant la recherche aux études orientées vers la preuve, les chercheurs ont négligé d'importants processus de la phénoménologie médiumnique sous-jacent à l'AIR, ainsi que la façon dont ces processus puissent renvoyer à l'hypothèse de la survie. L'investigation orientée vers les processus des expériences des médiums pourrait mener à une meilleure compréhension de la source de l'information que les médiums rapportent durant l'AIR. Cet article résume brièvement les résultats de récentes études orientées vers les processus avec des médiums mentaux de l'Amérique contemporaine durant d'apparentes expériences de communication avec des désincarnés. Nous suggérons également des zones à explorer dans les futures études médiumniques orientées vers les processus pour mieux régler la question à la source de la recherche médiumnique : y a-t-il une vie après la mort ?

German

ZUR BEHANDLUNG DER ÜBERLEBENS- VERSUS PSI-DEBATTE MITTELS DER PROZESSORIENTIERTEN FORSCHUNG MIT MEDIEN

ZUSAMMENFASSUNG: Eine mehr als hundertjährige Geschichte der Erforschung der Medialität ist zum Schluß gekommen, daß begabte Medien in der Lage seien, genaue und spezifische Angaben über verstorbene Angehörige (als „Jenseitige" bezeichnet) lebender Personen (als „Sitzungsteilnehmer" bezeichnet) mittels einer anomalen Informations-Rezeption (AIR) zu machen, das heißt ohne vorheriges Wissen über die Jenseitigen oder Sitzungsteilnehmer, beim Fehlen eines sensorischen Feedback und ohne die Verwendung von Täuschungstechniken. Dieses historisch gewonnene Material eines beweisorientierten Forschungszugangs schliesst jedoch nicht die Frage ein, welche Rolle parapsychologische Mechanismen bei der AIR seitens der Medien spielen. Die Datenlage an und für sich genommen läßt mehrfache Hypothesen zu – einschließlich des Super-Psi-Modells und des

Modells eines parapsychischen Reservoirs (zusammengefaßt als “Psi”) wie auch die Hypothese vom Fortleben des Bewusstseins (einfach „Überleben“ genannt). Wenn die Forschung auf beweisorientierte Untersuchungen begrenzt wird, werden von den Untersuchern jedoch wichtige Prozesse der Phänomenologie der Medialität vernachlässigt, die der AIR zugrundeliegen und wie diese Prozesse mit der Überlebenshypothese in Beziehung stehen. Diese prozeßorientierte Untersuchung medialer Erfahrungen könnte zu einem besseren Verständnis des Ursprungs der Information führen, über die Medien während der AIR berichten. Der Artikel fasst kurz die Ergebnisse prozeßorientierter Untersuchungen der Erfahrungen moderner amerikanischer Medien während derer scheinbaren Kommunikation mit Jenseitigen zusammen. Wir schlagen auch Forschungsgebiete für künftige prozessorientierte Untersuchungen mit Medien vor, um die Frage umfassender zu behandeln, die der Forschung mit Medien zugrunde liegt, nämlich: Gibt es ein Leben nach dem Tod?

ALLEGED ENCOUNTERS WITH THE DEAD: THE IMPORTANCE OF VIOLENT DEATH IN 337 NEW CASES

By Erlendur Haraldsson

ABSTRACT: Personal encounters with the dead are reported by 25% of Western Europeans and 30% of Americans. Three hundred thirty-seven Icelanders reporting such experiences were interviewed at length. Ninety percent of them reported sensory experiences (apparitions) of a deceased person; 69% were visual, 28% auditory, 13% tactile, and 4% olfactory. Fewer than half of the experiences occurred in twilight or darkness. In half of the cases the experiencer was actively engaged or working. Disproportionately prominent were apparitions of those who died violently and crisis apparitions observed close to the time of death of the person who was perceived, although in the majority of cases, the percipient did not know that the person had died. Reported mode of death and the identity of the deceased persons were verified by checking official records. A fair number of collective experiences were reported, some of which were verified by other witnesses.

Keywords: apparitions, encounters with the dead, surveys, Iceland, violent death, crisis apparitions, hallucinations, after-death communications

Over a century ago the founders of the Society for Psychical Research (SPR) conducted the first large systematic study of apparitions (Gurney, Myers, & Podmore, 1886; Sidgwick and Committee, 1894). In their meticulously thorough investigation they found that apparitions were reported by so many persons that they concluded that they are experienced by people who are normal and sane.

Their second major finding was that some of these "hallucinations," or phantasms, as they were called, seemed to have some factual basis. Some of them apparently coincided with events that were taking place somewhere else. Such "veridical hallucinations" were distinguished from the bulk of hallucinations that correspond to no factual event or situation.

These "veridical hallucinations" seemed to be more than "just hallucinations." They were related to an external event and hence meaningful to the percipient. In cases of this kind people have a vision of someone they know, and they learn later that this person unexpectedly died at the time of their vision or impression. In veridical hallucinations some information unknown to the percipient is apparently passed on to the recipient in an inexplicable manner. Some veridical hallucinations are apparently collective, that is, experienced by more than one person at the same time.

Since the early SPR studies, veridical hallucinations have remained a cause of debate and speculation. Researchers as well as theorists have

argued about how they should be interpreted and how much faith can be placed in the accuracy of the case reports (Myers, 1889a, 1889b, 1903; Podmore, 1889). The best of the cases involving persons at the time they are dying or shortly after their death have remained intriguing. Some researchers have seen in them a possibility for empirical research into the question of continued existence after bodily death (Gurney & Myers, 1887–88; Stevenson, 1982).

In 1975 I included the following item in a representative survey in Iceland ($N = 902$) dealing with paranormal and religious beliefs and experiences: "Have you ever perceived or felt the nearness of a deceased person?" Of the respondents, 31% replied yes (36% of the women, 24% of the men).

A representative U.S. survey ($N = 1467$) by McCready and Greeley (1976) showed that this high percentage of reported contact with the dead is not specific to Iceland. McCready and Greeley asked: "Have you ever felt that you were really in touch with someone who had died?" Twenty-seven percent responded in the affirmative. Later this item was included in the European Human Values Study (Haraldsson, 1985; Haraldsson & Houtkooper, 1991). In Western Europe, about 25% of the respondents reported having felt contact with the dead. In the U. S. in 1987, the figure had risen to 41% (Greeley, 1987). Even in Communist China, 40% of university students reported experiences of contact with the dead (McClenon, 1988). None of these surveys, however, describe the content of the experiences.

Since the early British researchers made their pioneering and monumental studies, no major attempt has been made to collect a large number of cases of apparitions of the dead in such a way that they would lend themselves to detailed analyses. There have been studies of specific groups, such as the Rees's (1971) study of widows and widowers, and the Osis and Haraldsson (1986) study of terminally ill patients. Guggenheim and Guggenheim (1995) collected a large number of encounters with deceased relatives or loved ones. Arcangel's (2005) large-scale survey of "afterlife experiences" emphasizes their effects on percipients.

This paper deals with a project that took several years to complete. In 1974 we conducted a representative survey of psychic experiences in Iceland ($N = 902$) using a random national sample (Haraldsson, 1985). Thirty-one percent reported encounters with the dead. An earlier paper (Haraldsson, 1988–1989) deals with 100 cases of encounters with the dead from this 1974 survey. The cases were from persons living in the Reykjavik and Akureyri areas and the conclusions were based on face-to-face interviews. The present paper reports 337 additional cases.

What is the nature of these encounters reported by a substantial part of the population? How are the deceased persons perceived? What sensory modalities are involved? Do these experiences mostly occur in dark and gloomy surroundings, as folk theory implies? Do they primarily occur when people are grieving their loved ones, as Rees's data suggest, or when resting or not fully awake, as suggested by Tyrrell (1953)? Who are the deceased?

Do they share specific characteristics? Do we find veridical apparitions? How strong is the evidence for collectively observed apparitions?

Method

Participants

We obtained cases from 307 experiencers. In addition, we included 30 cases from the 1974 random survey; these are of persons who lived too far away from Reykjavik and Akureyri to participate in face-to-face interviews in the earlier study.

Among the total 337 percipients, 186 were men and 151 were women; 29% had attended only primary school, 58% had attended high school, and 13% had attended college. Their ages varied greatly: 11% were 29 or younger, 11% were 30–39, 17% were 40–49, 25% were 50–59, 18% were 60–69, and 17% were older than 69. Sixty-four percent of the participants were from the greater Reykjavik area, 21% from towns in other parts of Iceland, and 15% from rural areas. These divisions reflect the approximate population of Iceland around the time the original data were gathered.

Questionnaire

Thirty questions dealt with the content and circumstances of the experience: the sensory modality through which the deceased was perceived, whether the deceased was perceived partially or fully, how the deceased appeared and disappeared, the lighting conditions, the time of day, how long the experience lasted, how long ago it occurred, how real the occurrence seemed, and so on.

Twenty items dealt with the perceived entity: whether the apparition was identified by the experiencer, whether the apparition was related to the experiencer, what were the sex, age at death, cause of death, and life circumstances of the deceased, and so forth.

Thirty-six questions concerned the experiencers directly: what they were doing at the time of the encounter, their physical and mental state, whether they had been grieving over the deceased, whether they were alone or with others; if others were present, whether they made the same observations, what effect did the encounter have on the experiencers, what was their prior belief in the paranormal, and so on. For an English translation of the 86-item questionnaire, see the author's homepage (www.hi.is~erlendur).

Procedure

In 1980–1981, a short one-page questionnaire and a self-addressed envelope were placed in five popular magazines: two for fishermen, sailors, and the fishing industry (5,800 subscribers); two for people interested in

spiritualism, spirituality, theosophy, and new religious movements (3,000 subscribers); and one that is widely read by people living in the countryside. We asked: "Have you ever in a waking state personally perceived or felt the presence of a deceased person?"

If the answer was yes, the respondents were asked if they had ever seen an apparition, heard the voice of a deceased person, sensed a smell, felt a touch, or in some other way felt the presence of a dead person. Then we asked for name, address, and phone number.

We received about 700 questionnaires with positive responses. Some of the questionnaires dealt with cases that obviously did not meet our criteria, such as dreams or experiences visiting mediums. They were immediately excluded. If respondents had more than one experience of the dead, they were asked to report which one seemed to be the most impressive. Some secondary cases were recorded, but they are not included in the present analyses.

In-depth interviews were conducted over the telephone and tape-recorded; most of them were in 1980–1981, but they continued up to 1986. The participants were interviewed in the order in which their questionnaires came in, until our resources were exhausted. First, we asked each respondent to give a detailed description of his or her most memorable experience, and then we went through all the relevant questions in a longer questionnaire we had developed for the interview. Our plan was to get an approximately equal number of cases from men and women. A final review of the individual cases revealed that more cases from women had to be discarded than from men, because after the interview these cases were found not to fulfil our criteria. Thus we had more cases reported by men than by women for the analyses.

Checks for Accuracy

Survey data are prone to several weaknesses, because often few or no attempts are made to check the accuracy of the respondents' statements. Furthermore, reports may be distorted for reasons such as faulty observation or recall, and changes in memory over time. We used three methods to check for accuracy:

1. At the University of Iceland, excellent records have been kept for over a century concerning the whole Icelandic population, including dates of birth and death and the cause of death. We obtained permission to check against these files the names of the deceased, as well as their year of birth and death as it was reported to us. We found no evidence of fictitious persons or other signs that we were being purposely misinformed.

2. From the same records we checked the accuracy of the participant's report of the cause of death. If the respondent did not know whether the person had died from disease or suffered a violent death, the cause of death was inserted into our data file.

3. Whenever our interviewees reported that another person had been present and had shared their experience, we spared no effort to trace and interview the second observers, and we carefully compared the independent testimonies.

Otherwise, we accepted the statements of our respondents at face value.

Results

Sensory Modalities of the Experiences

The alleged encounters with the dead came in a great variety of forms. Ninety percent of them were sensory: visual, auditory, tactile, olfactory, or multiple modalities. In 10% of the cases there was only a vivid sense of presence (see Table 1).

Table 1
Sensory Modalities of Reported Encounters With the Dead

	Number of cases			
	One modality	More than one modality	Total	% of $N = 337$
Visual	167	67	234	69
Auditory	36	57	93	28
Tactile	12	32	44	13
Olfactory	13	1	14	4
Only vivid sense of presence			33	10

Visual apparitions dominated, as they were 69% of the cases. The content varied widely. Four examples follow:

> This happened three years ago. I sat in a chair in my room and was reading. Then I looked up and saw my deceased grandmother standing in front of me, as fully alive. I told my mother about this the following day. She said, "That is nice, it was her birthday." I had not remembered it. (5018)

> I had recently started working in a factory when one day I see a man walking at the further end of the machine at which I was working. He walks up to a wall near which the machine was placed and back. I went to see who the man

> was but found no one. When I told my coworkers about this experience and described the man to them, they were sure that this had been a ghost which some others had also seen. It was the former director of the company who had committed suicide. (822)

> I brought my wife to the hospital in February 1960. She had been sick for several weeks. I visited her that same evening again and she was dressed in a blue nightgown. I had expected to see her again the following day but she died in the night. The next day my daughter-in-law comes to select a gown for her burial. A day later I come home late and take out the psalm book to select the psalms to be sung at her funeral. Then the astounding thing happened. My wife suddenly stands in front of me, bathed in white light of an oval form. I see her very distinctly and vividly as she holds both hands around the collar of her nightgown, not that one I saw her in when I saw her the last time, but a pink fancy gown she used only rarely. She held the collar together with both her hands under her cheek. I became startled and then she disappears. I had not known what gown my daughter-in-law had selected for her but discovered later that it was exactly this dress. (2172)

In 89% of the visual cases, the apparition was seen through open eyes. In a few cases (7%), the percipient saw the deceased person as if by the mind's eye, as a vivid image in the mind. In a few other cases (4%), the respondent was not sure if the apparition was perceived internally or externally, as in the following:

> As a young girl I rented a room here in Reykjavík. It had been a storage room for fire wood and no one had lived in it before me. I am not psychic, but bit by bit I began to realize there was a person in the room, someone friendly and it was a man. As winter came I could detect what he looked like, slowly it became clear to me. It did not happen suddenly.... The man was pleasant and I felt he was always asking me to pray for him. Then one rather cold evening, I had turned the lights off and faced the wall ready to go to sleep. Sometimes I would cover myself with two blankets, but now I distinctly felt that I was being covered, just like a mother covers a child. It was the man tending to me, but at the same time he constantly asked me to pray for him. I didn't hear any words; it was rather like a message was being pressed into me; that is how I sensed it.

> I neither heard nor saw anything. I sensed it like this … I don't know how to describe it. I understood the message although it was not put in words. Once when paying the rent the landlord asked jokingly if I had ever noticed anything in the room. There was a silence, so his wife scoldingly said to him that one should not ask questions like that. I thought it best to tell it like it was and let them have a laugh on me and said, "Why yes, there is a man in my room, a young man, but he isn't unfriendly." The landlord seemed startled and said, "Why do you say that?" "Well I'm not quite sure ... but he is friendly, he looks after me more than anything, there is no hostility between us." The landlord asked if I could describe this man and so I did. He was rather distinct, had curly hair and unusual characteristics. Although they were not outstanding he was easy to describe. The landlord was very surprised, and looked stunned. He asked if I knew what had happened in the room. I said I had no such knowledge but asked if the room had not been used for storage of firewood. The landlord answered, "Yes it was, but a man committed suicide in that room a long time ago and your description fits him perfectly..." My description was in accordance with what the landlord had been told about the man when he bought the house. I wasn't upset by all of this. I felt he was friendly, pleasant and pleading, and that he was grateful that I had prayed for him. (2172)

Auditory cases were the second most common: 28% of the experiences were auditory and 66% of these were of human voices. Voices alone without other modalities were rare, but here are two such cases, both reported by fishermen who were being warned of impending danger:

> This happened when I was a teenager. I was alone fishing on a small boat. Suddenly I heard a voice that says I should leave the fishing line and row ashore. I heard this quite clearly. It was said to me in a commanding tone. I do as ordered, I do not know why, but I found it so odd. Just as I am about to arrive at the harbour, suddenly there sweeps over a violent storm. I barely managed to dock. I did not recognize the voice, but later I connected it with my brother who had recently drowned. (7028)

> I was on a fishing boat from Stykkisholm. We were out in the bay in the storm and rain and had just laid our nets and should be wakened at six the following morning. I slept in

> the narrow cabin with the others and one man was on duty in the deckhouse. About five in the morning I and some others wake up as someone calls "rise." I went up to the cabin door and called to the man in the deckhouse and asked if he had been calling. No, he said, it was not time for that. I went back to my bunk, and was not yet asleep when again there was a shout, "rise, are you not going to get out of bed?" It was as if someone was calling loudly from the cabin door some two meters away. Everyone woke up and jumped out of bed. That surprised me; generally the crew was not that quick. I told the machinist to run aft and start the donkey engine to haul in the nets. As the machinist went down to the engine room he saw the dynamo catching fire. He barely managed to disconnect from the electricity so that it came to a halt. I am convinced if he had gone later we would have caught fire and there would have been a fatal explosion in the engine room. I did not recognize the voice. It called in a typical seaman fashion. I had reasons to suspect that it was my deceased grandfather. (7542)

Fairly common (30%) among the auditory cases were noises from various human activities that had been typical for a particular deceased person.

> Shortly after our father died I came to his house with my brother. We knew that there was nobody in the house and then we heard the old man at his desk. He was walking around, opened the door and closed it again. Both of us stopped and listened when we entered and then I remarked, "I guess there is no doubt who is up there." "No there is no doubt about it," my brother replied. Both of us went upstairs, no one was there. We had heard this so clearly. He was 85 years when he died, walked slowly you know, had the typical old man's way of walking. (2198)

We contacted the respondent's brother, who said he remembered clearly that this event had taken place, but he could give no details. When we read the above account to him, he said it all came back to him, and he confirmed everything in his brother's account.

Thirteen percent of the cases were tactile, involving a touch sensation. Three quarters of these tactile cases involve two or more modalities. Here is one involving only the sense of touch:

> My mother and my father-in-law died only a few months apart and I sensed one of them, I think it was my mother. I sat at a table and was working when I felt someone come up behind me, bend down and tightly grab my shoulder. At first I naturally thought it was someone in the household, I looked back but did not see anyone. Then I realized what it was ... it was a cold and tight grip, and it was not from anyone in the household. In a strange way I was astounded, sort of jumped up when I realized what it was, and I thought this would intimidate the person who was trying to comfort me, someone who was trying to get close to me because I was probably a little depressed. I felt as if someone had come to comfort me.... I never had an experience like this again. (2062)

This case combines vision, hearing, and touch:

> The night after my husband died I could not sleep, was at home in my bed and very lonely. Suddenly I sensed him standing by my bed. He seemed to be covered in something like mist. I saw him and felt his hand as he stroked my head and he recited part of a well-known poem that was about how good it was to rest and then wake up one day surrounded by eternal joy. I felt quite differently after this. (2090)

The olfactory cases were the fewest, only 4%. Some olfactory cases have very interesting features, although they have generally not been given much attention in the literature. Here are three olfactory cases:

> I lived in Sandgerdi where we had just bought a house two months ago. I was alone in the house. My husband was out working. Suddenly I see that a man enters through the front door and goes to the kitchen. This happened suddenly and was over. I then felt a strong smell of liquor. I never use alcohol. Well, then my husband comes home, and he says, "Who has been here?" Nobody, I tell him. "Oh, there is such a strong smell of liquor." Yes, I say, but nobody has been here.... The next day my husband comes home for supper and says, "No wonder there was a liquor smell here yesterday.... Erlingur from whom we bought the house was missing in Siglufjord yesterday…." He had been quite drunk and it was feared that he had fallen into the harbour and drowned. Two weeks later his body was found floating in the harbour. When this incident occurred, we

> had no idea about what had happened. My husband is now deceased. (2116)

> I was getting out of bed and then smelled a particular perfume that my wife always used. She had died some time back and this smell had no normal cause or explanation that I was aware of. My wife was making me aware of her to comfort me. (6001)

> My niece died from lung cancer. Most people with serious lung or digestion diseases have a bad odor, the smell of something rotting. One Sunday morning, some time after she passed away, I smell this stink very clearly in the kitchen where I was working. I look around to see if something in the kitchen could cause this but found nothing. Not an hour had passed when her husband comes unexpectedly to visit. I considered this clearly to be her odor. I was not thinking about her, because some time had passed since her death. I do not remember the date, but I presume it was close to a year between these events. I felt it was exactly the smell she had after she became very ill.... I only first thought of her when her husband arrived. (5058)

About 10% of the experiencers reported a case that consisted solely of a vivid sense of presence and hence does not technically fall under the category of apparitions. They were included because our purpose was not only to study apparitions but also to uncover what kinds of experiences tend to be interpreted as encounters with the dead. Three such cases follow:

> I have had this kind of experience a few times. One of them is especially memorable. My eye had been bothering me for quite some time, I had something in my eye and I was quite uncomfortable. Then one morning, I was awake with my eyes closed when I felt my mother, who was deceased, standing at my bed. She bent over me and I thought I could feel her breathing. I was well awake but did not want to open my eyes because I felt sure I would not see her. I am sure she was there and was checking how my eye was. I felt her bend down all the way to my face. (2000)

> I had just woken up. A deceased woman, whom I knew very well, came to me and took me in her arms. This perception only lasted a few moments. When I was talking about this

> that same morning, my daughter said, "I also dreamt about her last night." (2013)

> I was out in the country and going to visit my grandfather. I was having a pleasant trip, stayed at Blanda for two nights, or rather was going to. I was not in any hurry. All of a sudden I sensed my grandfather right there with me. I instantly knew that he had passed away, went to the post office and called. It was confirmed that he had died the day before. (2015)

It is evident that some of the cases in this section involve more than one modality; in fact, 22% of them do: visual and auditory (10%); visual, auditory, and tactile (6%); visual and tactile (4%); and auditory and tactile (2%).

Conditions Under Which the Encounters Occurred

Our collection of cases does not confirm the popular belief that apparitions are mostly seen in darkness or twilight. Half of the experiences (52%) occurred either in daylight (36%) or full electric light, 33% in twilight, 10% in darkness, and some 4% under variable conditions.

Another hypothesis, that of Gurney (Gurney, Myers, & Podmore, 1886), is that apparitions tend to occur when the mind is not actively engaged and in a restful state. He adds, however, that a telepathic impression may have reached the experiencer before the apparitional experience takes place. Thus, this hypothesis may be difficult to test. Others, such as Tyrrell (1953), have argued that apparitional experiences are more prone to occur in drowsy or dissociated states, when external stimulation is likely to be minimal.

Table 2
Conditions Under Which the Encounters Took Place, by %

Experient physically active	49	Daylight, full electric light	52
Resting	22	Twilight	33
Falling asleep	7	Darkness	10
Awakening	16	Varied, unknown	4
Unclear if sleeping or awake	5		

In half of our cases (49%), the experiencer was working or actively engaged in some way, which gives little support to the above speculations. An additional 22% were at rest. Still, a sizable number of the encounters (28%) occurred just prior to falling asleep or upon awakening, many of them quite impressive. The following case happened immediately upon awakening to a man working on a fishing vessel.

> This happened during the summer of 1966. I was somewhere between sleeping and waking, when I get wide awake. I see a man at the stove of the cabin, a young man who was stooping over it. He was doing something there. I recognized that this man was not a member of the crew. I was going to check this further but then he disappeared. Later I got the information that he had got burnt inside the cabin. He did not get burnt to death, he suffocated in smoke. I remember so clearly that he was wearing a blue sweater and a scarf around his neck. My description fit what I later learnt about him. (7018)

In the thanatology literature, grief has been found to be a major cause, if not *the* cause, of apparitional experiences. Only 21% of our experiencers were in a state of grief when they experienced an identifiable apparition.

To sum up, apparitional experiences occur under a variety of circumstances, and there are indications that the state of mind of the percipient plays only a minor role in the occurrence. These findings give some support to the theories of Myers (1903), Hart (1959), and others that these encounters—mostly apparitions—may take place relatively independently of the state of the person who has the experience.

Who Were the Deceased?

Most of the encounters (84%) were with persons who were recognizable to the respondents. Almost half (46%) of the deceased figures were related to the experiencers, 24% were acquaintances or persons slightly known to them, and the rest (30%) were strangers, some of whom were identified through information received after the experience occurred (see Table 3).

Table 3
Who Were the Encountered Dead?

	Number of cases	%
Relatives	155	46
Close friends	27	8
Coworkers	9	3
Acquaintances	45	13
Strangers	100	30

Unexpectedly, the apparitions were predominantly male (67%). This surprising dominance of males is remarkably uniform in

the experiences of both male (68%) and female (67%) experiencers. It confirms the results of our previous representative survey (Haraldsson, 1988–1989), in which 77% of both sexes reported encounters with males and only 23% with females. Has this sex difference been found anywhere else? It has. Schouten (1979) found in his analysis of the cases in *Phantasms of the Living* that involved death, 63% of the apparitions (target persons) were male and 37% female. Very similar figures are found for both male and female percipients. It is interesting that 67% of the cases in *Phantasms of the Living* are phantasms of the dead or dying. So why do men so dominate the scene? We will discuss this issue further below, when we look at another characteristic, namely, the cause of death of the perceived person.

This predominance of males is also reflected in the kinds of relatives that are predominantly encountered. For example, 43 percipients encountered their father, whereas only 22 encountered their mother; 21 perceived their grandfather and 16 their grandmother; 18 widows perceived their deceased husband, whereas only 9 widowers perceived their deceased wife. Regarding the last set of figures, it should be borne in mind that there are always more widows than widowers.

Encounters With Those Who Died Violently

First, a case of an encounter with a person who committed suicide:

> Jacob was a patient in a sanatorium where I worked. He was sometimes depressed, and I tried to brighten his stay with a bit of humour. One day I had talked to Jacob that he should visit us because he came from the same county as my husband and they would enjoy talking about the people from there. He says yes to that, is glad, and I say to him: "You promise to come tomorrow." "Yes, yes, I promise," he says. During the night, I wake up, and all strength is like taken away from me. I am unable to move. Suddenly I see the bedroom door opened and on the threshold stands Jacob, with his face all covered with blood. I look at this for a good while unable to speak or move. Then he disappears and I felt as if he closed the door behind him. I became my normal self, call my husband and tell him about the incident: "I can swear that something has happened at the sanatorium." I telephone in the morning and ask about if everything is not alright with Jacob. "No," said the nurse, "he committed suicide this night." (5076)

We interviewed the husband, who told us that his wife had awakened him in the middle of the night and told him about what she had seen. They did not

know Jacob's fate until morning. The percipient, whom we interviewed in 1982, did not know Jacob's surname. Through further inquiries in 2002, we were able to fully identify Jacob and know more about the circumstances of this incident. On the morning of October 8, 1962, Jacob was missing from his room. The police were called and a few hours later he was found drowned some hundred yards downstream from a walking bridge over a river close to the sanatorium. The post-mortem report declared the cause of death as "suicidum submergio," that is, suicide by drowning. In the report, it is written that there were "two large wounds on his head and the cranium much broken." This fits the percipient's description that she saw Jacob "with his face all covered with blood." The river is rather shallow, but it flows over sharp rocks of lava that must have caused the severe head injuries. A girl working in the sanatorium and returning home from a dance in the middle of the night had briefly met Jacob. He said he had gotten out of his room through the window. As he disappeared into the darkness, he asked the girl to give greetings to his wife.

The cause of death was known in 79% of our cases and verified by checking official records. In 70% of the cases in which the cause of death was known, the person had died naturally; in 30% of the cases the person had died violently (accident 23.87%, suicide 4.49%, and murder 1.50%). Those who had died by accident, suicide, or murder, were much more likely to be perceived than those who had died by disease. A prominent characteristic of apparitions of the dead is the relatively large number of people who died violently.

Table 4
Comparison of Causes of Death of 267 Persons Who Appeared to Our Perceivers

Cause of death	*N*	Apparitions %	Population 1951–1970 %
Disease	187	70.03	91.26
Accident	64	23.97	7.15
Suicide	12	4.49	1.51
Murder	4	1.50	0.08
Unknown	70	--	

Note. From the Icelandic population who died in 1951–1970; data from the National Registry

The median year of death of the persons who appeared to our percipients was around 1961. Hence, we compared our data with the causes of death for the years 1951–1970. During this period, 8.74% of all deaths had a violent cause: accidents 7.15%, suicides 1.51%, and murders 0.08%.

Table 4 allows a comparison between the cause of death in our collection of cases and in the general population. The percentage of violent deaths in our cases is 3.46 times the percentage of people in Iceland who actually die violently, according to official statistics. Evidently, the cause of death does matter when it comes to encounters with the dead.

Are there any comparable findings? Stevenson (1982) calculated the proportion of violent deaths among the 314 apparitional cases in *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886). He found that the mode of death was violent in 28% of the cases in which the cause of death was known. This is remarkably close to our 29.97%. In both case collections, a violent death seems to make the appearance of an apparition more likely than a natural death. These findings raise important questions: Do apparitions of those who suffer a violent death have an invasive character? We will get back to this later.

The cases of violent death show some interesting and striking characteristics that seem not to have been duly recognized. Also, encounters with persons suffering a violent death seem more independent of the relationship to the percipient than do apparitions of those who die naturally. This is evident from the fact that appearances of those who suffer a violent death are significantly more likely to be of strangers or near-strangers to the percipient than appearances of those who die by disease, $\chi^2(4, N = 187) = 35.74, p < .001$. A glance at Table 5 reveals that the more remote the relationship between the percipient and the appearing person, the more likely it is that the appearing person suffered a violent death; and vice versa, the closer the relationship between the percipient and the deceased, the more likely it is that the deceased died by disease.

Table 5
Causes of Death and Relationship to the Perceiver

	Disease	Violent
Relatives	127 (82%)	28 (18%)
Friends	20 (77%)	6 (23%)
Coworkers	4 (44%)	5 (56%)
Acquaintances	22 (54%)	19 (46%)
Strangers	14 (39%)	22 (61%)

$\chi 2 = 35.74, df = 4, p = .0000$

We also asked our interviewees if their relationship to the deceased was very close, somewhat close, slightly close, or not close at all. The data are presented in Table 6. The figures show that the closer the relationship, the more likely the appearer is to have died by disease;

conversely, the more remote the relationship, the likelier it is that the death was violent, χ^2 (3, N = 187) = 18.23, $p < .001$. This suggests that those who die violently appear to those close to them as often as do those who die naturally, but they *also* appear to many strangers or people who hardly know them, and they do so relatively more often than those who suffer a natural death. This suggests that those who suffer a violent death are stronger or more aggressive communicators. One would expect a person with little motivation to perceive a dead person who is a stranger, or—to put it differently—to involuntarily and unconsciously generate an image of a deceased person that the perceiver did not know or knew hardly at all. Hence, it is not inappropriate to assume that the deceased person is the motivational force behind the appearance or the encounter, that is, that the appearance is largely independent of the perceiver. This finding gives considerable support to the views of Myers (1903), Gibson (1944), and Stevenson (1982), as well as to the popular view that the dead person (the agent) plays an active role in the formation of the apparitional experience. This is in contrast to Louisa Rhine's (1957) hypothesis that only the percipient plays an active role in apparitions of the dead. From these data, one can argue that the apparitional experience is thrust upon the perceiver by the appearing deceased person. In short, the apparition is invasive in character. These findings have important consequences for the question of survival.

Table 6
Cause of Death and Closeness to Perceiver

	Very close	Somewhat close	Slightly close	None
Disease	91 (49%)	40 (21%)	25 (13%)	31 (17%)
Violent	18 (23%)	22 (28%)	13 (16%)	27 (34%)

$\chi 2 = 18.23$, $df = 3$, $p < .000$

Now we can come back to the striking finding that two thirds of the communicators were males. How can that be? In the relevant period of time in Iceland, 4.4% of women died violently compared to 12.5% of men. Thus, three fourths of those who died violently are males, and, as noted above, those who suffer violent deaths are the stronger communicators. If we take these strong communicators out of our calculations, a different picture emerges. If we consider only the encounters with those who died on their sickbed, we find the proportion of male apparitions reduced from 67% to 56%, and thus there is a comparable increase in the proportion of female apparitions from 33% to 44%. The predominance of encounters with males thus seems to be caused primarily by the fact that a much greater number of men than women suffered a violent death. In our collection of

apparitional cases, 41% of the males died violently compared to only 8% of the females. That is a huge difference.

Violent deaths are almost invariably sudden. For this reason, some researchers have speculated that suddenness or unexpectedness may be more crucial than the actual mode of death. The correctness of this assumption is hard to check, because unexpectedness, as judged by the dying person, is not a category in the official statistics on the causes of death, and hence reliable information on the frequency of sudden deaths in the population is not available. Physicians are generally reluctant to even give estimates, so we do not have a baseline for reliable comparison. Nonetheless, we asked in our interviews if the perceived persons who died naturally also died suddenly and unexpectedly. This was the case in 14% of the natural deaths. Stevenson found in his analysis of cases in *Phantasms of the Living* that 25% were sudden death by disease and 28% were violent death. It follows that 53% of the appearing persons in that large collection had died unexpectedly. In our collection, the corresponding percentage is 44% (14 + 30). Perhaps sudden death was more common over a century ago when medicine was less advanced. Apparently, not only violent death but also sudden, unexpected death is more likely to lead to appearances of deceased persons than the more common gradual process of dying.

Encounters Near the Time of Death

Another prominent characteristic of reported encounters with the dead is how often they appear close to the time of death. In 14% of the cases in which the time of death is known, the encounter is reported to have taken place within 24 hours before and 24 hours after the time of death. In half of these cases (7%), they were reported to have occurred within an hour of the time of death. Even more startling is the finding that in 86% of the cases occurring within 24 hours of death, the person who had the encounter did not know that the perceived person had died or was dying. In the cases occurring within an hour of death, this figure increases to 89%. Let me illustrate this with two cases:

> My wife and I had living with us a little girl about two and a half years old whom we fostered. One night I wake up and feel as though a woman were standing beside the bed. She says to me, "My name is Margret." Then she vanishes out the door. I look at the clock and see that it is exactly three thirty. The day after or the same day I learn that the girl's grandmother had died at that same minute, from a heart attack at Hvammstangi (a town in another part of the country). Her name was Margret. I knew nothing about her health, am not even sure I remembered her name. I had never seen her when she was living. (2180)

The wife of the percipient told us that her husband had told her about his experience immediately the following morning and before they had learned of Margret's demise.

The next encounter occurred to a well-known member of parliament:

> I was a member of Parliament for 18 years and during that time I was in contact with many men who later became good acquaintances of mine. One of them was Karl Kristjánsson MP.... We were friends, and kept in touch on and off after we retired.... One winter's day I went out to the stable as I usually did after lunch. When I had been shovelling for a while, I suddenly felt Karl Kristjánsson standing in front of me in one of the stalls in the stable and he says something rather peculiar: "You were lucky, you did fine," and that was all; then he disappeared. That evening his death was announced on the radio. While pondering about the incident and trying to figure it out, I learnt that he had suffered a heart attack and been brought to the Reykjavik City Hospital where he died. I had been admitted to that hospital a year earlier after suffering a similar attack. I luckily recuperated and could go home, whereas he died. In that context I understood his words, "You were lucky, you had good luck." (7030)

The British founders of psychical research coined the term *crisis apparitions* for those experiences that occurred 12 hours before or after a person's death (Gurney, Myers, & Podmore, 1886). The frequency of reported crisis apparitions far exceeds what is to be expected if the timing of apparitional experiences is randomly distributed. This is confirmed in our survey, in which 14% of the encounters took place 24 hours before or after the person died (a longer time than in the British survey). Psychological factors, such as tending to remember striking experiences longer, may explain this effect to some extent, but they seem to be an insufficient explanation overall. It is also interesting that over half of all apparitional cases in our collection (51%) occurred within a year from the time the person died. In over one fifth of our cases, (22%) the percipient did not know that the perceived person had died.

Expressions of Purpose and Meaning

The previous case raises the question of how often a purpose is expressed in apparitional experiences. In our survey, a specific purpose is expressed in 28% of the cases. This is an example:

> This happened shortly after my father died. I was in bed sleeping but woke up and felt there was something at my side. I saw my father there, I looked at him, reached out and felt him. He was just as he had been, wearing a blue shirt over his underwear.... He got out of the bed and walked to the room he had lived in and was across from mine. As he walked, he pointed to the wall without saying a word. On that wall there was a large clock that he had earlier asked me to deliver to his grandson in Reykjavík. (2010)

The percipient's father was reminding him to fulfil his promise.

In the great majority of our cases, no purpose was evident to the respondent, except perhaps to let the percipient know that the deceased person "is there." Osis (1986) describes an interesting case of purposeful action by an apparition.

Collective Encounters

In half of the encounters (167), another person was present, and in 85 instances the second persons were in a position that should have allowed them to perceive the apparition (e.g., awake, turned in the proper direction). Thirty-nine of these 84 respondents reported that their experience was shared by the others who were present. Seven of these 39 second observers had died and two could not be traced, which left us with 30 witnesses that we were able to interview. In three cases the witnesses did not remember the incident or refused an interview, and in six cases it was not clear or doubtful whether the new testimony should be considered confirmatory, or whether it was referring to the same incident. In 21 instances out of the 30, the witnesses verified the respondent's description of the case.

In the first example, two persons report seeing an apparition of the same person at the same time:

> I was around twenty. My father and I sat in the kitchen around noon. Then I see clearly a woman coming towards us. I was not going to mention it but notice that my father also sees this. I asked him what he was looking at and he replied, "Surely the same as you." Then he said he knew this woman. She had died a while back. Three or four hours later there was a phone call for my father who was a clergyman. The husband of the deceased woman we saw had died. We had seen the woman around the time her husband had died. (5102)

We interviewed the father of the respondent, who described the incidence to us and thus verified his daughter's account.

Below is the case of a young man who was seen by more than two people at a time when he was living in a far away part of the country. However, the apparition was never seen by two people at exactly the same time and place.

> I was skating on ice in the open when I felt I saw my friend Erik. I thought this was some nonsense and skated on. A little later I see him again and close to me, and think again this cannot be and I must be hallucinating and try to shake it off. I felt a strange feeling going through my body and I look behind and see his face. Then I felt sure he must have died. I went home and told them. They said of course that I was talking nonsense. The next morning there came a telegram announcing his death. We did not have a telephone. (7616)

Erik had been living at a tuberculosis sanatorium where he died at the age of sixteen. When we asked the percipient's half-sister Thora, she remembered that her brother had looked shocked when he had come home and told them that Erik had come, he had seen him, and that he had looked pale and miserable. Thora thus certified that the primary witness had told her about his experience before Erik's death was known. Thora told us that some other people had also seen Erik more than once after the incident, although it was never more than one person at the same time. One incident that she experienced herself is particularly interesting:

> This happened in the winter. The sheep were out in the valley. I round them up and want to drive them into the barn. Whatever I do they do not go in and run to the side. Then I see where Erik is standing in the door. With his arm he points west to the farm where his mother was living. Erik then disappeared and I was able to get the sheep into the barn. Later in the winter his mother died of tuberculosis. (6026)

Thora believed that by pointing to the farm where his mother was living, Erik was expressing concern about his mother's health or indicating that she might soon die, which she did, although not immediately.

A close scrutiny of the cases reveals that collective observations often do not take place at exactly the same time. This is exemplified by the case below, reported by a well-known attorney in Iceland. It still seems reasonable to consider such a case collective.

> It was just after graduation in 1939. I was coming home from a dance. I hadn't tasted a drop of alcohol. It was about four o'clock in the morning and full light as we were in the

middle of summer. I was walking over a bare hill on my way home from town. Then there comes a woman towards me, kind of stooping, with a shawl over her head. And I don't pay any attention to her but as she passes me I say "Good morning" or something like that. She didn't say anything. Then I notice that she has changed her course and follows me a bit behind. I got slightly uneasy about this, and found it odd. When I stopped, she stopped also. I started saying my prayers in my mind to calm myself. When I came close to home she disappears. I lived in a house on the compound of a psychiatric hospital where my father worked. I go up to my room. My brother Agnar wakes up and says half asleep, "What is this old woman doing here? Why is this old woman with you?" And I tell him not to speak such nonsense but to continue sleeping, although I knew what he meant. I did not see the woman at that time but my brother appeared to see her when he woke up. I go out to get me some coffee. When I return to my room, Agnar gets up again and says, "Why has this woman come back?" And I tell him not to act like that, that there is no woman in here, that he is confused and should go to sleep.

At lunch the following day I say to my brother, "What nonsense was this last night? You thought you saw a woman in our bedroom." "Yes," he said, "I felt as though an old woman came with you into the room." Then our father became attentive and said to me "Did you see something last night?" I told him that I had seen this woman. "That is strange," he said, "around three o'clock this morning old Vigga died." What I had seen fit her description perfectly. (2196)

We approached the respondent's brother. He reported that he remembered this incident and that his brother had told him what he had seen. When asked whether he had seen the woman himself, he replied, "I saw a vague image of a hag ... but not clearly. When I woke up there I saw a woman come in with him.... This was a patient and always kept inside, I think, if it was her."

Earlier in this paper I described an auditory case (2198) where two witnesses had the same experience of their recently deceased father—a scholar of great repute in Iceland—at exactly the same time. Both of his sons were academics.

Discussion

What can we learn from the data in our new case collection of apparitions of the dead or, perhaps more precisely, alleged encounters

with the dead? First, how do they compare with the more than century-old monumental British surveys of apparitions, which have set the standard for such studies up to now?

We obtained results that are strikingly similar to those of the pioneering British surveys, and—nota bene—from a different country. This shows the generality of these experiences beyond particular time periods and locations. We found, for example, a number of crisis apparitions and a fair number of collective cases, as well as cases in which the percipients received correct information about a deceased person unknown to them at the time of the experience. Hence, our new case collection indicates that the prevailing academic attitude that experiences of this kind are "just hallucinations" appears to be as unsatisfactory today as it was to psychical researchers such as Myers, Podmore, Gurney, and the Sidgwicks more than a century ago. In our cases, we find ample data that reveal to us what can be seen as dramatic intrusions of meaningful imagery into consciousness. This meaningful imagery, in the form of apparitions, is of the same kind as that which has been put forward as evidence for survival (Myers, 1889a, 1889b, 1903; Stevenson, 1982), and thus the evidence for survival is further strengthened by the analyses of this case collection.

The crucial questions are: Where do these intrusions come from? Who initiates these "shadows of the dead"? Some of our cases point to an active role for the agent (the deceased person), for example, cases involving persons who have suffered a violent death. The best cases have features that many experiencers, as well as researchers, believe are best explained by accepting some form of survival, as the surveys clearly show (Emmons, 1982; Haraldsson, 1985; Haraldsson & Houtkooper 1991; Rees, 1971). Others may continue to argue that all of these experiences originate in the mind of the perceiver and that no external agency is needed to explain them. Such interpretations seem cumbersome in view of the presented data.

Could it be that our findings are biased and not representative, because of the self-report nature of most of our sample? This hypothesis can be tested. Of our cases, 307 are self-reports. The other cases include 130 cases from our random (and hence, representative) survey from 1974–75, 100 of which have already been analysed and published (Haraldsson, 1988–89). The main features of the 307 self-report cases can thus be compared with the same features from the 130 cases from our representative sample. Table 7 shows that the distribution of sensory modalities does not differ significantly in the two samples, nor does the cause of death or the relationship with the deceased. The only significant difference is the number of males versus females in the two samples; 78% of the deceased were males in the random sample and 66% were males in the self-report sample, $\chi^2(1, N = 411) = 5.52$, $p < .05$. The fact that there are more males in the random sample strengthens our conclusion about the preponderance of males in the apparitional cases. However, on the whole, the characteristics of the two

samples are comparable and indicate that the larger, self-selected sample is basically unbiased and representative of apparitional experiences in the population.

Table 7
Comparison of Features of the Deceased in the Random Sample (129) and the Self-Reported Sample (307)

	Random sample (%)	Self-reported sample (%)
Sensory modality		
Visual	48	50
Auditory	13	11
Olfactory	4	4
Touch	4	4
More than one modality	17	23
Feeling of presence	15	10
Cause of death		
Disease	70	72
Violent	30	28
Relationship to deceased		
Family relationship	46	49
Friends	11	7
Coworkers and acquaintances	14	14
Strangers	29	30
Sex of deceased		
Male	78	66
Female	22	34

Violent death plays an important role in several kinds of survival-related cases. First, there is a much higher percentage of persons suffering violent death in apparition cases compared to the general population. Second, the incidence of violent death is also very high—between 60 and 70%—in children who claim memories of a past life (Stevenson, 2001). Third, in a disproportionate number of trance mediumship cases we find communicators who suffered a violent death. For example, an

examination of a small sample of Mrs. Piper's sittings shows that a third of the communicators had died violently (Hodgson, 1892, 1897–1898). In 1972, when I was an observer at over forty sittings with the Icelandic medium Hafsteinn Björnsson, I noticed a high incidence of violent deaths among the communicators (Haraldsson & Stevenson, 1974). Regrettably, no systematic record was kept of the relative number of violent deaths among direct communicators, except for one séance. In this séance, nine direct communicators appeared, six of whom revealed how they had died: four in accidents and two from disease; thus, two thirds had suffered a violent death. I remember quite clearly that the preponderance of direct communicators who had suffered a violent death was very striking in these sittings.

Finally, whereas apparitions of persons who suffered a violent death often have been found empirically to have an invasive character, "drop-in" communicators in mediumship cases are invasive by definition. These are cases in which communicators that are *unknown* to both the sitters and the medium appear in the séance. Hence, it is hard to attribute motivation for their appearance to the sitters. I found two drop-in cases in Iceland, both of which involved violent death (Haraldsson & Stevenson, 1975a, 1975b). Stevenson independently published reports of four drop-in cases, two of which involved violent death (Ravaldini, Biondi, & Stevenson, 1990; Stevenson, 1965, 1970, 1973).

What is needed is a more thorough analysis of the mode of death of direct communicators in sittings with mediums of repute. This could strengthen or weaken the generality of the findings given above about the preponderance in trance mediumship of communicators who suffered a violent death.

In short, persons suffering a violent death feature predominantly in cases of apparitions of the dead and in cases of the reincarnation type, as well as in mediumship, including both direct communicators and drop-ins. The cases tend to have an invasive character, in that the deceased persons are frequently unknown to those who experience them and thus seem to assume an active role in their appearance. All of these findings tend, in my view, to support the survival hypothesis.

In conclusion, it is extremely important to conduct further research along all these lines to learn more about the role of the deceased agent in apparitional experiences and in mediumistic cases with direct as well as drop-in communicators. This further research is also important because of its implications for the question of survival after death.

References

Arcangel, D. (2005). *Afterlife encounters.* Charlottesville: Hampton Roads.

Emmons, C. F. (1982). *Chinese ghosts and ESP.* Metuchen, NJ: Scarecrow Press.

Gibson, E. P. (1944). An examination of motivation as found in selected cases from *Phantasms of the Living*. *Journal of the American Society for Psychical Research*, **38**, 83–105.

Greeley, A. M. (1987, January/February). Mysticism goes mainstream. *American Health*, 1987.

Guggenheim, B., & Guggenheim, J. (1995). *Hello from heaven*. Longwood, Florida: ADC Project.

Gurney, E., Myers, F. W. H., & Podmore, F. (1886). *Phantasms of the living* (2 vols.). London: Trübner.

Gurney, E., & Myers, F. W. H. (1887–88). On apparitions occurring soon after death. *Proceedings of the Society for Psychical Research*, **5**, 403–485.

Haraldsson, E. (1985). Representative national surveys of psychic phenomena: Iceland, Great Britain, Sweden, USA and Gallup's multinational survey. *Journal of the Society for Psychical Research*, **53**, 145–158.

Haraldsson, E. (1988–89). Survey of claimed encounters with the dead. *Omega, the Journal for Death and Dying*, **19**, 103–113.

Haraldsson, E., & Houtkooper, J. M. (1991). Psychic experiences in the Multinational Human Values Study. Who reports them? *Journal of the American Society for Psychical Research*, **85**, 145–165.

Haraldsson, E., & Stevenson, I. (1974). An experiment with the Icelandic medium Hafsteinn Björnsson. *Journal of the American Society for Psychical Research*, **68**, 192–202.

Haraldsson, E., & Stevenson, I. (1975a). A communicator of the "drop in" type in Iceland: The case of Runolfur Runolfsson. *Journal of the American Society for Psychical Research,* **69**, 33–59.

Haraldsson, E., & Stevenson, I. (1975b). A communicator of the "drop in" type in Iceland: The case of Gudni Magnusson. *Journal of the American Society for Psychical Research*, **69**, 245–261.

Hart, H. (1959). *The Enigma of survival: The case for and against an after life*. Springfield IL: Charles C. Thomas.

Hodgson, R. (1892). A record of observations of certain phenomena of trance. *Proceedings of the Society for Psychical Research*, **8**, 1–167.

Hodgson, R. (1897–1898). A further record of observations of certain phenomena of trance. *Proceedings of the Society for Psychical Research*, **13**, 284–582.

McClenon, J. (1988). A survey of Chinese anomalous experiences and comparison with Western representative national samples. *Journal for the Scientific Study of Religion,* **27**, 421–426.

McCready, W. C., & Greeley, A. M. (1976). *The ultimate values of the American population*. Beverly Hills, CA: Sage Publications.

Myers, F. W. H. (1889a). On recognised apparitions occurring more than a year after death. *Proceedings of the Society for Psychical Research*, **6**, 13–65.

Myers, F. W. H. (1889b). A defence of phantasm of the dead. *Proceedings of the Society for Psychical Research*, **6**, 314–357.

Myers, F. W. H. (1903). *Human personality and its survival of bodily death* (2 vols.). London: Longmans, Green.

Osis, K. (1986). Characteristics of purposeful action in an apparition case. *Journal of the American Society for Psychical Research*, **80**, 175–193.

Osis, K., & Haraldsson, E. (1986) *At the hour of death* (2nd ed.). New York: Hastings House.

Podmore, F. (1889). Phantasm of the dead from another point of view. *Proceedings of the Society for Psychical Research*, **6**, 229–313.

Ravaldini, S., Biondi, M., & Stevenson, I. (1990). The case of Giuseppe Riccardi: An unusual drop-in communicator in Italy. *Journal of the Society for Psychical Research*, **56**, 257–265.

Rees, W. D. (1971). The hallucinations of widowhood. *British Medical Journal*, **4**, 209–221.

Rhine, L. E. (1957). Hallucinatory psi experiences. II. The initiative of the percipient in hallucinations of the living, the dying, and the dead. *Journal of Parapsychology*, **21**, 13–46.

Schouten, S. A. (1979). Analysis of spontaneous cases as reported in the "Phantasms of the Living." *European Journal of Parapsychology*, **2**, 408–455.

Sidgwick, H., & Committee (1894). Report on the census of hallucinations. *Proceedings of the Society for Psychical Research*, **10**, 25–422.

Stevenson, I. (1965). New evidence on an important detail in the case of Abraham Florentine. *Journal of the American Society for Psychical Research*, **59**, 47–55.

Stevenson, I. (1970). A communicator unknown to medium and sitters. *Journal of the American Society for Psychical Research*, **64**, 53–65.

Stevenson, I. (1973). A communicator of the "drop in" type in France: The case of Robert Marie. *Journal of the American Society for Psychical Research*, **67**, 47–76.

Stevenson, I. (1982). The contribution of apparitions to the evidence for survival. *Journal of the American Society for Psychical Research*, **76**, 341–358.

Stevenson, I. (2001). *Children who remember previous lives. A question of reincarnation.* (Rev. ed.). Jefferson, NC: McFarland.

Tyrrell, G. N. M. (1953). *Apparitions.* New York: Pantheon Books.

Department of Psychology
University of Iceland
101 Reykjavik, Iceland
Erlendur@hi.is

Acknowledgments

The author is grateful for grants from the Research Fund of the University of Iceland, the Parapsychology Foundation in New York, and Salarrannsoknafelag Islands in Reykjavik. Special thanks to Haukur

Hjaltason, Jón Ingi Björnsson and Júlíus Björnsson, who conducted most of the interviews, and to anonymous reviewers for their constructive comments.

Abstracts in Other Languages

Spanish

SUPUESTOS ENCUENTROS CON LOS MUERTOS: LA IMPORTANCIA DE MUERTES VIOLENTAS EN 337 CASOS NUEVOS

RESUMEN: Encuentros personales con los muertos son informados por el 25% de los europeos occidentales y por el 30% de los americanos. Trescientos treinta y siete islandeses informaron sobre tales experiencias y fueron entrevistados en detalle. El 90% de estos informaron tener experiencias sensoriales (apariciones) sobre una persona fallecida; el 69% fueron visuales, el 28% auditivas, el 13%, táctiles, y el 4% olfativas. Menos de la mitad de las experiencias ocurrieron en el crepúsculo o en la oscuridad. En la mitad de los casos la persona estaba ocupada o trabajando. Una alta proporción de los casos fueron de apariciones de personas que murieron violentamente y apariciones de crisis observadas cerca del momento de la muerte de la persona percibida, aunque en la mayoría de los casos el percipiente no sabía que la persona había muerto. La forma de la muerte y la identidad de los difuntos fueron verificadas inspeccionando los registros oficiales. Un buen número de experiencias colectivas fueron informadas, algunas de las cuales fueron verificadas por otros testigos.

French

RENCONTRES ALLEGUEES AVEC LES MORTS: L'IMPORTANCE DE LA MORT VIOLENTE DANS 337 NOUVEAUX CAS

RESUME: Des rencontres personnelles avec des morts sont rapportées par 25 % des Européens de l'Ouest et 30 % des Américains. Trois cent trente sept islandais rapportant de telles expériences furent longuement interrogés. 90 % d'entre eux ont rapporté des expériences sensorielles (apparitions) d'une personne décédée ; 69 % étaient visuelles, 28 % auditives, 13 % tactiles et 4 % olfactives. Moins de la moitié des expériences sont survenues dans la pénombre ou l'obscurité. Dans la moitié des cas, la personne était activement absorbée dans une tâche. Il y a une disproportion importante des apparitions de ceux qui sont morts violemment et des apparitions de crise observées à proximité du moment de la mort de la personne perçue, bien que dans la majorité des cas, le percipient ne savait pas que la personne était morte. Le type de mort rapportée et l'identité des personnes décédées ont été vérifiés en consultant les registres officiels. Un bon nombre d'expériences collectives ont été rapportées, dont certaines ont été vérifiées auprès d'autres témoins.

German

BEHAUPTETE BEGEGNUNGEN MIT DEM TOD: DIE BEDEUTUNG DES GEWALTSAMEN TODES BEI 337 NEUEN FÄLLEN

ZUSAMMENFASSUNG: Persönliche Begegnungen mit dem Tod werden bei 25% der Westeuropäer und bei 30% der Amerikaner berichtet. Dreihundertsiebenundreißig Isländer, die solche Erfahrungen berichteten, wurden ausführlich interviewt. Neunzig Prozent von ihnen berichteten von sensorischen Erfahrungen (Erscheinungen) einer verstorbenen Person; 69% waren visueller, 28% auditiver, 13% taktiler und 4% olfaktorischer Art. Weniger als die Hälfte der Erfahrungen wurde in der Dämmerung oder Dunkelheit berichtet. In der Hälfte der Fälle war der Berichtserstatter mit etwas beschäftigt oder bei der Arbeit. Unverhältnismässig häufig vertreten waren Erscheinungen solcher Personen, die eines gewaltsamen Todes gestorben waren und Krisenerscheinungen, die in zeitlicher Nähe zum Tod der wahrgenommenen Person berichtet wurden, obwohl in der Mehrzahl der Fälle der Perzipient nicht wusste, dass die betreffende Person gestorben war. Die berichtete Todesart und die Identität der verstorbenen Personen wurden anhand offizieller Unterlagen verifiziert. Eine beträchtliche Anzahl kollektiver Erfahrungen wurde berichtet, darunter solche, die von anderen Zeugen bestätigt wurden.

DECISION AUGMENTATION IN A COMPUTER GUESSING TASK

By John Palmer[1]

ABSTRACT: Several psi and nonpsi hypotheses were tested in a computer guessing task. Participants (Ps) were 64 volunteers, 32 self-described strong believers in the paranormal and 32 self-described strong nonbelievers. Ps guessed sequences of the numbers 1–4 by calling each guess out loud and simultaneously clicking the mouse to register the response. In the 1st 2 runs, the target sequence reflected either pure repetition avoidance or pure counting, e.g., 2,3,4,1,2,3,4,1,2…. After Run 2, Ps completed psychological tests while the experimenter calculated their response bias in the preceding 2 runs. The 100 scored targets for Run 3 were random, except that every time P clicked the mouse when a computer address registered a 1 (1-state), which occurred randomly 20% of the time, they would receive a target for the next trial that matched their response bias in the preceding 2 runs, increasing the chances of a hit. As predicted from May's decision augmentation theory, in Run 3 believers clicked the mouse significantly more frequently than chance when the computer was in the 1-state, and significantly more often than nonbelievers. Both random and total hits in Run 3 were positively and significantly correlated with scores on the AT-20 test of tolerance for ambiguity.

Keywords: precognition, psychokinesis, decision augmentation, belief in the paranormal, tolerance of ambiguity

In recent years, many of the major psi testing paradigms have involved what I call *implicit psi*. These paradigms share in common that the designated psi sources are not asked to produce the hypothesized effect and may not even be aware that they are being tested for psi. Examples include research on presentiment (e.g., Bierman & Radin, 1997; Radin, 1997), the mere-exposure effect (e.g., Bem, 2003), and the global consciousness project (Nelson, 2001). The theoretical foundation for implicit psi, at least from a psychological perspective, is Stanford's (1977, 1990) psi-mediated instrumental response model, and, more recently, Carpenter's (2004, 2005) first sight model.

[1] A longer version of this paper was presented as a poster at the 50th annual convention of the Parapsychological Association in Winchester, England, August 2–5, 2007. I am grateful to the Cogito Foundation and to the Bial Foundation for supporting this research, and to the Parapsychology Foundation for funding the prize to the highest scoring participant. Thanks also to Peter Brugger for his contributions to the design of the experiment and his assistance with the logistics, and to Enrique Wintsch for writing some of the software. The paper for the *Journal* was independently peer reviewed under the auspices of Richard Broughton.

Decision Augmentation Theory

Another model with the potential for contributing to our understanding of implicit psi is decision augmentation theory (DAT). Proposed by Edwin May and colleagues, DAT is intended primarily to explain ostensible micro-PK effects as in fact due to ESP (May, Spottiswoode, Utts, & James, 1995a; May, Utts, & Spottiswoode, 1995b). Although controversial (Dobyns & Nelson, 1998), DAT appears to account successfully for at least some of the relevant data. Most of the experiments that the theory has addressed are of the random number generator psychokinesis (RNG-PK) type. In most such experiments, a "hardware" RNG converts electronic noise into binary numbers, the distribution of which should follow the stochastic laws of chance. Most notably, the numbers of each digit generated should be exactly equal in an infinite sequence and approximately equal in finite sequences, the approximation improving as the length of the finite sequence increases.

The traditional interpretation of data from these experiments is that participants (Ps) use PK to bias the noise source such that it yields a significantly unequal distribution of the two binary numbers. DAT rejects these "force" models. It maintains instead that P intersects the target stream being produced by the RNG at the point at which a "biased" subsequence is about to appear, i.e., a sequence with an excess of one of the binary digits. Such subsequences will occasionally occur even in a true random sequence. The anomalous mechanism that P uses to detect (or predict) the point at which the target stream should be intercepted is what we call ESP, or more precisely, precognition. In DAT experiments, P decides when to intersect the target stream by pressing a button or clicking a mouse to initiate the test or run. In an important sense the button press or mouse click *is* the ESP response. DAT and the existing force models make different predictions about the relationship between the length of the sequences and the ESP scores resulting therefrom, and it is on the basis of tests of these predictions that May and colleagues claim confirmation of the theory (May et al., 1995b).

DAT can be applied to ESP as well as PK data, and it is especially well suited to ESP experiments of the RNG type. One way it could work would be for P to use the DAT mechanism to enter the target stream at the time it was about to produce a "biased" subsequence that is consistent with a naturally occurring response bias of P. This would then create a bias-matching situation and thereby yield an increase in ESP hits.

In this experiment, I tested the mechanism underlying DAT by assigning Ps a hit in the first nonfeedback run whenever they made a mouse click registering their guess at the same time a hidden computer address was in a certain state, determined randomly with a 1/5th probability. Moreover, Ps were rewarded for these hits by being given on the next trial a target that matched their response bias, as determined from the preceding runs.

Belief in the Paranormal

There is ample evidence in the parapsychological literature that believers in ESP score on average more positively than nonbelievers in ESP tests (Lawrence, 1993; Palmer, 1971; Schmeidler & McConnell, 1958/1973). Most of this evidence comes from ESP tests of the forced-choice type. Since DAT is postulated to be the mechanism by which ESP operates, I would expect it to have comparable correlates, including belief. For this reason, I expected DAT to function more positively in believers in the paranormal than in nonbelievers. This is especially likely in the present experiment, because one would expect the "reward" for using DAT (the opportunity to enhance one's score on the overt ESP test) to be rewarding for believers but not nonbelievers.

Hypotheses

The formal ESP hypotheses for the experiment are as follows:

1. Ps will register their responses more often than expected by chance when the computer is in a state leading to a favorable target on the next trial.
2. Hypothesis 1 will be confirmed more strongly for believers than for nonbelievers.
3. Ps will score significantly above chance on those trials with biased targets.

Method[2]

Participants

Sixty-four volunteers were recruited from the University of Zürich community and the city of Zürich. Written informed consent was obtained at the beginning of the test session.

As an additional requirement, these recruits had to indicate either that they have a strong belief in ESP and have had previous psychic experiences, or that they have a strong disbelief in ESP and no previous psychic experiences. This variable will be referred to hereafter as "belief." This specification was included in the recruitment poster.

Midway through the experiment I became concerned that I would not be able to obtain a sufficient number of Ps before I had to leave Zürich.

[2] As this experiment was funded primarily to test specific hypotheses regarding response biases and implicit sequence learning, the ESP tests had to be fitted into the experimental protocol without compromising the tests of these hypotheses or increasing the duration of testing. For this reason, the ESP tests are more complicated than they would have been otherwise.

I thus decided to offer a prize of 500 Swiss Francs (approximately $400) to the P who achieved the highest score in the experiment. To which runs this scoring applied was left undefined so Ps would be equally motivated for all the runs in the experiment. In fact, the prize applied only to the first three runs, as the procedure for Run 4 was not the same for all Ps. Although all Ps were eligible to win the prize, only those 38 Ps who were tested after the prize was decided upon knew about it before their test session. The winner was a nonbeliever.

Questionnaires

The Australian Sheep-Goat Scale (ASGS). The ASGS (Thalbourne & Delin, 1993) was used as a check on the status of Ps who assigned themselves to the believer and nonbeliever groups. It consists of 18 items reflecting both belief in and experiences of various types of psychic phenomena. The items were presented in a visual analogue format, with possible scores on each item ranging from 0 to 13.

The Post-Test Assessment Scale (PTAS). This rating scale was developed by Peter Brugger to assess how Ps react to the test procedure in implicit learning experiments of the type conducted by himself and his associates. The most important question asks Ps whether they came to expect that a target sequence was biased and, if yes, at what point in the testing and the nature of that bias. Ps who can correctly identify the bias are classified as "detectors." In past research of this type conducted by Brugger and associates, about 15% of the participants in the original sample have proven to be detectors. In this experiment, data from detectors were not included in the formal analyses, and they were replaced by new Ps with the same belief in ESP.

A second set of questions asks Ps to estimate how many trials were included in each run. Third, Ps are asked if they responded intuitively, adopted a logical strategy, or a combination of the two. Finally, they are asked to describe any guessing strategies they used and when they used them.

Drawing Task. This test was developed as a measure of cerebral lateralization with respect to perceptuo-motor organization (Alter, 1989; Alter, Rein, & Toro, 1989). Ps are asked to draw rapidly on separate sheets of paper six familiar objects: bicycle, walking dog, bus, facial profile, airplane, and pitcher (ewer). The score is the number of drawings in which the object is facing right minus the number in which it is facing left, divided by the total number of drawings. The scale has a range from -1 to +1. Drawings in which the object faces neither right nor left are not counted. Right-handers tend to produce drawings facing left, and left-handers tend to produce drawings facing right, but the discrimination is not absolute (Alter, 1989).

LIMBEX Scale. The LIMBEX is intended to measure signs of temporal lobe dysfunction, or what is referred to more specifically as complex partial epilepsy. It was developed by Brugger, who chose those 13 items from a longer scale by Makarec and Persinger (1990) that had the highest point biserial correlations with the total score in a sample of 40 volunteers. Each item of the LIMBEX is a 6-point scale, resulting in a theoretical range of scores from 0 to 65. Although persons with complex partial seizures have been shown to score high on the original scale, some others also obtain high scores, and a high score by itself is not diagnostic of a seizure disorder.

Ambiguity Tolerance Scale (AT-20). This 20-item true-false scale is a revision of the 16-item Rydell-Rosen Ambiguity Tolerance Scale (MacDonald, 1970). MacDonald defines a high scorer on the scale as a person who seeks out ambiguity, enjoys ambiguity, and excels in the performance of ambiguous tasks. The task in this experiment clearly could be described as ambiguous. The AT-20 correlates in the .4 range with Rokeach's Dogmatism Scale, as well as with Gough and Sanford's Rigidity Scale (MacDonald, 1970).

Equipment

Testing was performed on a Compaq Deskpro EXM/P800 computer. Random target sequences were generated using Visual Basic, whereas the on-screen presentation was programmed with Java-Script.

Guessing Task

P was seated in front of the computer monitor, which continuously displayed squares containing the digits 1, 2, 3, 4, arranged in a vertical column in either increasing or decreasing numerical order (counterbalanced across Ps) from the top to the bottom of the screen. The reason for the vertical display was to eliminate the effect of left/right response biases potentially confounding P's guesses. At the beginning of the run, a box surrounding the word *start* was superimposed over the column of digits. P mouse-clicked on this box to begin the run, at which time the box disappeared. P's task was then to repeatedly guess which digit the computer would select for the ensuing trial. Ps indicated each choice by saying the digit out loud and simultaneously clicking the mouse. The experimenter (E), who was seated next to P, immediately entered Ps response on the computer keyboard. P's oral responses were tape recorded, and after the session E checked the typed responses against these oral responses to check for possible entry errors. The setup is illustrated in Figure 1.

The number of milliseconds between the appearance of the array and Ps' mouse click to indicate their guess was recorded by the computer as a measure of reaction time. The computer also recorded and stored the target sequence type (see below), the run number, the targets, and Ps' responses.

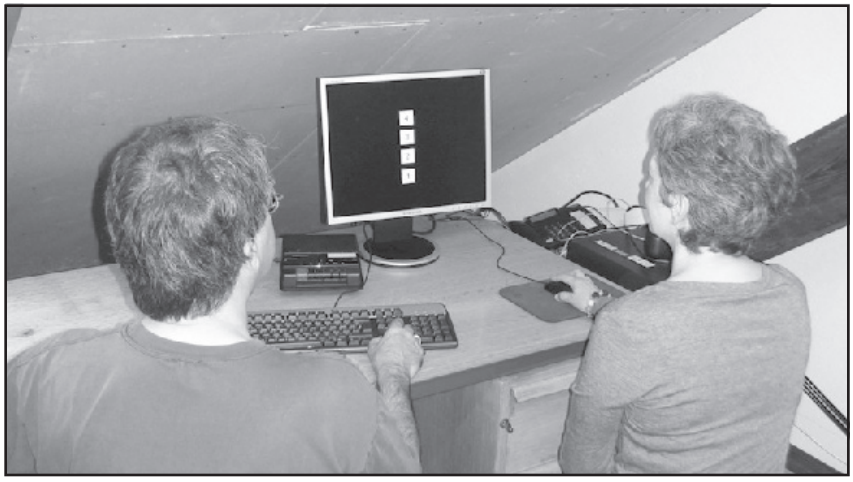

Figure 1. Layout for the testing

Target randomization employed an algorithm developed by Marsaglia and Zaman (1987) and thoroughly tested to assure passage of numerous tests of nonrandomness. The first pair of seed numbers for the formal experiment was 1 and 2,[3] and every time in the experiment that a new sequence was called for, the seeds were advanced to the next pair. This procedure provided each P with unique target sequences (no stacking effect).

Procedure and Test Protocol

Each P completed two sets of two runs. The first run was preceded by as many practice trials as necessary to assure that P understood the procedure and that P and E were "in synch" regarding their respective mouse and keyboard entries. If two successive mouse clicks or keyboard entries occurred without an intervening input of the opposite type, the computer indicated the error by "1" (indicating successive keyboard entries) or a series (indicating successive mouse clicks) of beeps. E then said "repeat" or "next," thereby instructing P what to do to correct the error, and repeated the keyboard entry if necessary. This problem arose quite rarely in the formal testing.

Runs 1 and 2. The first two runs each consisted of 80 scored trials[4] and were administered without feedback. One of the runs drew exclusively biased targets generated by an algorithm created to mimic a kind of response

[3] The Marsaglia algorithm requires input of two seed numbers.

[4] Each run began with an unscored trial. This was necessary because some of the target and response biases were defined by the relationship between the trial in question and the immediately preceding trial.

bias often demonstrated by normal Ps, namely *repetition avoidance*. In this run the targets never repeated, but after the first target in the sequence each target appeared an equal number of times (i.e., 20). Otherwise the sequence was random. For the other run, targets were assigned by an algorithm that, after the randomly selected first target, produced the extreme form of the *counting* bias characteristic of Alzheimer's patients (Brugger, Monsch, Salmon, & Butters, 1996). For example, if the first target was 2, the sequence was 2, 3, 4, 1, 2, 3, 4, 1, 2, 3 …. The order of these two run types was counterbalanced across Ps.

Following the second run, P moved to a chair facing away from E and the computer screen and then completed, in order, the drawing task, the AT-20, and the LIMBEX scale. At the same time, E moved to the adjacent chair in front of the computer and determined P's most marked response bias in the previous two runs. The computer records from these runs were merged and the resulting file submitted to analysis using software developed by Towse and Neil (1998). The frequency of each *single* target (1, 2, 3, and 4) and the relationship of each target to its predecessor (*shifts* of 0, +1, +2, or +3 units) were recorded from the Towse output.[5] The chance probability for each of these eight alternatives is .25. The summed frequencies for the first, second, and third most frequent single and shift responses, respectively, were then computed and recorded. A table had been developed which indicated the chance likelihood for each of these frequencies and ranked them, with the least likely alternatives getting the highest ranks (see Appendix). The table provides ranks for each of 24 possible response biases, i.e., the sum of the most frequently called one, two, and three choices for singles and shifts, respectively. For example, conformance to the counting bias would yield a high rank for +1 shifts, whereas repetition avoidance would be reflected by a high rank for the sum of +1, +2, and +3 shifts, which is equivalent to a low frequency of 0 shifts, or repetitions. The bias that received the highest rank, and the value of that rank, were then recorded by E. For example, in the rating scale illustrated in the Appendix, repetition avoidance (+1, +2, +3) received a rank of 20, which is higher than the ranks given to the excess of +1 and +2 shifts (17.5), the excess of 1s and 2s (4), and the excess of 1s, 2s, and 3s, or a deficiency of 4s (4). Thus, repetition avoidance was chosen as the most likely response bias in Run 3 and therefore was used for the DAT manipulation described below.

Run 3. Following completion of their respective tasks, P and E resumed the seating arrangements in effect for the first two runs. Following a few practice trials, Run 3 (N = 100 scored trials), which tested the DAT mechanism, was initiated. From P's point of view the procedure was the same as for the first two runs, except that a 2 s delay was introduced before each trial, during which the computer screen was blank. Ps were instructed

[5] This required that +1 and –3, +2 and –2, and –1 and +3 each be summed from the Towse table.

to blank their minds during the 2 s interval and only formulate their guesses when the column of digits returned to the screen. This modification of procedure was introduced in an effort to increase the variability of reaction times by attempting to break up the rhythm Ps often got into during the first two runs. Pilot testing had indicated this modification would have the desired effect.

An address inside the computer randomly alternated its content between 0 and 1, such that it (or, we could say, the computer) was in the 1-state 20% of the time during the run. This outcome was programmed as follows. Thirty repetitions of the digits 2 through 6 (N = 150) were randomly permutated, separately for each P. Each digit represented a .2-s interval, during which the computer would be in the 0-state. Following this time span, the computer would be in the 1-state for .2 s. Thus, it would be in the 0-state anywhere from .4 to 1.2 s (the sequence of these intervals being random) before the next 1-state, and there were never two 1-states in a row. The subroutine was activated at the time P clicked the start box on the screen, and the sequence simply recycled after it was exhausted (every 2.5 min).

Each time P clicked the mouse while the computer was in the 1-state, the next target was guaranteed to conform to P's most likely response bias, as defined by the calculation (described above) of P's most extreme response bias during the first two runs. (In the example in the Appendix, this is repetition avoidance.) For example, Ps who called an excess of 4s in the first two runs would be guaranteed to receive a 4 as the target for the next trial following any trial in which they clicked the mouse while the computer was in the 1-state. Likewise, if Ps had demonstrated repetition avoidance previously, their target following a 1-state mouse click would never duplicate their immediately preceding response. The effect of this procedure was to increase Ps' chances of a hit on the manipulated trials, insofar as they maintained the response bias they demonstrated in the first two runs. The course of Run 3 is illustrated in Figure 2.

Run 4. This run consisted of 90 scored trials with trial-by-trial feedback that was subliminal for half of the participants. For half of each subgroup, the response bias was the same as in Runs 1 and 2, and for the other half it was opposite to that in Runs 1 and 2. As this run is not relevant to the DAT test (which was completed in Run 3) it will not be described further, and the results from it will not be reported in this paper.

After Run 4, P was administered the PATS and ASGS, in that order. During this period, E returned to his office, printed out the results of the four guessing runs, and entered the data on the Participant Feedback Form, which also explained the rationale of the experiment. When E returned to the testing room, and after P had completed the scales, E gave P the feedback form, which P read. E then showed P the data sheets and answered any questions P had about the experiment or his or her results. Finally, P was asked not to discuss the details of the experiment with anyone who might participate in the experiment at a later time.

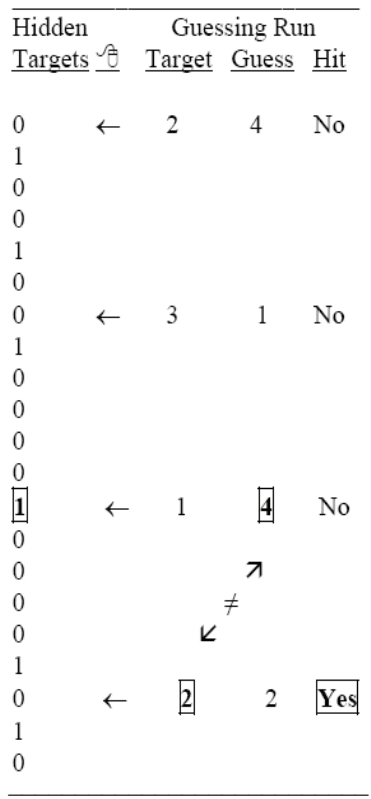

Figure 2. The DAT manipulation

Summary of Design

Five between-P variables were counterbalanced: (1) *belief* in the paranormal (believer vs. nonbeliever), (2) order of the four digits on the *screen* (ascending vs. descending), (3) target *bias* in the first two runs (repetition avoidance vs. counting), (4) bias of *targets* in the feedback run (pro-bias vs. counter-bias), and (5) *speed* of presentation of the feedback digits in the feedback run (supraliminal vs. subliminal). The four runs served as the single within-Ps variable. However, as the hypotheses were run-specific, no analysis was performed corresponding to this full design.

Results

Elimination of Flawed Data

Eight Ps were replaced during the course of the experiment. Five were replaced because of recording errors of either targets or responses in

one or more runs. This came about because of errors in the sequence of oral calls and mouse clicks by P or E that could not be resolved by listening to the tapes of P's calls. There were five other cases involving Run 1 or 2 in which such errors involved the final five or fewer trials in the run. In these cases, the suspect trials were eliminated from the calculations of the run scores. One P was replaced because she had been defined as a nonbeliever but scored above the midpoint on the ASGS, i.e., in the believing direction. One believer was replaced because in Runs 3 and 4 she called the same number many times in succession, creating extreme response bias scores. Finally, one believer was replaced because she correctly detected during Run 4 that the targets were related to her own responses. This caused her to obtain an extremely high number of hits on this run.

After completion of testing it was found that for one nonbeliever in the control condition of Run 4, the protocol for defining the target bias for this run was grossly violated, such that the targets reflected the P's response bias in Runs 1 and 2 positively rather than negatively. There was not sufficient time to replace this P, so her Run 4 guessing data were eliminated from the analyses.

Tests of Hypotheses

Hypothesis 1 was tested by examining how frequently Ps clicked the mouse when the computer was in the "1-state" in Run 3—20% of the time by chance. The mean percentage of such clicks was 20.80 ($SD = 3.34$), $t(63) = 1.94$, $p = .057$.[6] As this result does not quite reach significance, Hypothesis 1 is suggestively supported. However, the percentage for believers was significantly high ($M = 21.7$; $SD = 3.23$), $t(31) = 3.06$, $p = .006$, and significantly higher than the nonsignificant percentage for nonbelievers ($M = 19.85$; $SD = 3.21$), $t(62) = 2.36$, $p = .022$. Thus Hypothesis 2 was strongly supported.

On trials in which Ps received targets consistent with their response biases in the preceding runs—trials in which the computer was in the 1-state for the preceding trial—the percentage of hits was quite high (30.92; $SD = 11.18$) and strongly significant, $t(63) = 4.23$, $p = .0001$. Thus, Hypothesis 3 was strongly supported. The observed mean was compared to the mean chance expectation of .25 that would apply under null conditions, that is, no matching target and response biases; thus, this result is not an ESP effect. It nonetheless confirms that 1-state trials produced the intended positive reinforcement. However, this advantage was not enough to produce significant positive scoring in the entire Run 3 for either believers ($M = 26.06$; $SD = 4.56$), $t(31) = 1.32$, or nonbelievers ($M = 25.84$; $SD = 3.07$), $t(31) = 1.55$. However, due to greater power, the mean for the whole sample just missed significance ($M = 25.97$; $SD = 3.86$), $t(63) = 1.97$, $p = .053$.

[6] All p values in this report are two-tailed.

Exploratory Analyses

The only predictor variables from the questionnaires to correlate significantly with the ESP scores in Run 3 are listed below. They should not be taken too seriously unless or until they are replicated.

Ambiguity tolerance. There was a significant positive correlation between scores on the AT-20 Scale and the proportion of hits in Run 3, both for all trials, $r(61) = .320$, $p = .010$, and for the random trials, $r(61) = .266$, $p = .035$. This means that the greater the tolerance for ambiguity, the higher the proportion of hits. The means on the ambiguity scale were quite similar for believers ($M = 11.09$; $SD = 3.16$) and nonbelievers ($M = 10.82$; $SD = 3.18$), $t(61) = 0.34$.

Post-test Rating Scale. Not surprisingly, believers estimated a higher proportion of hits for Run 3 than did nonbelievers ($M = .343$ vs $.250$), $t(45.6) = 2.32$, $p = .025$, but the variance was also significantly higher for believers ($F = 6.38$, $p = .014$, by Levene's test). Estimated success in Run 3 correlated negatively with success in the random trials of this run to a significant degree among all Ps, r_s ($N = 61$) = -.346, $p = .006$. The average number of trials Ps estimated for Run 3, which consisted of 101 trials, was 62.3. This marked underestimate occurred despite the fact that the written instructions mentioned that the number of trials per run would vary between 80 and 120.

Discussion

DAT predicts that in RNG "PK" experiments positive scoring is achieved by P intersecting a random stream of binary digits at a time that captures a scored subsequence tending to match the designated target. I operationalized this principle in the present experiment by allowing Ps to create targets matching their response biases, as estimated by the response biases they demonstrated in previous runs. Ps could create these targets by making their mouse-click responses at an opportune time, namely at a time in which the computer was randomly in the 1-state. Doing so would allow them to improve their score, and I thus predicted that Ps would generate more 1-state trials than predicted by chance. This hypothesis was suggestively confirmed with $p < .10$. As believers are more likely than nonbelievers to be motivated to attain a high score, it is not surprising that, as hypothesized, a significant excess of 1-state trials was achieved only by believers. The fact that trials determined by the manipulation yielded a high percentage of hits (30.92%) demonstrated that the manipulation had the intended effect, although it was not strong enough to yield overall significant positive scoring in Run 3 for either believers or nonbelievers. The bottom line is that these results confirm the DAT mechanism and show that it applies to ESP as well as PK test paradigms.

In a DAT experiment of the PK type, the DAT effect is generally created by a keyboard button press or mouse click that intercepts a rapidly moving bit stream of 0s and 1s in the computer. It thereby selects a long sequence of subsequent numbers that, if DAT is operating, will have an excess of, say, 1s. In the present experiment, there was also a rapidly moving sequence of 0s and 1s that, unlike in the PK example, P intercepts on each trial, using a mouse click. Thus, P selects an individual target rather than a sequence of targets. This is actually a more challenging task than P confronts in the PK case. In the latter, it is likely that any of several adjacent sequences could be selected that have the necessary bias. This means that P could press the button at any one of several adjacent moments and still achieve the desired result. That leeway is not provided by the design of the present experiment. Offsetting this disadvantage is the fact that the number stream moved more slowly in the present experiment than in a typical PK experiment.

These results represent the strong sense of what I call implicit psi; that is, psi occurred without awareness by Ps that psi was being tested. Although Ps probably realized that ESP was tested in Run 3, they were not informed that the timing of their mouse clicks had any influence on their results.

References

Alter, I. (1989). A cerebral origin for "directionality." *Neuropsychologia*, **27**, 563–573.

Alter, I., Rein, S., & Toro, A. (1989). A directional bias for studies of laterality. *Neuropsychologia*, **27**, 251–257.

Bem, D. J. (2003). Precognitive habituation: Replicable evidence for a process of anomalous cognition. *Proceedings of Presented Papers: The Parapsychological Association 46th Annual Convention*, 6–20.

Bem, D. J., & Honorton, C. (1994). Does psi exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin*, **115**, 4–18.

Bierman, D. J., & Radin, D. I. (1997). Anomalous anticipatory response on randomized future conditions. *Perceptual and Motor Skills*, **84**, 689.

Brugger, P., Monsch, A. U., Salmon, D. P., & Butters, N. (1996). Random number generation in dementia of the Alzheimer type: A test of frontal executive functions. *Neuropsychologia*, **34**, 97–103.

Carpenter, J. C. (2004). First Sight: Part one, a model of psi and the mind. *Journal of Parapsychology*, **68**, 217–254.

Carpenter, J. C. (2005). First Sight: Part two, elaboration of a model of psi and the mind. *Journal of Parapsychology*, **69**, 63–112.

Dobyns, Y. H., & Nelson, R. D. (1998). Empirical evidence against decision augmentation theory. *Journal of Scientific Exploration*, **12**, 231–257.

Lawrence, T. R. (1993). Gathering in the sheep and the goats…A meta-analysis of forced-choice sheep-goat ESP studies, 1947–1993. *Proceedings of Presented Papers: The Parapsychological Association 36th Annual Convention*, 75–86.

MacDonald, A. P., Jr. (1970). Revised scale for ambiguity tolerance: Reliability and validity. *Psychological Reports*, **26**, 791–798.

Makarec, K., & Persinger, M. A. (1990). Electroencephalographic validation of a temporal lobe signs inventory in a normal population. *Journal of Research in Personality*, **24**, 323–337.

Marsaglia, G., & Zaman, A. (1987). Toward a universal random number generator (FSU-SCRI-87-50). Gainesville, FL: Florida State University.

May, E. C., Spottiswoode, S. J. P., Utts, J. M., & James, C. L. (1995a). Applications of decision augmentation theory. *Journal of Parapsychology*, **59**, 221–250.

May, E. C., Utts, J. M., & Spottiswoode, S. J. P. (1995b). Decision augmentation theory: Applications to the random number generator database. *Journal of Scientific Exploration*, **9**, 453–488.

Nelson, R. (2001). Correlation of global events with REG data: An internet-based, nonlocal anomalies experiment. *Journal of Parapsychology*, **65**, 247–272.

Palmer, J. (1971). Scoring in ESP tests as a function of belief in ESP. Part I. The sheep-goat effect. *Journal of the American Society for Psychical Research*, **66**, 1–26.

Radin, D. I. (1997). Unconscious perception of future emotions: An experiment in presentiment. *Journal of Scientific Exploration*, **11**, 163–180.

Schmeidler, G. R., & McConnell, R. A. (1973). *ESP and personality patterns.* Westport, CT: Greenwood. (Original work published 1958)

Stanford, R. G. (1977). Conceptual frameworks of contemporary psi research. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 823–858). New York: Van Nostrand Reinhold.

Stanford, R. G. (1990). An experimentally testable model for spontaneous psi events: A review of related evidence and concepts from parapsychology and other sciences. In S. Krippner (Ed.), *Advances in parapsychological research 6* (pp. 64–167). Jefferson, NC: McFarland.

Thalbourne, M. A., & Delin, P. S. (1993). A new instrument for measuring the sheep-goat variable: Its psychometric properties and factor structure. *Journal of the Society for Psychical Research*, **59**, 172–186.

Towse, J. N., & Neil, D. (1998). Analyzing human random generation behaviour: A review of methods used and a computer program for describing performance. *Behaviour Research Methods, Instruments, and Computers*, **30**, 583–591.

Rhine Research Center
2741 Campus Walk Ave.
Building 500
Durham, NC 27705, USA
john@rhine.org

Abstracts in Other Languages

Spanish

AUMENTO DE DECISIONES EN UNA TAREA DE COMPUTADORA DE ADIVINAR

RESUMEN: Varias hipotesis psi y no-psi fueron puestas a prueba en una tarea de adivinación de computadora. Los participantes (Ps) fueron 64 voluntarios, 32 que se describieron como creyentes fuertes en lo paranormal y 32 como no creyentes. Los Ps adivinaron secuencias de los números 1-4 a voz alta y al mismo tiempo aprentaron el ratón para registrar la respuesta. En la primeras dos secuencias los objetivos reflejaron evitación de repetición o contar, tal como 2,3,4,1,2,3,4,1,2…. Después de la secuencia 2 los Ps contestaron pruebas psicológicas mientras el experimentador calculaba sus tendencias de respuestas en las dos secuencias anteriores. Los objetivos registrados para la tercera secuencia fueron aleatorios, excepto que cada vez que que el P activaba el ratón cuando la computadora registraba un 1 (1-estado), el cual occurría al azar el 20% del tiempo, ellos recibían un objetivo para la próxima prueba que correspondía a su tendencia de respuesta en las dos secuencias anteriores, aumentando la posibilidad de un acierto. De acuerdo a la predicción de la teoría de May de aumento de decisiones, en la tercera secuencia los creyentes activaron el ratón significativamente más frecuentemente de lo esperado al azar cuando la computadora estaba en el 1-estado, y significativamente más frecuentemente que los que no tenían creencias paranormales. Los aciertos aleatorios y totales obtivieron una correlación positiva y significativa con las puntuaciones de la prueba AT-20 de tolerancia de ambiguedad.

French

AUGMENTATION DE LA DECISION DANS UNE TACHE INFORMATISEE DE DIVINATION

RESUME: Plusieurs hypothèses psi et non-psi furent testées dans une tâche informatisée de divination. Les participants (Ps) étaient 64 volontaires, 32 se décrivant comme croyant fortement au paranormal et 32 se décrivant comme des incroyants invétérés. Les Ps devinaient des séquences des nombres 1 à 4 en disant chaque divination à haute voix et en cliquant simultanément sur la souris pour enregistrer la réponse. Dans les deux premières séries d'essais, la séquence cible

correspondait à un pur évitement de la répétition ou à un pur comptage, c'est-à-dire : 2,3,4,1,2,3,4,1,2… Après la deuxième série, les Ps complétaient des tests psychologiques tandis que l'expérimentateur calculait leur biais de réponse dans les deux séries précédentes. Les 100 nombres cibles dans la troisième série étaient aléatoires, sauf que chaque fois qu'un participant cliquait sur la souris quand l'ordinateur demandait la réponse 1 (état-1), ce qui arrivait aléatoirement 20 % du temps, il recevait pour l'essai suivant une cible qui correspondait à son biais de réponse dans les deux séries précédentes, augmentant ainsi ses chances de succès. Comme le prédit la théorie de l'augmentation de la décision de May, durant la troisième série d'essais, les croyants ont cliqué sur la souris significativement plus fréquemment que le hasard lorsque l'ordinateur était dans un état-1, et significativement plus que les incroyants. A la fois les succès aléatoires et totaux dans la troisième série étaient positivement et significativement corrélés avec les scores au test AT-20 de la tolérance à l'ambiguïté.

German

ENTSCHEIDUNGSZUWACHS BEI EINER COMPUTER-RATEAUFGABE

ZUSAMMENFASSUNG: Mehrere Psi- und Nicht-Psi-Hypothesen wurden in einer Computer-Rateaufgabe getestet. Teilnehmer (Tn) waren 64 Freiwillige, 32 beschrieben sich als starke Psi-Gläubige und 32 als starke Ungläubige. Tn hatten die Abfolge der Zahlen 1–4 zu erraten, indem sie jede Zahl laut aussprachen und gleichzeitig mit einem Mausklick die Antwort registrieren mußten. Bei den ersten 2 Durchgängen (runs) bestand die Abfolge der Ziele (targets) entweder aus einer reinen Wiederholungsvermeidung oder aus einem reinen Abzählen, z. B. 2,3,4,1,2,3,4,1,2…. Nach dem 2. Run füllten die Tn psychologische Tests aus, während der Experimentator ihren Reaktionsbias in den beiden vorhergehenden Runs auswertete. Die 100 registrierten Ziele für Run 3 waren zufällig, ausgenommen dann, daß jedesmal, wenn der T die Maus anklickte, wenn eine Computeradresse eine 1 (1-Zustand) registrierte, was zufällig in 20% der Zeit passierte, er ein Target für den nächsten Versuch bekam, das mit seinem Reaktionsbias in den beiden vorangegangen Runs übereinstimmte, wodurch die Wahrscheinlichkeit für ein Treffer erhöht wurde. Wie May's Theorie des Entscheidungszuwachses (decision augmentation theory) vorhersagt, klickten die Gläubigen bei Run 3 die Maus signifikant häufiger an als der Zufall, wenn sich der Computer im 1-Zustand befand, und signifikant häufiger als die Ungläubigen. Sowohl die zufälligen wie die Gesamttreffer in Run 3 waren positiv and signifikant mit den Ergebnissen im AT-20 Test für Toleranzambiguität korreliert.

APPENDIX

Ranks for Response Sums

ANY 1		ANY 2		ANY 3	
42	1	82	1	122	1
43	2	83	2	123	2
45	3	85	3	123	3
46	4	86	4	126	4
48	5	88	5	128	5
50	6	90	6	130	6
51	7	91	7	131	7
53	8	93	8	133	8
54	9	94	9	134	9
56	10	96	10	136	10
58	11	98	11	138	11
59	12	99	12	139	12
61	13	101	13	141	13
62	14	102	14	142	14
64	15	104	15	144	15
66	16	106	16	146	16
67	17	107	17	147	17
69	18	109	18	149	18
70	19	110	19	150	19
72	20	112	20	152	20
74	21	114	21	154	21
75	22	115	22	155	22
77	23	117	23	157	23
78	24	118	24	158	24
80	25	120	25	160	25
82	26	122	26		
83	27	123	27		
85	28	125	28		
86	29	126	29		
88	30	128	30		
90	31	130	31		
91	32	131	32		
93	33	133	33		
94	34	134	34		
96	35	136	35		
98	36	138	36		
99	37	139	37		
101	38	141	38		
102	39	142	39		
104	40	144	40		
106	41	146	41		
107	42	147	42		
109	43	149	43		
110	44	150	44		
112	45	152	45		
114	46	154	46		
115	47	155	47		

RESPONSE BIAS RATING SCALE: RUNS 1 + 2 (SAMPLE)

Name: _____Date:_____
SN: _____

Single:

Bias	Tot	Rnk
1	46	1
2	40	2.5
3	40	2.5
4	34	4

Shift (sum):

0	8	4
1	50	2
2	58	1
3	44	3

Shift (1):

Bias	Tot
-3	26
-2	30
-1	22
0	8
+1	24
+2	28
+3	22

Single	Sum	Rnk	Numbers		
Highest 2:	86	4	1	2	
Highest 3:	126	4	1	2	3

Shift	Sum	Rnk	Numbers		
Highest 2:	108	17.5	+1	+2	
Highest 3:	152	20	+1	+2	+3

Choice: (~~Single~~/Shift) +1 +2 +3 Rank: 20

HYPNOTIZABILITY AND DISSOCIATION AS PREDICTORS OF PERFORMANCE IN A PRECOGNITION TASK: A PILOT STUDY[1]

By Etzel Cardeña, David Marcusson-Clavertz, and John Wasmuth

ABSTRACT: We investigated whether hypnotizability, dissociation, and belief in psi are related to performance in a precognition task that measures whether later practice enhances previous memory recall. Participants low (*n* = 19) and high (*n* = 19, 7 also high in dissociation) in hypnotizability completed the task and a measure of belief in psi, with the experimenters masked as to participants' level of hypnotizability and dissociation. A general precognition effect was not replicated and there was no overall effect for hypnotizability, but high hypnotizables who were low in dissociation differed significantly from dissociative high hypnotizables and from low hypnotizables, and scored below chance. Belief in psi was related to dissociation and history of trauma, and trauma and working memory were negatively correlated. With respect to features of the experiment itself, we were contacted after we had run about one third of the participants by the developer of the program, who asked us to use a modified version. We did not change our protocol, but decided to analyze the data in segments before and after we were asked to use a different program. Although not significant, the first third of the data was clearly in the direction of psi-hitting whereas later segments were in the opposite direction. A regression analysis controlling for groups showed a significant decline effect. Our results support the value of using selected groups and suggest that dissociation may mediate the effects of hypnotizability in psi performance.

Keywords: precognition, hypnosis, hypnotizability, dissociation, decline effect

Honorton and Ferrari (1989) conducted a meta-analysis of precognitive experiments published between 1935 and 1987 and concluded that there is experimental support for precognition and that it could not likely be explained by real-time psi phenomena. A later series of six experiments by Steinkamp (2003), however, reported inconsistent support for a precognitive effect. Particularly relevant to this study are the recent findings of Bem (2008a), who created the precognition program and procedure we used in this study (Bem, 2008b). He reported (Experiment 3; Bem, 2008a) significant results in support of precognition, especially among participants scoring high in a measure of novelty seeking. In this study we evaluated whether hypnotizability, dissociation, and belief in psi affect performance on the precognition test. We decided to use Bem's

1 This research was partly supported through the Parapsychology Foundation's Coly Research Grant to the first author.

program to try to replicate his previous studies and evaluate its usefulness in our sample.

Psi and Hypnotizability

Hypnotizability is a construct that refers to the extent to which individuals follow and experience specific suggestions after a hypnotic induction. It has been related to abilities such as imagery (Kogon et al., 1998), fantasy-proneness (Lynn & Ruhe, 1986), absorption (Tellegen & Atkinson, 1974), and experiential mental boundaries (Cardeña & Terhune, 2008). Considering the nature of these correlates, the paucity of even moderate correlations between hypnotizability and the "big five" personality traits, and the finding that hypnotizability is related to the character trait of self-transcendence, Cardeña and Terhune (2008) proposed that the propensity to have experiences that suggest personal unboundariedness may be the latent trait underlying all of these correlations.

Kumar and Pekala (2001) reviewed studies that evaluated the relationship between hypnotizability and anomalous experiences and beliefs (including psi). Across 5 studies they found 11 correlations between hypnotizability and paranormal belief, of which 9 were significant (median $r = .20$). They found stronger results for hypnotizability and paranormal experiences; 23 correlations, of which 20 were significant, were reported across 11 studies (median $r = .31$). They also found 3 significant correlations across 2 studies for the relation between imagery and psi experiences, (median $r = .28$) and 9 significant correlations across 3 studies between absorption and anomalous experiences as well as 8 significant correlations between fantasy-proneness and paranormal experiences (median $r = .36$). As for the relationship between these constructs and psi belief, they found that 3 correlations in as many studies were significant (median $r = .16$).

Following an earlier meta-analysis by Schechter (1984), Stanford and Stein (1994) reviewed the literature on the topic of hypnosis and performance on controlled psi tests. The main question was whether psi performance is better following a hypnotic induction than during a control condition. The answer was positive, but no firm conclusions about the cause of the effect could be drawn, as it was not clear whether the induction per se or the difference in hypnotic abilities or even an experimenter effect might have caused the differences in performance. In an abstract, May, Bányai, Vassy, and Faith (2000) reported no evidence between hypnotizability and psi performance in a remote viewing experiment. However, Tressoldi and Del Prete (2007), replicating the results of an earlier experiment, found significant psi scoring in the first of two hypnotic sessions and significant small to moderate correlations between successful psi performance and the personality traits of absorption and transliminality, which have been related to anomalous experiences and perhaps significant psi scoring.

Psi, Dissociation, and Trauma

Also of particular relevance is the trait of dissociation, which refers to the propensity to have alterations of consciousness characterized by detachment from self or others, or failures in the integration of psychological processes that should ordinarily be integrated (Cardeña, 1994). Dissociative processes are sometimes assumed or directly suggested in hypnotic performance and there have been various proposals, harking back at least some decades (cf. White, 1937), that there may be two distinct types of high hypnotizable individuals: those who are mostly dissociative and do not exert imagery, and those who are mostly imaginative (e.g., Barber, 1999; Barrett, 1990; Cardeña, 1996).

Overall, there seems to be a stronger relation between dissociation and psi experiences than between the latter and overall hypnotizability. Re-analyses of data have shown that individuals exhibiting both high dissociativity and high hypnotizability were more likely to report anomalous (including putative psi) experiences than those with high scores in either trait and much more so than those with low scores in both of those traits (Pekala & Cardeña, 2000, p. 71). Thus, participants with high hypnotizability and high dissociation tend to believe more in the paranormal and to report a greater number of paranormal experiences than those who do not score high in both variables. It bears mentioning, however, that dissociation is not necessarily related to all types of unusual belief, such as those in fantastic animals (Sharps, Matthews, & Asten, 2006).

With respect to the relationship between dissociation and actual psi abilities, Cardeña (1998) described various observed links between the two, such as successful performance in psi tasks under dissociated states, a link previously discussed by early psychic researchers such as William James (e.g., Taylor, 1983) and the psychoanalyst Ferenczi. The latter reported that a "traumatic trance" would not only trigger dissociative mechanisms but also psi ("clairvoyant") abilities because of the "timeless and spaceless omniscience" (Ferenczi, 1955, pp. 162 and 243) occurring at the time of trauma, abilities that might later be used to correctly evaluate and perhaps escape further punishment and abuse (Ferenczi, 1955). Although not a sufficient cause, there is a close association between acute or chronic trauma and dissociation (Cardeña, Butler, & Spiegel, 2003). That chronically traumatized individuals might develop psi abilities could be predicted by the psi-mediated instrumental response (PMIR) theory of Stanford (1977), because psi information could help avoid distressing events.

Irwin (1994a, 1994b) has reported that individuals who have a history of early trauma tend to believe in psi phenomena and that the latter belief is positively correlated with a tendency to dissociate. He has explained these associations by positing that belief in psi and paranormal events may be a psychological defensive mechanism constructed to deal with the uncontrollability experienced early in life. Watt, Watson, and

Wilson (2007) corroborated Irwin's finding in a study that found a negative association between paranormal belief and perceived childhood control, although they cautioned that this relationship explains only a small amount of the variance in belief in reputed psi and paranormal events; they also corroborated that females have stronger paranormal belief than men. A sophisticated analysis by Lawrence, Edwards, Barraclough, Church, and Herrington (1995) tested different models related to Irwin's hypothesis. They found that although reported childhood trauma was associated with childhood fantasy, which was related to reports of paranormal experience, there was also a direct pathway between childhood trauma and paranormal experience. They also confirmed that paranormal experience tends to give rise to paranormal belief, rather than the other way around. Although Lawrence et al. do not seem to have been aware of his work, their study is the clearest general corroboration of Ferenczi's thesis that trauma will directly give rise to paranormal experience, which is also consistent with the proposal by Ring and Rosing (1990) that the association between early abuse and propensity to have a near-death experience can be explained by the alterations of consciousness first experienced around the time of trauma.

To the best of our knowledge, no study has previously looked at the interaction between hypnotizability, dissociation, and trauma in a psi test. Cardeña (2006) hypothesized that individuals high in fantasy or those high in dissociation may perform differently according to the psi tasks (e.g., those requiring good imaginative abilities such as the *ganzfeld* protocol may be more suitable to high fantasy rather than high dissociative individuals).

Belief in Psi

One way to obtain more reliable results in psi experiments is to identify which individuals may perform better in controlled experiments. Schmeidler (1952) divided her participants in two groups, "sheep and goats," on the basis of their belief in psi. Sheep are those who believe in the possibility of psi in general or, more specifically, in the experiment in which they were about to participate; goats have the opposite expectation. The sheep-goat effect refers to the difference in performance on a psi task between the two groups, where sheep have been found to perform significantly better in psi tasks than goats, especially when asked specifically about the psi task in consideration. Under the hypothesis of no psi effects, of course, there should not be any difference in psi tasks between the groups, provided that the tasks prevented ordinary forms of information acquisition. Although the sheep-goat effect is typically small, it has been consistently replicated across studies. Lawrence (1993) reviewed 73 forced-choice ESP studies with 37 principal investigators. He found a small but highly significant sheep-goat effect; 24% of the studies found significant results, in contrast to the expected 5% by chance. The effect size did not covary with the study quality,

so he reported a robust effect for this relationship. As with all meta-analyses there is a risk for a possible file-drawer effect (i.e., nonsignificant studies remaining unpublished), although this seems unlikely given the paucity of research in the area and the amount of "filed" studies that would be needed to invalidate published findings. Thus, there is additional reason to believe that, because they tend to believe more in psi, highly hypnotizable and dissociative individuals may perform better in psi tasks than those low in hypnotizability and/or dissociation (Kumar & Pekala, 2001).

Experimenter Effects

In all branches of science, some experimenters seem to be consistently more successful than others in obtaining significant results. In general there is a significant relationship between the experimenter's actions on the task and the participants' performance. Rosenthal (1966) has also discussed observer errors, interpretation errors and fraud as potential artifacts in research. Silverman, Shulman, and Wiesenthal (1972) found that different experimenters obtained different responses from participants even on a self-rating inventory. Silverman (1974) also conducted a survey to investigate the numbers of experimenters in research. The sample consisted of articles published in three APA journals between October 1968 and September 1969. Only 60 out of 300 studies included more than one experimenter. In 20 cases it was just a matter of convenience with no attention paid toward counterbalancing or measuring a possible experimenter effect. A more recent survey in various disciplines suggested that the use of "masked" methodology is very rare in other disciplines than parapsychology (Sheldrake, 1998). Thus, despite the evidence for experimenter effects, in practice not much attention is given to this source of variance in research.

With respect to psi, Smith (2003a) found significant correlations between psi-conduciveness, self-rated by researchers who had conducted experiments, and reported beliefs in various aspects of psi. Both psychological (e.g., how the experimenter interacts with the participant) and parapsychological (e.g., the experimenter using his or her own putative psi ability) factors may underlie the differences in the data (Smith, 2003b). Wiseman and Schlitz (1997) examined the experimenter effect by conducting a joint study in which a skeptic and a proponent acted as experimenters for two sets of trials. In this experiment electrodermal activity was used to measure whether participants could detect remote staring. Participants who were run by the proponent were significantly more physiologically reactive in stare than nonstare trials compared to participants that were run by the skeptic. The findings were replicated in a second study, but a third experiment yielded nonsignificant results (Wiseman & Schlitz, 1999; Schlitz, Wiseman, Radin, & Watt, 2006).

Our Study

The purpose of this study was to examine whether hypnotizability, dissociation, and belief in psi predict performance in a precognition task. We evaluated the level of hypnotizability and dissociation, but no hypnotic induction was conducted. Therefore, any differences between low and high hypnotizable individuals would be caused by this trait and not by a hypnotic procedure and its possible effect on the state of consciousness. We also included as a variable belief in psi. Two experimenters ran separate sets of trials and their belief in psi was measured; thus another variable was the possible effect of the experimenter that ran the sessions.

We had four hypotheses. The first one was that individuals would remember more words to which they would be exposed again in the future. The second was that high hypnotizables would perform better than lows in the precognitive memory test. The third was that high hypnotizable, high dissociators would perform better than the low dissociator highs. The fourth was that belief in psi would correlate positively with performance on the task.

Method

Participants

Participants in this experiment were recruited from a list of people who had previously taken part in a hypnosis study and consisted of those high or low in hypnotizability. The participants from each group were spread out on different days for balancing purposes. The experimenters were masked to group identity until the completion of data collection. The number of participants was prespecified as $N = 30$ (Marcusson-Clavertz & Wasmuth, 2008) for completion of a thesis. After those initial analyses were done, however, 8 additional participants were recruited to increase the power of the study. For all 38 participants, age ranged from 19 to 65 years ($M = 24.11$, $SD = 7.28$). There were 19 high hypnotizables (6 males, 13 females; $M_{Age} = 23.21$, $SD = 2.25$), 7 (6 females, 1 male; $M_{Age} = 24.00$, $SD = 2.00$) of whom also scored high in dissociation, whereas 10 scored low in dissociation (5 males, 5 females; $M_{Age} = 23.00$, $SD = 2.49$) and there were no data on dissociation for the remaining 2 high hypnotizables. We did not measure dissociation among the low hypnotizables because we have found almost no low hypnotizables that also score high in dissociation, as have other studies (e.g., Putnam, Helmers, Horowitz, & Trickett, 1995). There were also 19 low hypnotizables (11 males, 8 females; $M_{Age} = 25.00$, $SD = 10.11$). The participants were mainly Lund university undergraduates. All participants gave informed consent to participate and were compensated with a cinema ticket.

Experimenters

D. C. acted as the experimenter for 24 trials (12 high, 12 low) whereas J. W. was the experimenter for 14 (7 high, 7 low) trials. The experimenters were students and hold slightly different views on parapsychology, with D. C. being more positive to the research in general. J.W. had a more skeptical view but did not rule out the possibility of psi phenomena. D. C. scored 14 out of 32 on the Australian Sheep-Goat Scale (ASGS) and J. W. scored 8. The first author of this paper is generally supportive of the psi hypothesis and scored 23 in the ASGS but did not interact with the participants.

Materials

Precognitive memory task. Precognitive memory was measured with a computer program created by Bem (2008b). The program includes a Filemaker database engine. The task was run on a PC computer in a sound-attenuated room. In the task, participants first experienced a 3-min relaxation period in which they were shown Hubble images of the galaxy accompanied by relaxing music. After the relaxation period we asked the participants to visualize the referent to 48 English nouns, shown in succession. The nouns came from four categories (foods, animals, occupations, and clothes). Each word was presented for 3 s with interstimulus intervals of 1 s. Next, participants were administered an automated surprise recall test for which they were asked to recall as many of the words as possible within a 5-min period. The recall test was then followed by a practice session. Both the selection of practice words and the practice session occurred following the completion of the recall test; thus the participants could not know which words were practice words at the time of the first recall test.

Half of the words from the original list were then randomly selected and grouped into a list (the practice words). The remaining half were not shown again and thus served as control words. The practice words were divided into four lists with six words each. Each list consisted of words from one of the four categories. The participants were first exposed to one of these lists and asked to memorize the words. When they clicked on the "ready" button, the list was replaced by six empty slots for which the participants were asked to fill in the correct words. If the participants forgot any of the words, they had the opportunity to click on a button that revealed the list again for a brief amount of time. This repetition could be done until all slots were filled. The program did not monitor their answers at this stage except that they had to at least write something in all six slots before they were asked to recall six words from a category. When this criterion was met and the participants clicked on an "OK" button, the practice continued with the remaining three lists.

Australian Sheep-Goat Scale. The ASGS (Thalbourne & Delin, 1993) is a Rasch-scaled questionnaire with 16 items relating to belief in

psi phenomena. In this version two items relating to belief in afterlife are excluded. Items consist of two bipolar statements. The ASGS uses a visual analog scale; participants make a mark on a line above the two statements. Items are scored from zero to two points, depending on where the mark is placed. Total scores range from 0 to 32, with greater scores reflecting increased belief in psi phenomena. The 18-item version of the ASGS was found to have strong test-retest reliability (Thalbourne & Delin, 1993). The ASGS had good internal consistency ($\alpha = .94$) in this study.

Waterloo-Stanford Group Scale of Hypnotic Susceptibility (WSGC). The WSGC (Bowers, 1993, 1998) is a group-version of the Stanford Hypnotic Susceptibility Scale, Form C (SHSS:C; Weitzenhoffer & Hilgard, 1962) and has strong psychometric properties (Bowers, 1993, 1998). The scale includes 12 dichotomously-scored suggestions including ideomotor, challenge, and cognitive-perceptual suggestions. Scores range from 0 to 12 and we defined those scoring 0-4 as low hypnotizables and those scoring 8-12 as high hypnotizables.

Dissociative Experiences Scale (DES). The Swedish version (Körlin, Edman, & Nybäck, 2007) of the DES (Bernstein & Putnam, 1986; Carlson & Putnam, 1993) is a 28-item scale with good psychometric properties in which participants rate the percentage of time they experience different dissociative phenomena. The final score reflects the average percentage of the items. Individuals were categorized as high dissociators if they scored above a cutoff value of 20.

Life Stressor Checklist-Revised. The LSC-R (Wolfe & Kimerling, 1997) is a valid and reliable 30-item questionnaire that assesses the number and impact of various traumatic and adverse events throughout a person's life. We used the number of checked items as the overall score for this measure.

Procedure

The WSGC had been previously administered by either the first author or another experimenter, Devin Terhune. The LSC-R had also been previously filled out by 21 participants. For the next stage of the study, volunteers were recruited for a study involving reading English words and looking at faces (the latter for a different study). There was no mention of psi or hypnotizability until the end of the experiment. The consent form, however, mentioned that the purpose of data gathering could not be revealed until the end of the experiment. Participants were tested individually in laboratory sessions that lasted approximately 20 min. The instructions were to listen to the relaxation music and then answer the English words test presented by the computer at the participants' ease. The chair and the computer were arranged so that they would be comfortable for each participant and lights in the room were turned down, making the laboratory semidarkened. After the completion of the task, the ASGS was administered to participants. At the

completion of data collection, participants were debriefed by e-mail about the real purpose of the experiment and were provided with feedback on how they had done on the precognitive test.

After about one third of our participants had been run, we were contacted by the developer of the program, who suggested that we change to a new version that included a slightly different practice test. Because it was not practical to change programs at that point, we decided instead to analyze the data in segments to ascertain whether this new knowledge might affect the results; thus, before looking at the results, we divided the data for the first 30 participants in thirds, to which we added a fourth segment when we gathered additional data.

Statistical Analyses

Data were weighted; that is, the scores used were the difference between the number of practice words recalled from the number of control words recalled multiplied by the participant's overall score, to control for total words recalled (Bem, 2008a). We used parametric tests for analyses where all required assumptions were met and Kendall tau-b coefficients to evaluate correlations between the DES and other variables, given the nonnormal distribution of the DES reported in previous research (Cardeña, 2008b) and also observed in our data. We assessed the effect of dissociation/hypnotizability on other variables through ANOVAs, planned comparisons and Tukey's HSD test. We also conducted a regression analysis on the weighted scores to evaluate performance across sessions. All analyses were done in SPSS (16.0). An alpha level of .05, two-tailed was used for all statistical tests, and Chauvenet's criterion (Taylor, 1997) was used to determine the presence of any possible outlier, for exclusion from the relevant analyses.

Results

Demographic and Related Variables

We evaluated the correlations between age, sex, history of trauma, belief in psi, dissociation, and number of words recalled. Belief in psi had significant correlations with the tendency to dissociate, $\tau(22) = .53$, $p < .001$, report more traumatic events, $r(19) = .46$, $p = .04$, and with being female, $r(36) = -.39$, $p = .02$. History of trauma was further associated with being older, $r(19) = .62$, $p = .002$ and with fewer words recalled, $r(19) = -.51$, $p = .02$. The only other significant correlation were between dissociation and history of trauma, $\tau(19) = .33$, $p = .04$, and dissociation and being female, $\tau(22) = -.35$, $p = .04$.

Age, sex, trauma history and belief in psi were not significantly related to performance on the psi task ($p > .3$ for all analyses), so these variables were not used further in analyses of the main dependent variable.

However, after we blocked the sample by dissociation we found that the groups differed on their belief in psi, $F(2, 32) = 5.48$, $p = .009$, $\eta_p^2 = .26$. High hypnotizables who also scored high in dissociation (henceforth "HdHys") exhibited stronger belief in psi than low hypnotizables (henceforth "Lows"; $M = 20.00$, $SD = 4.86$ vs. $M = 8.89$, $SD = 8.15$; planned contrast $p = .002$), and marginally than high hypnotizables low in dissociation (henceforth "LdHys"; $M = 12.56$, $SD = 8.02$; planned contrast $p = .06$). There was no difference between Lows and LdHys (Tukey's HSD, $p = .47$).

Performance on the Precognition Task

On average, participants recalled 0.08 more control than practice words, and the *t* test showed no significant precognitive effect, $t(37) = -0.27$, $p = .79$. High hypnotizables were found to exhibit lower scores ($M = - 1.76$, $SD = 11.63$) than low hypnotizables ($M = 0.77$, $SD = 10.95$), but the difference was not significant, $F(1, 35) = 0.35$, $p = .56$, $\eta_p^2 = 0.01$. The weighted scores for sessions run by D. C. were in the predicted direction ($M = 0.27$, $SD = 11.86$), whereas those for J. W. were in the opposite direction ($M = - 1.81$, $SD = 10.30$), but the difference was nonsignificant, $F(1, 36) = 0.30$, $p = .59$, $\eta_p^2 = 0.01$.

When high hypnotizables were grouped according to dissociative tendencies, an ANOVA revealed a significant main effect of Group, $F(2, 32) = 3.81$, $p < .05$, $\eta_p^2 = .19$. Low-dissociative high hypnotizables (LdHy, $M = - 9.59$, $SD = 6.77$) had lower scores than low hypnotizables (Lows, $M = 0.77$, $SD = 10.95$; Tukey HSD $p = .030$) and marginally lower scores than dissociative high hypnotizables (HdHy) ($M = 0.15$, $SD = 8.11$; planned contrast, $p = .05$); Lows did not differ from HdHys (planned contrast, $p = .89$). See Figure 1. Analyses of deviation from chance showed that LdHys recalled a higher ratio of control words than chance would predict, $t(8) = -4.25$, $p = .003$, but neither HdHys, $t(2) = 0.78$, $p > .05$, nor Lows deviated from chance, $t(14) = 0.87$, $p > .05$.

Decline Effect

The practice-control difference was positive in the first sequence, but negative in the last three (see Table 1).

This pattern differed across groups. Whereas the scores of the HdHys, $r(5) = - .84$, $p < .05$, and Lows, $r(17) = -.53$, $p < .05$, declined across sessions, $r(17) = -.53$, $p < .05$, the scores for the LdHys increased nonsignificantly, $r(7) = .05$, $p = .90$ (see Figure 2). Thus we decided to run a regression analysis partialing out grouping by hypnotizability/dissociation, and obtained a significant decline effect, $r(34) = - .36$, $p = .03$ (see Figure 3). Exploratory analyses showed that only the experimenter that was most invested in the project (D. C.) produced a significant decline effect, after controlling for grouping, $r(22) = -.42$, $p = .04$.

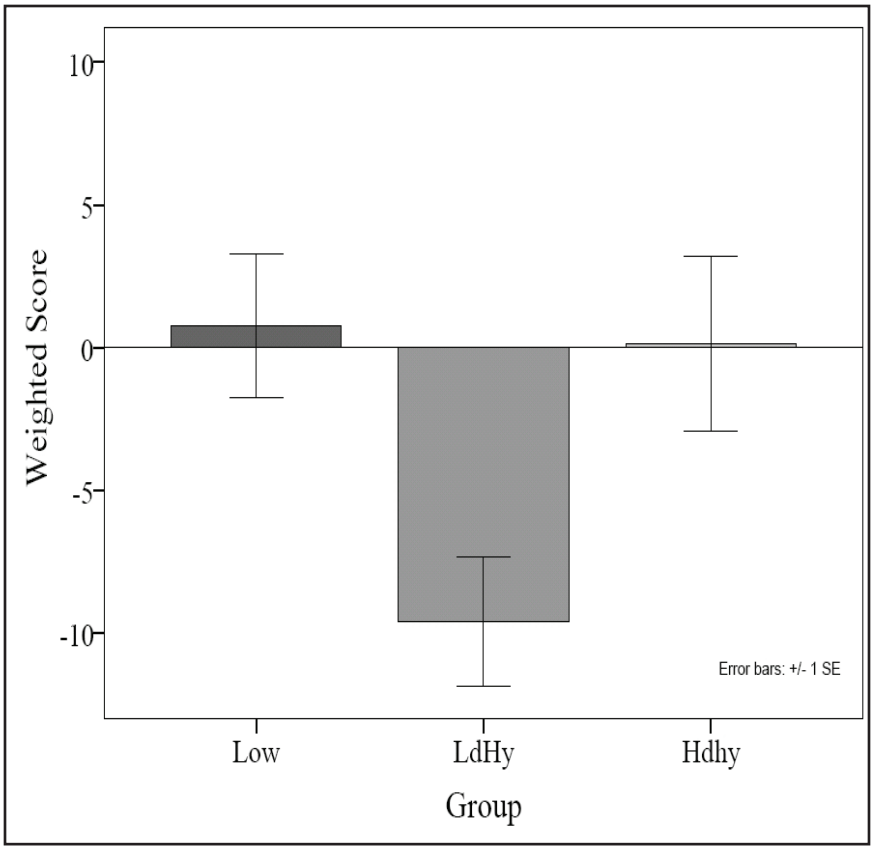

Figure 1. Scores grouped by hypnotizability/dissociation.

Table 1
Scores As a Function of Sequence

Sequence	*N*	*M*	*SD*
1	10	5.10	11.24
2	10	-2.52	9.98
3	10	-1.55	11.98
4	8	-3.67	11.22

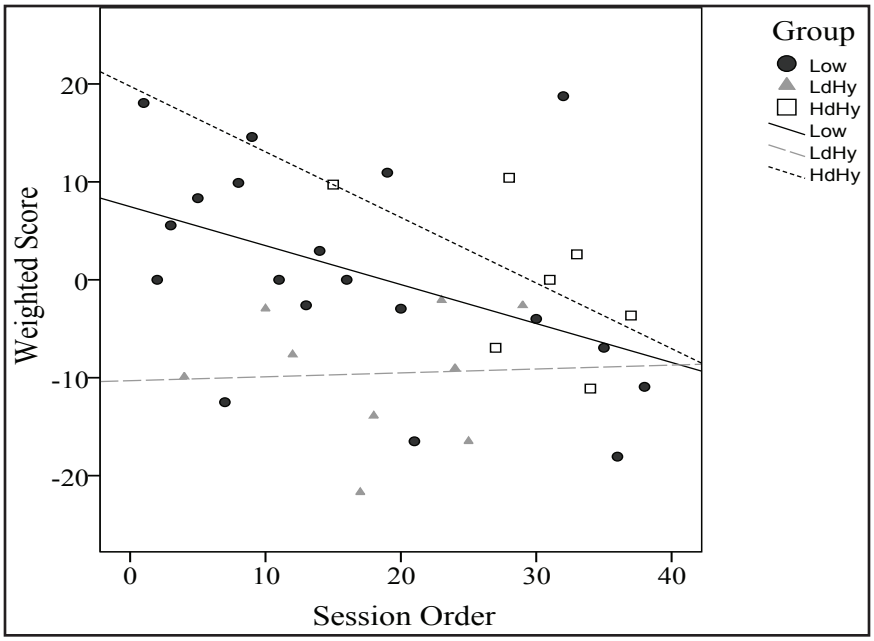

Figure 2. Scatterplot of individual scores stratified by hypnotizability and dissociation, by session order.

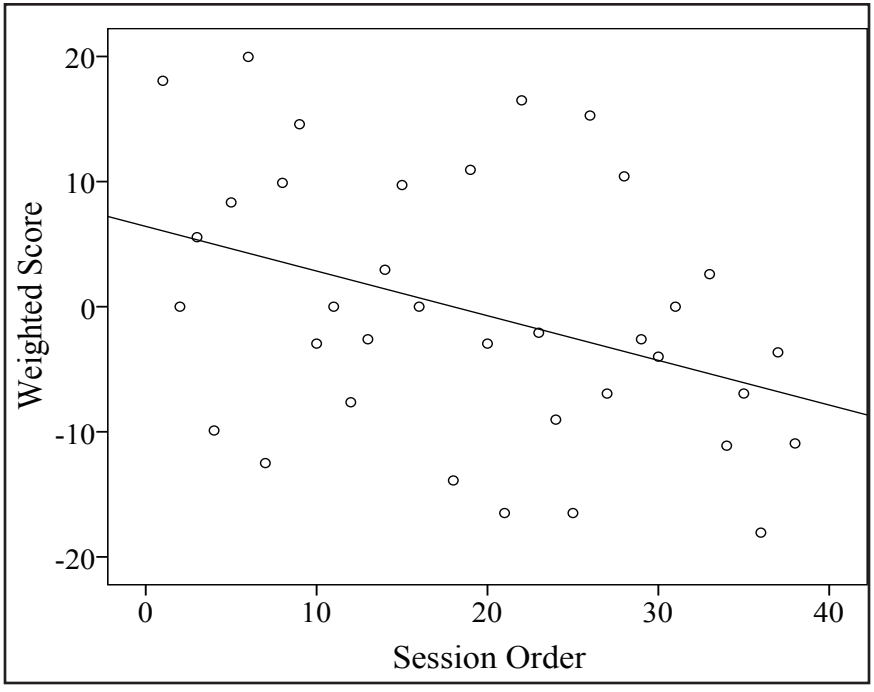

Figure 3. General scatterplot of individual scores by session order.

Discussion

In replication of previous research, we found that having a history of trauma, being more dissociative and hypnotizable, and being a female were positively associated with believing in psi. We also replicated a recent finding with adults that a history of trauma is related to poorer working memory (El-Hage, Gaillard, Isingrini, & Belzung, 2006). Taking the group as a whole, we did not replicate Bem's previous research (Bem, 2008a), and contrary to what we hypothesized, neither hypnotizability nor belief in psi as main effects predicted performance on the precognitive memory test. However, the interaction of hypnotizability and dissociation did produce significant results, with the LdHys scoring worse than chance and lower than the other two groups, which suggests that dissociativity may be a mediator of the possible relationship between hypnotizability and performance in psi tasks. Other areas in psychology (e.g., McFatter, 1994) have revealed that interactions between traits have different or increased explanatory power than the effect of those traits alone.

It is puzzling why LdHys psi-missed, as other research has found hypnosis (rather than hypnotizability, though) to be associated with psi scoring. A possible explanation is that we encountered a psi *differential effect,* a phenomenon in which comparing contrasting conditions produces one condition to score significantly better or worse than chance, and the other one to have the opposite effect or none at all (Irwin & Watt, 2007). We did not tell participants before debriefing that they were participating in a psi test, so it might have been that nonconsciously they misconstrued their task and recalled more of the control than the practice words. A possible factor in the psi-missing effect was the fact that we continued to use the same test even after becoming aware that there might be a problem with it. This might have created a negative psychological set in one or both experimenters, somehow affecting more the LdHys, perhaps making them less comfortable in the situation, a factor that Palmer (1997) has discussed as a mediator of whether participants tend to psi-hit or psi-miss. That the first third of the data was suggestive of psi-hitting whereas the other segments reversed direction supports this hypothesis.

Low hypnotizables scored almost exactly at chance; HdHys also scored at chance, contrary to our expectations, but the positive correlation between trauma and dissociation, and the negative correlation between trauma and recall suggests that psi memory tests may not be ideal with this group. In any event, we think that further research, especially with high hypnotizables, is warranted, although they should be grouped by dissociativity as well. Bem (2008a) recommended using individuals high in novelty seeking, and our literature search did not reveal any studies evaluating a relationship between novelty seeking and hypnotizability or psi beliefs or abilities. A linear regression analysis on data of a group of alcohol dependents showed that depression was not predicted by either novelty

seeking or self-transcendence (Evren, Sar, & Dalbudak, 2008). Because self-transcendence is related to hypnotizability (Cardeña & Terhune, 2008), the only data we know of suggests that novelty seeking is most likely a different process than the ones we investigated in this study.

The data showed a decline effect, in accord with much of the research in psi (cf. Irwin & Watt, 2007). Figure 2 suggests that the effect was differential in that the group that was in the direction of psi-missing at the beginning tended to improve, whereas the groups that were in the direction of psi-hitting, followed the opposite pattern. This seemed to be even clearer in the fourth stage of data collection, after we had looked at the initial data. At that stage we obtained outlier or near-outlier scores that went against the previous pattern. Although by no means a test of von Lucadou's observation systems theory, our results are consistent with it (cf. Lucadou, Römer, & Walach, 2007).

Finally, although not significantly, we found some support for the notion of an experimenter effect in that the experimenter with a stronger belief in psi obtained scores in the direction of psi-hitting, whereas the other experimenter got scores in the opposite direction. However, the first experimenter was also the only one that showed a significant decline effect. In any case, this study does suggest the need to at least evaluate the effect of experimenters in a psi study.

This study had a number of limitations that should be acknowledged. First, the N, especially after grouping, was modest, so the experiment had limited power. Also, with small *ns* the significance of the data can easily shift with inclusion of new data; thus our results should be considered tentative and in need of replication. Another issue to consider is that the task was completely in English and included words that the participants were asked to visualize. In general, the words were not very complicated but a few of them might have been difficult for Swedish volunteers to understand (e.g., "mortician" and "parka"). If the participants did not understand the words, they would not have been able to visualize them. Hence, the way the participants processed the words may have differed. Some may have activated working rather than long-term memory. According to Irwin's (1979) interpretation of Roll's memory trace theory, that might have influenced how well the participants performed on the test. It would therefore be optimal that additional studies using this task be conducted in the participant's native tongue. The language problem might have also depressed the scores of our other measures, although probably not to a large degree (cf. Cardeña, Kallio, Terhune, Buratti, & Lööf, 2007).

Second, it is possible that far from everyone gets relaxed by the type of stimuli that the relaxation video included. For some individuals, such relaxation could be associated with traumatic/dissociative experiences and might have the opposite effect. This question should be evaluated in future research. Also, there was a constant humming sound coming from another computer in the room; although the sound was fairly low, it might have served as another distraction.

Another limitation might be the nature of the task. Presentiment and other psi studies suggest that emotionality is an important factor (Cardeña, 2008a; Hinterberger, Studer, Jäger, Haverty-Stacke, & Walach, 2007). There was no emotional component in our task. It may therefore be a good idea to expose the participants to stimuli of greater salience in further experiments of this kind. Another issue worth pursuing is to design a study with consequences that may be desirable (or not) to the participants, as per Ferenczi's hypothesis (1955) and the PMIR model (Stanford, 1977). And, as mentioned earlier, memory tasks may not be ideal with individuals who are likely to have more traumatic events in the past and to dissociate. The creator of the task also mentioned to us (Bem, personal communication, August 2009) that the program as he had originally sent it to us did not produce psi effects, as compared with other versions of the program, which is why he had wanted us to modify it. This factor may have affected the overall performance in the study, but it is worth pointing out that we nonetheless obtained some significant effects when grouping and when analyzing a decline effect.

In parapsychology the role of the experimenter has been emphasized (Smith, 2003b); particularly the experimenter-participant interaction has been brought into focus. Therefore the inexperience of the experimenters and the way they interacted with the participants may have affected the outcome of the study. Furthermore, both researchers had a moderate score on the ASGS, which could have influenced the outcome as well. It is hard to pinpoint exactly what experimental factors could have made an impact on the outcome, as little is known about the importance of personal characteristics in experimenters (Watt & Wiseman, 2002). Although the program was developed to minimize the experimenter role with the test being run mainly on computer, the initial contact with the experimenter may still have an important role to play in participants' performance.

Finally, hypnotizability may have had a greater effect in the context of an actual hypnotic induction, as is suggested by reports of an advantage of hypnosis over control conditions and of specific hypnotic suggestions (Tressoldi & Del Prete, 2007; see also Cardeña, in press). Studies with and without a hypnotic procedure but also measuring hypnotizability and dissociation should be conducted in the future. In sum, we found a more complicated set of results than we had originally predicted, but one pregnant with suggestions for future research on the possible relationship between hypnosis, dissociation, and psi.

References

Barber, T. X. (1999). A comprehensive three-dimensional theory of hypnosis. In I. Kirsch, A. Capafons, E. Cardeña-Buelna, & S. Amigo (Eds.), *Clinical hypnosis and self-regulation: Cognitive-behavioral perspectives* (pp. 21–48). Washington, DC: American Psychological Association.

Barrett, D. (1990). Deep trance subjects: A schema of two distinct subgroups. In R. G. Kunzendorf (Ed.), *Mental imagery* (pp. 101–112). New York: Plenum Press.

Bem, D. J. (2008a). Feeling the future III: Additional experimental effect for apparent retroactive influences on cognition and affect. *Proceedings of Presented Papers: The Parapsychological Association 51st Annual Convention*, 24–32.

Bem, D. J. (2008b). *Instructions for running the precognitive memory (PM 2.0) study and computer program.* Retrieved March 2008 from http://dbem/psistuff.

Bernstein, E. M., & Putnam, F. W. (1986). Development, reliability, and validity of a dissociation scale. *Journal of Nervous and Mental Disease*, **174**, 727–735.

Bowers, K. S. (1993). The Waterloo-Stanford Group C Scale of Hypnotic Susceptibility: Normative and comparative data. *International Journal of Clinical and Experimental Hypnosis,* **41**, 35–46.

Bowers, K. S. (1998). Waterloo-Stanford Group Scale of Hypnotic Susceptibility, Form C: Manual and response booklet. *International Journal of Clinical and Experimental Hypnosis,* **46**, 250–268.

Cardeña, E. (1994). The domain of dissociation. In S. J. Lynn & J. W. Rhue (Eds.) *Dissociation: Clinical and theoretical perspectives* (pp. 15–31). New York: Guilford.

Cardeña, E. (1996). "Just floating on the sky." A comparison of shamanic and hypnotic phenomenology. In R. Quekelbherge & D. Eigner (Eds.), *6th Jahrbuch für Transkulturelle Medizin und Psychotherapie* (pp. 367–380). Berlin: Verlag für Wissenschaft und Bildung.

Cardeña, E. (1998). Dissociation and PSI: What are the links? [Abstract]. In N. L. Zingrone, M. J., Schlitz, C. S. Alvarado, & J. Milton (Eds.), *Research in Parapsychology 1993* (pp. 153–156). Lanham, MD: Scarecrow Press.

Cardeña, E. (2006). T. X. Barber's typology implications for parapsychology. *Journal of Parapsychology,* **10**, 226.

Cardeña, E. (2008a). Consciousness and emotions as interpersonal and transpersonal systems. *Journal of Consciousness Studies*, **15**, 249–263.

Cardeña, E. (2008b). Dissociative disorders measures. In A. J. Rush, M. B. First, & D. Becker (Eds.), *Handbook of psychiatric measures* (2nd ed., pp. 587–599). Washington, DC: American Psychiatric Press.

Cardeña, E. (in press). Anomalous experiences during deep hypnosis. In M. Smith (Ed.), *Developing perspectives on anomalous experience.*

Cardeña, E., Butler, L. D., & Spiegel, D. (2003). Stress disorders. In G. Stricker & T. Widiger (Eds.), *Handbook of psychology*: Vol. 8. (pp. 229–249). New York: John Wiley.

Cardeña, E., Kallio, S., Terhune, D., Buratti, S., & Lööf, A. (2007). The effect of translation and sex on hypnotizability testing. *Contemporary Hypnosis,* **24**, 154–160 (see also erratum in *Contemporary Hypnosis,* **25**, 110).

Cardeña, E., & Terhune, D. (2008). A distinct personality trait? The relationship between hypnotizability, absorption, self-transcendence, and mental boundaries. *Proceedings of Presented Papers: The Parapsychological Association 51st Annual Convention*, 61–73.

Carlson, E. B., & Putnam, F. W. (1993). An update on the Dissociative Experiences Scale. *Dissociation,* **6**, 16–27.

El-Hage, W., Gaillard, P., Isingrini, M., & Belzung, C. (2006). Trauma-related deficits in working memory. *Cognitive Neuropsychiatry,* **11**, 33–46.

Evren, C., Sar, V., & Dalbudak, E. (2008). Temperament, character, and dissociation among detoxified male inpatients with alcohol dependency. *Journal of Clinical Psychology,* **64**, 717–727.

Ferenczi, S. (1955). *Final contributions to the problems & methods of psycho-analysis.* (M. Balint, Ed.). London: Hogarth Press.

Hinterberger, T., Studer, P., Jäger, M., Haverty-Stacke, C., & Walach, H. (2007). Can a slide-show presentiment effect be discovered in brain electrical activity? *Journal of the Society for Psychical Research,* **71**, 148–166.

Honorton, C., & Ferrari, D. C. (1989). "Future telling": A meta-analysis of forced-choice precognition experiments, 1935–1987. *Journal of Parapsychology,* **53**, 281–308.

Irwin, H. J. (1979). On directional inconsistency in the correlation between ESP and memory. *Journal of Parapsychology,* **43**, 31–39.

Irwin, H. J. (1994a). Childhood trauma and the origins of paranormal belief: A constructive replication. *Psychological Reports,* **74**, 107–111.

Irwin, H. J. (1994b). Paranormal belief and proneness to dissociation. *Psychological Reports,* **75**, 1344–1346.

Irwin, H. J., & Watt, C. A. (2007). *An introduction to parapsychology* (5th ed.). Jefferson, NC: McFarland.

Kogon, M. M., Jasiukaitis, P., Berardi, A., Gupta, M., Kosslyn, S. M., & Spiegel D. (1998). Imagery and hypnotizability revisited. *International Journal of Clinical and Experimental Hypnosis,* **46**, 363–370.

Körlin, D., Edman, G., & Nybäck, H. (2007). Reliability and validity of a Swedish version of the Dissociative Experiences Scale (DES-II). *Nordic Journal of Psychiatry,* **61**, 126–142.

Kumar, V. K., & Pekala, R. J. (2001). Relation of hypnosis-specific attitudes and behaviors to paranormal beliefs and experiences: A technical review. In J. Houran & R. Lange (Eds.), *Hauntings and poltergeists: Multidisciplinary perspectives* (pp. 260–279). Jefferson, NC: McFarland.

Lawrence, T. R. (1993). Gathering in the sheep and goats. A meta-analysis of forced choice sheep-goat ESP studies, 1947–1993. *Proceedings of Presented Papers: The Parapsychological Association 36th Annual Convention,* 75–86.

Lawrence, T. R., Edwards, C., Barraclough, N., Church, S., & Herrington, F. (1995). Modelling childhood causes of paranormal belief and experience: Childhood trauma and childhood fantasy. *Personality and Individual Differences,* **19**, 209–215.

Lucadou, W., Römer, H., & Walach, H. (2007) Synchronistic phenomena as entanglement correlations in Generalized Quantum Theory. *Journal of Consciousness Studies.* **14**, 50–74.

Lynn, S. J., & Ruhe, J. W. (1986). The fantasy-prone person: Hypnosis, imagination, and creativity. *Journal of Personality and Social Psychology,* **51**, 404–408.

Marcusson-Clavertz, D., & Wasmuth, J. (2008). Hypnotizability and belief in psi as predictors of precognition. Unpublished bachelor's thesis, Lund University, Sweden.

May, E., Bányai, É., Vassy, Z., & Faith, L. V. (2000). Hypnotic susceptibility as a predictor of anomalous cognition performance [Abstract]. *3º Simpósio da Fundação Bial: Aquém e Além do Cérebro,* 417.

McFatter, R. M. (1994). Interactions in predicting mood from extraversion and neuroticism. *Journal of Personality and Social Psychology,* **66**, 570–578.

Palmer, J. (1997). *The psychology of ESP: Magnitude vs. direction.* Paper presented at the First International Brazilian Congress of Parapsychology, Recife, Brazil.

Pekala, R., & Cardeña, E. (2000). Methodological issues in the study of altered states of consciousness and anomalous experiences. In E. Cardeña, S. J. Lynn, & S. Krippner (Eds.), *Varieties of anomalous experience: Examining the scientific evidence* (pp. 47–81). Washington, DC: American Psychological Association.

Putnam, F. W., Helmers, K., Horowitz, L. A., & Trickett, P. K. (1995). Dissociativity and hypnotizability in sexually abused girls. *Child Abuse & Neglect,* **19**, 645–655.

Ring, K., & Rosing, C. J. (1990). The Omega Project: An empirical study of the NDE-prone personality. *Journal of Near-Death Studies,* **8**, 211–239.

Rosenthal, R. (1966). *Experimenter effects in behavioral research.* NY: Appleton-Century-Crofts.

Schechter, E. I. (1984). Hypnotic induction vs. control conditions: Illustrating an approach to the evaluation of replicability in parapsychological data. *Journal of the American Society for Psychical Research,* **78**, 1–27.

Schlitz, M., Wiseman, R., Watt, C., & Radin, D. (2006). Of two minds: Sceptic-proponent collaboration within parapsychology. *British Journal of Psychology,* **97**, 313–322.

Schmeidler, G. (1952). Personal values and ESP scores. *Journal of Abnormal and Social Psychology,* **47**, 757–761.

Sharps, M. J., Matthews, J., & Asten, J. (2006). Cognition and belief in paranormal phenomena: Gestalt/feature-intensive processing theory and tendencies toward ADHD, depression, and dissociation. *Journal of Psychology,* **140**, 579–590.

Sheldrake, R. (1998). Experimenter effects in scientific research: How widely are they neglected? *Journal of Scientific Exploration,* **12**, 73–78.

Silverman, I. (1974). The experimenter: A (still) neglected stimulus object. *Canadian Psychologist,* **15**, 258–270.

Silverman, I., Shulman, A. D., & Wiesenthal, D. L. (1972). The experimenter as a source of variance in psychological research: Modelling and sex effects. *Journal of Personality and Social Psychology,* **21**, 219–227.

Smith, M. D. (2003a). The psychology of the "psi-conducive" experimenter: Personality, attitudes towards psi, and personal psi experience. *Journal of Parapsychology,* **67**, 117–128.

Smith, M. D. (2003b). The role of the experimenter in parapsychological research. In J. Alcock, J. Burns, & A. Freeman (Eds.), *Psi wars: Getting to grips with psi* (pp. 69–84). Exeter: Imprint Academic.

Stanford, R. G. (1977). Conceptual frameworks of contemporary psi research. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 823–858). New York: Van Nostrand Reinhold.

Stanford, R. G., & Stein, A. G. (1994). A meta-analysis of ESP studies contrasting hypnosis and a comparison condition. *Journal of Parapsychology,* **58**, 235–269.

Steinkamp, F. (2003). Does precognition foresee the future? Series 5, A laboratory replication. Series 6 and WWW replication. *Proceedings of Presented Papers: The Parapsychological Association 46th Annual Convention*, 257–268.

Taylor, E. (1983). *William James on exceptional mental states. The 1896 Lowell lectures*. Amherst: University of Massachusetts Press.

Taylor, J. R. (1997). *An introduction to error analysis* (2nd ed.). Sausalito, CA: University Science Books.

Tellegen, A., & Atkinson, G. (1974). Openness to absorbing and self-altering experiences ("absorption"), a trait related to hypnotic susceptibility. *Journal of Abnormal Psychology,* **83**, 268–277.

Thalbourne, M. A., & Delin, P. S. (1993). A new instrument for measuring the sheep-goat variable: Its psychometric properties and factor structure. *Journal of the Society for Psychical Research,* **59**, 172–186.

Tressoldi, P., & Del Prete, G. (2007). ESP under hypnosis: The role of induction instructions and personality characteristics. *Journal of Parapsychology,* **71**, 125–137.

Watt, C., Watson, S., & Wilson, L. (2007). Cognitive and psychological mediators of anxiety: Evidence from a study of paranormal belief and perceived child control. *Personality and Individual Differences,* **42**, 335–343.

Watt, C., & Wiseman, R. (2002). Experimenter differences in cognitive correlates of psi belief, and in psi. *Journal of Parapsychology*, **66**, 371–385.

Weitzenhoffer, A. M., & Hilgard, E. R. (1962). *Stanford Hypnotic Susceptibility Scale, Form C.* Palo Alto, CA: Consulting Psychologists Press.

White, R. W. (1937). Two types of hypnotic trance and their personality correlates. *Journal of Psychology,* **3**, 279–289.

Wiseman, R., & Schlitz, M. (1997). Experimenter effects and the remote detection of staring. *Journal of Parapsychology,* **61**, 197–207.

Wiseman, R., & Schlitz, M. (1999). Experimenter effects and the remote detection of staring: An attempted replication. *Proceedings of Presented Papers: The Parapsychological Association 42nd Annual Convention*, 471–479.

Wolfe, J., & Kimerling, R. (1997). Gender issues in the assessment of posttraumatic stress disorder. In J. P. Wilson & T. M. Keane (Eds.) *Assessing psychological trauma and PTSD* (pp. 192–238). New York: Guilford Press.

Center for Research on Consciousness and Anomalous Psychology (CERCAP)
Department of Psychology, Lund University
P.O. Box 213 SE-221 00
Lund, Sweden
Etzel.Cardena@psychology.lu.se

Abstracts In Other Languages

Spanish

HIPNOTIZABILIDAD Y DISOCIACIÓN COMO PREDICTORES DE DESEMPEÑO EN UNA PRUEBA DE PRECOGNICIÓN: UN ESTUDIO PILOTO

RESUMEN: Investigamos si la hipnotizabilidad, la disociación, y la creencia en psi estaban relacionadas con el desempeño en una prueba de precognición que medía si la práctica posterior aumentaba recuerdos anteriores. Los participantes fueron bajos (n = 19) y altos (n = 19, 7 tambien altos en disociación) en hipnotizabilidad, y completaron prueb as sobre creencias sobre psi mientras los experimentadores estaban ciegos en cuanto a los niveles de hipnotizabilidad y de disociación. No replicamos el efecto general de precognición y no hubo un efecto global para hipnotizabilidad, pero los hipnotizables altos que eran bajos en disociación obtuvieron una diferencia significativa de los que eran altos en disociación e hipnotizabilidad y de los que eran bajos en hipnotizabilidad, y obtuvieron puntuaciones significativas bajo lo esperado al azar. La creencia en

psi estuvo relacionada a la disociación y a la historia de trauma, y el trauma y la memoria de trabajo (working memory) obtuvieron una correlación negativa. Después de procesar alrededor de una tercera parte de los participantes el desarrollador del programa se puso en contacto con nosotros y nos solicitó que usaramos una versión modificada del programa. No cambiamos el protocolo pero decidimos analizar los datos en segmentos antes y después de que nos solicitaran usar otro programa. Aunque no fue significativa, la primera tercera parte de los datos mostró una dirección hacia tener aciertos mientras que otros segmentos mostraron una dirección opuesta. Un análisis de regresión controlando por grupos mostró un efecto de declinación significativo. Nuestros resultados apoyan el valor de usar grupos especiales y sugieren que la disociación puede mediar los efectos de la hipnotizabilidad en la manifestación de psi.

French

HYPNOTISABILITE ET DISSOCIATION COMME PREDICTEURS DE PERFORMANCES DANS UNE TACHE DE PRECOGNITION: UNE ETUDE PILOTE

RESUME: Nous avons étudié si l'hypnotisabilité, la dissociation et la croyance au psi étaient liées à la performance dans une tâche de précognition qui mesure si une pratique ultérieure améliore le rappel de souvenirs antérieur. Les participants bas (n = 19) et hauts (n = 19, 7 également hauts en dissociation) en hypnotisabilité ont complété la tâche et une mesure de la croyance au psi sans que les expérimentateurs ne sachent leur niveau d'hypnotisabilité et de dissociation. Un effet de précognition générale ne fut pas répliqué et il n'y a pas d'effet global pour l'hypnotisabilité, mais les personnes ayant à la fois un score élevé en hypnotisabilité et bas en dissociation différaient significativement des personnes ayant un score élevé en dissociation et de celles ayant un score bas en hypnotisabilité, et obtenaient des résultats inférieurs au hasard. La croyance au psi fut liée à la dissociation et à une histoire de trauma, et le trauma et la mémoire de travail furent corrélés négativement. En ce qui concerne les caractéristiques de l'expérience elle-même, nous avons été contacté après avoir commencé les essais d'un tiers des participants par le développeur du programme qui nous a demandé d'utiliser une version modifiée. Nous n'avons pas changé notre protocole, mais nous avons décidé d'analyser les données par segments avant et après que nous ayons dû utiliser un programme différent. Bien que non significatif, le premier tiers des données était clairement en direction d'un succès psi tandis que les segments suivants allaient dans la direction opposée. Une analyse de régression contrôlant les groupes montra un effet de déclin significatif. Nos résultats encouragent l'utilisation de groupes sélectionnés et suggèrent que la dissociation pourrait médiatiser les effets de l'hypnotisabilité dans la performance psi.

German

HYPNOTISIERBARKEIT UND DISSOZIATION ALS PRÄDIKTOREN FÜR DAS ABSCHNEIDEN BEI EINER PRÄKOGNITIONSAUFGABE: EINE PILOT-STUDIE

ZUSAMMENFASSUNG: Wir untersuchten, ob sich Hypnotisierbarkeit, Dissoziation und der Glaube an Psi auf das Abschneiden bei einer Präkognitionsaufgabe auswirken, die messen sollte, ob eine spätere Übung eine vorherige Gedächtnisleistung verstärkt. Teilnehmer mit geringer (n = 19) und hoher (n = 19, 7 davon auch mit hoher Dissoziation) Hypnotisierbarkeit absolvierten die Aufgabe und füllten einen Fragebogen zur Messung des Glaubens an Psi aus, wobei die Experimentatoren in Bezug auf den jeweiligen Grad der Hypnotisierbarkeit und Dissoziation der Teilnehmer verblindet waren. Ein allgemeiner Präkognitionseffekt ließ sich nicht replizieren, und es zeigte sich kein Gesamteffekt in Bezug auf Hypnotisierbarkeit; leicht Hypnotisierbare mit geringen Dissoziationswerten unterschieden sich jedoch signifikant von dissoziierten leicht Hypnotisierbaren und von schlecht Hypnotisierbaren und schnitten zudem unterzufällig ab. Der Glaube an Psi hing mit Dissoziation und der Geschichte des Traumas zusammen, und Trauma und Arbeitsgedächtnis waren negativ korreliert. Was besondere Merkmale des Experiments selbst betraf, so wurden wir – nachdem wir über ein Drittel der Teilnehmer getestet hatten – vom Programmentwickler kontaktiert mit der Bitte, eine modifizierte Version zu verwenden. Wir änderten nicht unser Protokoll, entschlossen uns aber, die Daten in Segmenten auszuwerten – entweder vor dem Zeitpunkt oder nachdem wir gebeten wurden, ein unterschiedliches Programm zu verwenden. Obwohl nicht signifikant, deutete das erste Drittel der Daten eindeutig in Richtung eines Psi-Hitting, während die späteren Segmente in die gegenteilige Richtung gingen. Eine Regressionsanalyse, in der der Gruppeneinfluß kontrolliert wurde, zeigte einen signifikanten Absinkungseffekt. Unsere Ergebnisse unterstützen die Annahme, dass es wichtig ist, mit ausgewählten Gruppen zu arbeiten und legen die Vermutung nahe, daß Dissoziation die Wirkungen von Hypnotisierbarkeit in Bezug auf Psi-Trefferleistungen vermittelt.

OBITUARY

GERTRUDE R. SCHMEIDLER

1912–2009

By Ruth Reinsel

Gertrude R. Schmeidler, who resided for most of her life in Hastings-on-Hudson, New York, passed away in Whittier, California, on March 9, 2009, at the age of 96.

So many words come to mind when thinking of Gertrude: modest and unassuming; soft-spoken, even shy, and hesitant; very polite, always a lady. Even in the most contentious faculty meetings, she never raised her voice, and often played the role of reasonable mediator. She always had time to listen to her students, and always took their ideas seriously. Unlike so many mentors, she never imposed her own research agenda on her students, but allowed them to develop their own ideas. She was one of a kind, and she will be sorely missed.

I had the privilege of doing post-graduate work in parapsychology with Gertrude at the City College of New York. I was by no means her most talented student, and there are others who worked more closely with her than I did. So I offer this appreciation of her life with the realization that others will surely add their own perceptions and fond memories. She was affectionately known to students and colleagues simply as "Gertrude," so I will continue to refer to her in this informal way. Although I knew Gertrude for over 30 years, she never spoke very much about herself. I learned much of what follows from her reminiscences when interviewed by Professor Larry Nyman of City College of New York, who was compiling an oral history

of the psychology department there (Nyman, 1976). She also shared her reflections on her career in parapsychology in an interview with Rosemarie Pilkington (Pilkington, 1987). This obituary will be divided into two major sections; the first is biographical in nature and the second considers her work in both parapsychology and mainstream psychology, with thoughts on her legacy.

Biography

Early Years

Gertrude was born in New York City in 1912. Her father was a lawyer, and her mother was determined that her only child would receive a college education. In high school, she wanted to become a poet. Then she was "tempted" by physics. But in college she chose to major in psychology, because she took a laboratory course that seemed so "easy and fun" and because there were so many unanswered questions and interesting problems where, she felt, one could really make a contribution. I suspect this is also what drew her to parapsychology. She received her BA from Smith College in 1932 and went on to work for a time as a statistician at Worcester State Hospital, living in a dormitory and receiving room and board but no wages. This was during the Depression; she writes that she was glad just to have a job. While in Worcester, she was accepted to the M.A. program at Clark University, emerging one year later with "a strong aversion to rigid old-line behaviorism" (Nyman, 1976). For a while thereafter she did research in the laboratory of Robert Woodworth at Columbia University. She went on to study experimental psychology at Harvard and received her doctorate in 1935.

Most of her early research was in visual perception and memory (for example, Schmeidler, 1939). Gertrude was known for her careful experimental technique. For her dissertation, she told me that she started with two experiments that gave completely opposite results, whereas theory predicted that they should be the same. Then she started out to replicate one experiment and changed one variable at a time, step by step, and finished by replicating the second experiment.

Gertrude was also a careful observer, and she used the opportunities that were at hand. When she was pregnant with her twin sons, she noticed that when she was active, the babies kicked a lot; when she was at rest, so were the babies. She began to keep a formal log and published an article in *Child Development* titled "The Relation of Fetal Activity to the Activity of the Mother" (Schmeidler, 1941).

War Work

Gertrude married Robert Schmeidler in 1937 and they raised four children. She often told the story of how she went to pick up a copy of her

doctoral degree. In those years, when women scholars were less common than they are today, the assistant in the Harvard office was reluctant to give her the diploma, because it was in the name of Gertrude Raffel. Gertrude had a difficult time explaining to her that she was now Gertrude Schmeidler. "I got married, and Schmeidler is my husband's name. Look, he's right over there!" Robert, a chemist, was in the Navy then and was sent overseas after Pearl Harbor. Gertrude was kept busy with the babies during the days, but had too much time on her hands in the evenings. She was lonely and worried about her husband off at war. So she decided to take a long shot; she wrote to Gordon Allport at Harvard University and asked if he had a part-time job for her.

When she received his affirmative response, Gertrude moved to Cambridge, Massachusetts, and joined a group of social psychologists around the country working to improve civilian morale. She called this her "War Work." This is how Gertrude became a propaganda specialist funded by the Department of Defense! This work resulted in two papers she coauthored with Gordon Allport, one in *Psychological Bulletin* (Allport & Schmeidler, 1943) and the other in the *Journal of Social Psychology* (Schmeidler & Allport, 1944).

Influence of Gardner Murphy

After the war, Gertrude was greatly influenced by Gardner Murphy whom she met at Harvard, though she had actually met him years earlier at Columbia and had a discussion with him about psychical research. Murphy was known for his keen intelligence and critical mind, and had an encyclopedic knowledge of multiple fields. He had been educated at Yale and Harvard, and received his PhD in Psychology from Columbia in 1923. Murphy was simultaneously an eminent psychologist and a leading parapsychologist. He became President of the Society for Psychical Research in 1949—one of only three Americans to hold that post up to that time. In this, he followed in the footsteps of another Harvard psychical researcher, William James. Later in his life, Murphy was elected President of the American Society for Psychical Research. Murphy devoted an enormous amount of his time to the ASPR—approximately half time, without pay. Gertrude would later follow him in that role.

During Gertrude's first summer in Cambridge in 1942, she audited a seminar with Gardner Murphy on psychical research. At first she planned to attend only one lecture, as she felt that would be enough to be sure there was really nothing to it. But the first lecture was so fascinating, she returned for another, and then another. And before long she was hooked. At that time, Murphy was the holder of the Hodgson Fellowship in Psychical Research at Harvard. He had taken the position with the explicit understanding that he would be the "midwife," or "idea man" but would supervise the work of others to do the actual research. Soon, Murphy offered Gertrude research

funds to do an experiment on ESP. This first experiment was the birth of the "sheep/goat effect." Three papers followed in *JASPR* (Schmeidler, 1943a, 1943b, 1945).

A paper on the sheep/goat effect, coauthored with Murphy, was published in the *Journal of Experimental Psychology* (Schmeidler & Murphy, 1946). As of 2009, only one other paper on the sheep/goat effect and ESP scores has been published in the mainstream literature (Schmeidler, 1966).

The City College Years

Educated at Harvard, and having worked early in her career with two of the most illustrious psychologists in America, Gertrude began her teaching career at City College of New York, then known as "the poor man's Harvard." For over a century, City College was famous for the excellent education it provided free of charge to poor and middle-class urban students (tuition was free until 1976). City College drew the brightest young students in New York, most of whom were the first in their families to attend university (Traub, 1995). City College boasts nine recipients of the Nobel Prize among its alumni.

When Gardner Murphy, recently elected President of the prestigious American Psychological Association, came to the City College to found and chair its Department of Psychology, he invited Gertrude to teach some courses on introductory psychology on a part-time basis. Within a few years she became a full-time faculty member and moved on to teaching courses on perception and experimental psychology. For six years, she directed the M.A. program in psychology and supervised numerous students in their research.

In the 1970's, another kind of social experiment began at City College. Though located in the center of Harlem, the College did not reflect the diversity of the surrounding community. Both faculty and students were predominantly white. Responding to student protests against the low number of African Americans and Puerto Ricans it enrolled, City College began a policy of open admissions in 1970. Any student who graduated from a New York City high school would be automatically accepted into City College. It is generally acknowledged that the academic quality of City College went downhill sharply after that (Traub, 1995). The decline was not really evident in the graduate programs, but it was very marked for its undergraduate education. CCNY became the butt of jokes about students who were functionally illiterate and who were hopelessly unprepared for higher education. City College was no longer spoken of in the same breath as its downtown neighbors, Columbia and New York University.

And as for Gertrude, I believe that as she became more and more deeply involved in parapsychology, she became more and more isolated from her colleagues in the psychology department. When the City College joined several other colleges to form the City University of New York, she

was at first denied a position on the doctoral faculty in psychology. It seems clear that this was because of her activity in parapsychology. She appealed the decision, pointing out that the research methods she used were the same ones used in other areas of psychology. In addition, she had published several books, and the journals in which her research was published were peer-reviewed. Even if the other faculty members did not agree with her research topic, they could not deny her the academic freedom to pursue it. Her argument was successful and she was eventually appointed to the doctoral faculty at the City University of New York.

Still, her older colleagues at CCNY began to retire, and the newer faculty had a much more physiological bent. They were not sympathetic to Gertrude's research interests, and some actively discouraged their students from studying with Gertrude. She was left teaching the masters students and the lower level graduate courses. Some students even complained when she inserted too much parapsychology into these general courses on research methods. She did, however, have the opportunity to teach the occasional course on parapsychology and altered states of consciousness, which I remember as highlights of my graduate years. When she came to the age of retirement in the mid-1980s and they began a search for someone to replace her, the psychology department made it clear they were not interested in hiring another parapsychologist. They felt neuropsychology was the direction the department should be moving in, and they hired someone with that background. So parapsychology was effectively dead at CCNY. I believe I was the last student to work with Gertrude on a psi-related project.

Scientific Work in Psychology and Parapsychology

Of course, we remember Gertrude here because of her work in parapsychology. She served our field well. Gertrude was present at the founding meeting of the Parapsychological Association (PA) and served as its first vice-president from 1957 to 1958. She went on to become President of the PA in 1959, and again in 1971. She was later President of the American Society for Psychical Research from 1981 to 1985. Gertrude's research papers are stored at Duke University along with those of J. B. Rhine.

In some ways, Gertrude was very much in the mainstream of the psychology of her time. She coauthored a book titled *Freshman Rorschachs and College Performance* (Schmeidler, Nelson, & Bristol, 1959), whereas today the emphasis would be on SAT scores. She applied her knowledge of projective techniques and personality theory to her work in parapsychology. For example, she provided interpretations of Rorschachs and other projective tests taken by poltergeist families and paranormal percipients for several published studies (e.g., Maher & Schmeidler, 1975).

Gertrude also published five books on parapsychology. Three were collections of articles by others (Schmeidler, 1969, 1974, 1976) and

two were original works (Schmeidler, 1988; Schmeidler & McConnell, 1958). All her books endeavored to draw relations between psychic function and other areas of theory and experiment. She sought to show that parapsychology is not a field that exists in isolation from other fields of science or social science. In my opinion, the most notable is her last book. Although it did not get much attention from mainstream psychology, it is surely worth the attention of anybody in this audience. Her thesis is that psi is a psychological process; it functions in the same way, and obeys the same laws, as any other psychological process. Her writing is so clear you can almost hear the sound of her voice, and her insights are fresh and still valuable. Her evidence is so comprehensive, and so well presented, that one can't help but be convinced that psi is a natural, psychological function.

In contrast to how research is done today, when it is common to see at least half a dozen names on a paper, Gertrude worked alone more often than not. She was the sole author on most of her papers in parapsychology. About a third were coauthored with a student, and these appear to have been the result of a student research project. Again, in contrast to how things are done today, she kept her projects elegantly simple. Most of her studies had just one dependent variable —an ESP score—and at most two or three other variables, usually including a personality correlate. The data were often analyzed just with a simple *t* test, but nothing more complicated than a 2 x 2 ANOVA. Remember, this was in the days before personal computers. Statistics were done by hand with pencil and paper. Several times I remember Gertrude showing me a data analysis in her tiny handwriting, on the back of an envelope! One of her sons grew up to become a statistician, and he was a great help to her in her work.

No matter what your area of interest in parapsychology, you will probably find that Gertrude has written on the topic. So much of our field rests on her early work that it can truly be said that she helped to lay the foundations of parapsychology as a scientific enterprise. If we look in more detail at Gertrude's oeuvre, it is spread over 6 decades (see attached bibliography). A large proportion of her publications in mainstream journals were actually on topics related to psychic functioning. So in total, her work in parapsychology far outnumbers her papers in conventional psychology.

Mentor

Gertrude ran an informal meeting once a week, and all students were welcome, whatever their rank or area of study. Usually the meeting focused on some student's research project, and Gertrude would invite the group to offer advice on research methods. Her own advice was usually the most cogent. Sometimes she would bring a letter she had received and

ask the group's advice on how to answer it. I remember on one occasion, she read a letter from a young woman who said that she and her lover had held a séance at the grave of Elvis Presley and believed that his spirit had communicated with them. We were all at a loss for how to answer that one!

Gertrude was always so very patient. No matter how silly the question, she would nod and agree that it was a very interesting problem. Then she would say something like, "But perhaps you could think of it another way," and go on to reframe the discussion into something so much more sophisticated. I remember so many occasions when a student would come in all excited about some harebrained theory, and Gertrude would listen patiently, with a slightly distressed look on her face, while she lit a new cigarette. Then she would say, ever so gently, "But how would you TEST that?"—and that was usually the end of the discussion.

Gertrude grew as a researcher through her students. Some students were interested in imagery or mood or creativity, and the research that Gertrude supervised resulted in several papers. One of her students, Larry Lewis, was interested in EEG alpha biofeedback, and this resulted in several papers with Gertrude on the role of feedback on psi performance. Her papers on brain function were coauthored with Michaeleen Maher. Maher was also a film maker, so the opportunity arose to film parapsychologists as they read their papers at a PA conference. The soundtracks were removed, and the films were viewed and rated by undergraduates for aspects of nonverbal communication. This became the groundbreaking work on the experimenter effect in parapsychology—psi conducive experimenters could be identified by their posture, gestures, and facial expressions (Schmeidler & Maher, 1981).

Ultimately, Gertrude's legacy must rest with her students. I had hoped to be able to present some numbers of the students that Gertrude mentored over the years for the MA and the PhD degrees. Unfortunately these statistics were not available from the doctoral program in psychology at CUNY. A new generation of faculty does not remember Gertrude, who retired over 25 years ago. It will be up to this group to keep her memory alive.

Gertrude's doctoral students who went on to contribute to parapsychology include Michaeleen C. Maher, Janet L. Mitchell, and Nancy Sondow. Maher conducted her dissertation research on cerebral laterality and psi, and is well known for her haunting investigations, a meta-analysis of which appeared in the *JP* (Maher, 1999). Mitchell is best known for her out-of-body research with psychic Ingo Swann. She later published two books, including an influential handbook on OBEs (Mitchell, 1989). Sondow published a fascinating paper on precognitive dreaming (Sondow, 1988) and did ganzfeld research with Chuck Honorton. Like Gertrude and Gardner Murphy before her, Sondow has held the position of President of the American Society for Psychical Research since 1993.

With one or two notable exceptions, most of Schmeidler's students have not continued to do active research in the field of parapsychology.

Academic and research opportunities have been few to nonexistent. Time spent earning a living in other fields inevitably detracts from time available for research in parapsychology. I don't think this is Gertrude's fault; she always advised her students to develop other skills that would be useful in the academic job market. But one can criticize parapsychology in general for not being able to support talented young investigators. Indeed, any field that cannot provide employment for its young researchers will soon find itself without any researchers at all.

Legacy

Gertrude proved her commitment to supporting young investigators when she donated funds to establish the Award for Outstanding Student Contribution to Parapsychology, which the PA named after her. So far, 10 young parapsychologists have received this award. In a way, this is a continuation of the encouragement that Gardner Murphy had shown to her in her early days. Perhaps one of these young investigators will prove to be another Gertrude Schmeidler.

References

Allport, G. W., & Schmeidler, G. R. (1943). Morale research and its clearing. *Psychological Bulletin,* **40**, 65–68.

Maher, M. C. (1999). Riding the waves in search of the particles: A modern study of ghosts and apparitions. *Journal of Parapsychology,* **63**, 47–80.

Mitchell, J. L. (1981). *Out-of-body experiences: A handbook.* Jefferson, NC: McFarland.

Nyman, L. (1976, 1989). *Recollections: An oral history of the psychology department of the City College of the City University of New York.* Oxford, England: Lawrence Nyman.

Pilkington, R. (Ed.) (1987). *Men and women of parapsychology: Personal reflections.* Jefferson, NC: McFarland.

Schmeidler, G. R. (1939). Retroaction and proaction in serial learning. *American Journal of Psychology,* **52**, 592–600.

Schmeidler, G. R. (1941). The relation of fetal activity to the activity of the mother. *Child Development,* **12**, 63–68.

Schmeidler, G. R. (1943a). Predicting good and bad scores in a clairvoyance experiment: A preliminary report. *Journal of the American Society for Psychical Research,* **37**, 103–110.

Schmeidler, G. R. (1943b). Predicting good and bad scores in a clairvoyance experiment: A final report. *Journal of the American Society for Psychical Research,* **37**, 210–221.

Schmeidler, G. R. (1945). Separating the sheep from the goats. *Journal of the American Society for Psychical Research,* **39**, 47–49.

SCHMEIDLER, G. R. (1966). The influence of attitude on ESP scores. *International Journal of Neuropsychiatry*, **2**, 387–397.

SCHMEIDLER, G. (Ed.) (1969). *Extrasensory perception.* Oxford, England: Atherton Press.

SCHMEIDLER, G. R. (Ed.) (1974). *Parapsychology.* Metuchen NJ: Scarecrow Press.

SCHMEIDLER, G. R. (Ed.) (1976). *Parapsychology: Its relation to physics, biology, psychology, and psychiatry.* Oxford, England: Scarecrow.

SCHMEIDLER, G. R. (1988). *Parapsychology and psychology: Matches and mismatches.* Jefferson, NC: McFarland.

SCHMEIDLER, G. R., & ALLPORT, G. W. (1944). Social psychology and the civilian war effort: May 1943–May 1944. *The Journal of Social Psychology*, **20**, 145–180.

SCHMEIDLER, G. R., & MAHER, M. (1981). Judges' responses to the nonverbal behavior of psi-conducive and psi-inhibitory experimenters. *Journal of the American Society for Psychical Research*, **75**, 241–257.

SCHMEIDLER, G. R., & MCCONNELL, R. A. (1958). *ESP and personality patterns.* Oxford, England: Yale University Press.

SCHMEIDLER, G. R., & MURPHY, G. (1946). The influence of belief and disbelief in ESP upon individual scoring levels. *Journal of Experimental Psychology*, **36**, 271–276.

SCHMEIDLER, G. R., NELSON, M. J., & BRISTOL, M. (1959). Freshman Rorschachs and college performance. *Genetic Psychology Monographs*, **59**, 3–43.

SONDOW, N. (1988). The decline of precognized events with the passage of time: Evidence from spontaneous dreams. *Journal of the American Society for Psychical Research*, **82**, 33–51.

TRAUB, J. (1995). *City on a hill: Testing the American dream at City College.* New York: Perseus Books.

SELECTED PUBLICATIONS IN PARAPSYCHOLOGY, 1944–1997

Sole Authorship

SCHMEIDLER, G. R. (1944). Position effects as psychological phenomena. *Journal of Parapsychology*, **8**, 110–123.

SCHMEIDLER, G. R. (1948). Research projects in parapsychology. *Journal of Parapsychology*, **12**, 108–113.

SCHMEIDLER, G. R. (1949). Comparison of ESP scores with Rorschachs scored by different workers. *Journal of the American Society for Psychical Research*, **43**, 94–97.

SCHMEIDLER, G. R. (1949). Personality correlates of ESP as shown by Rorschach studies. *Journal of Parapsychology*, **13**, 23–31.

SCHMEIDLER, G. R. (1950). Some relations between Picture-Frustration ratings and ESP scores. *Journal of Personality*, **18**, 331–343.

SCHMEIDLER, G. R. (1952). Personal values and ESP scores. *Journal of Abnormal Psychology*, **47**, 757–761.

SCHMEIDLER, G. R. (1952). Rorschachs and ESP scores of patients suffering from cerebral concussion. *Journal of Parapsychology*, **16**, 80–89.

SCHMEIDLER, G. R. (1954). Picture-Frustration ratings and ESP scores for subjects who showed moderate annoyance at the ESP task. *Journal of Parapsychology*, **18**, 137–152.

SCHMEIDLER, G. (1954). Personality dynamics and psychical research. *Bulletin of the Menninger Clinic*, **18**, 19–30.

SCHMEIDLER, G. (1958). Analysis and evaluation of proxy sessions with Mrs. Caroline Chapman. *Journal of Parapsychology*, **22**, 137–155.

SCHMEIDLER, G. R. (1959). Comments and additional data on sheep-goat classification and targets. *Journal of the Society for Psychical Research*, **40**, 63–72.

SCHMEIDLER, G. R. (1962). Note on precognition with and without knowledge of results. *Psychological Reports*, **11**, 486–486.

SCHMEIDLER, G. (1964). An experiment on precognitive clairvoyance: I. The main results. *Journal of Parapsychology*, **28**, 1–14.

SCHMEIDLER, G. (1964). An experiment on precognitive clairvoyance: II. The reliability of the scores. *Journal of Parapsychology*, **28**, 15–27.

SCHMEIDLER, G. R. (1964). An experiment on precognitive clairvoyance: III. Precognition scores related to the subjects ways of viewing time. *Journal of Parapsychology*, **28**, 93–101.

SCHMEIDLER, G. R. (1964). An experiment on precognitive clairvoyance: IV. Precognition scores related to creativity scores. *Journal of Parapsychology*, **28**, 102–108.

SCHMEIDLER, G. R. (1964). An experiment on precognitive clairvoyance: V. Precognition scores related to feelings of success. *Journal of Parapsychology*, **28**, 109–125.

SCHMEIDLER, G. R. (1966). Quantitative investigation of a "haunted house." *Journal of the American Society for Psychical Research*, **60**, 137–149.

SCHMEIDLER, G. R. (1966). Two horns of a dilemma in relating ESP to personality. *Corrective Psychiatry & Journal of Social Therapy*, **12**, 98–114.

SCHMEIDLER, G. R. (1967). Personality interactions in current research. *Transactions of the New York Academy of Sciences*, **30**, 261–264.

SCHMEIDLER, G. R. (1968). A search for feedback in ESP: I. Session salience and stimulus preference. *Journal of the American Society for Psychical Research*, **62**, 130–142.

SCHMEIDLER, G. R. (1970). High ESP scores after a swami's brief instruction in meditation and breathing. *Journal of the American Society for Psychical Research*, **64**, 100–103.

SCHMEIDLER, G. R. (1970). Studying individual psi experiences. *Journal of Parapsychology*, **34**, 197–209.

SCHMEIDLER, G. R. (1971). Mood and attitude on a pretest as predictors of retest ESP performance. *Journal of the American Society for Psychical Research,* **65**, 324–343.

SCHMEIDLER, G. R. (1971). Parapsychologists' opinions about parapsychology, 1971. *Journal of Parapsychology,* **35**, 208–218.

SCHMEIDLER, G. R. (1973). PK effects upon continuously recorded temperature. *Journal of the American Society for Psychical Research,* **67**, 325–340.

SCHMEIDLER, G. R. (1975). Personality differences in the effective use of ESP. *Journal of Communication,* **25**, 133–161.

SCHMEIDLER, G. (1976). Some lines about Gardner Murphy, the psychologists' parapsychologist. *Parapsychology Review,* **7**, 6–9.

SCHMEIDLER, G. R. (1977). Research findings in psychokinesis. In S. Krippner (Ed.), *Advances in parapsychological research 1* (pp. 79–132). New York: Plenum Press.

SCHMEIDLER, G. (1979). What next in survival research: V. A proposal for predictive research on survival. *Journal of Indian Psychology,* **2**, 24–29.

SCHMEIDLER, G. R. (1979). Gardner Murphy and his thinking: A retrospect and a prospect. *Journal of Parapsychology,* **43**, 86-100.

SCHMEIDLER, G. R. (1980). Gardner Murphy: A short biography. *Journal of the American Society for Psychical Research,* **74**, 1-14.

SCHMEIDLER, G. R. (1982). A possible commonality among gifted psychics. *Journal of the American Society for Psychical Research,* **76**, 53–58.

SCHMEIDLER, G. R. (1982). PK research: Findings and theories. In S. Krippner (Ed.), *Advances in Parapsychological Research 3* (pp. 115–146). New York: Plenum Press.

SCHMEIDLER, G. R. (1983). Interpreting reports of out-of-body experiences. *Journal of the Society for Psychical Research,* **52**, 102–104.

SCHMEIDLER, G. R. (1983). Psi scores and personality: Basic questions and a theory to encompass the answers. *Parapsychology Review,* **14**, 1–4.

SCHMEIDLER, G. R. (1984). Further analyses of PK with continuous temperature recordings. *Journal of the American Society for Psychical Research,* **78**, 355–362.

SCHMEIDLER, G. R. (1984). Psychokinesis: The basic problem, research methods, and findings. In S. Krippner (Ed.), *Advances in parapsychological research 4* (pp. 12–30). Jefferson, NC: McFarland.

SCHMEIDLER, G. R. (1985). Field and stream: Background stimuli and the flow of ESP responses. *Journal of the American Society for Psychical Research,* **79**, 13–26.

SCHMEIDLER, G. R. (1987). The mind-matter-relation: Out of metaphysics and into the laboratory. *Theoretical & Philosophical Psychology,* **7**, 12–18.

SCHMEIDLER, G. R. (1987). Psychokinesis: Recent studies and a possible paradigm shift. In S. Krippner, (Ed.). *Advances in Parapsychological Research 5* (pp. 76–88). Jefferson, NC: McFarland.

SCHMEIDLER, G. R. (1990). Is psi a subcognitive process? *Journal of Parapsychology,* **54**, 321–329.

SCHMEIDLER, G. R. (1991). Perceptual processing of psi: A model. *Journal of the American Society for Psychical Research,* **85**, 217–236.

SCHMEIDLER, G. R. (1992). Response to Hansen: Background, corrections, and amplifications. *Journal of Parapsychology,* **56**, 367–369.

SCHMEIDLER, G. R. (1994). ESP experiments: The glass is half full. In S. Krippner (Ed.), *Advances in parapsychological research* **7** (pp. 104–197). Jefferson, NC: McFarland.

SCHMEIDLER, G. R. (1994). PK: Recent research reports and a comparison with ESP. In S. Krippner (Ed.,. *Advances in parapsychological research* **7** (pp. 198–237). Jefferson, NC: McFarland.

SCHMEIDLER, G. R. (1997). Psi-conducive experimenters and psi-permissive ones. *European Journal of Parapsychology,* **13**, 83–94.

Coauthorship With a Student or Colleague

BRIER, B., SCHMEIDLER, G. R., & SAVITS, B. (1975). Three experiments in clairvoyant diagnosis with Silva Mind Control graduates. *Journal of the American Society for Psychical Research,* **69**, 263–271.

EILBERT, L., & SCHMEIDLER, G. R. (1950). A study of certain psychological factors in relation to ESP performance. *Journal of Parapsychology,* **14**, 53–74.

FRIEDMAN, R. M., SCHMEIDLER, G. R., & DEAN, E. D. (1976). Ranked-target scoring for mood and intragroup effects in precognitive ESP. *Journal of the American Society for Psychical Research,* **70**, 195–206.

GERBER, R., & SCHMEIDLER, G. R. (1957). An investigation of relaxation and of acceptance of the experimental situation as related to ESP scores in maternity patients. *Journal of Parapsychology,* **21**, 47–57.

HUDESMAN, J., & SCHMEIDLER, G. R. (1971). ESP scores following therapeutic sessions. *Journal of the American Society for Psychical Research,* **65**, 215–222.

HUDESMAN, J., & SCHMEIDLER, G. R. (1976). Changes in ESP scores after therapy sessions. *Journal of the American Society for Psychical Research,* **70**, 371–380.

JACKSON, M., FRANZOI, S., & SCHMEIDLER, G. R. (1977). Effects of feedback on ESP: A curious partial replication. *Journal of the American Society for Psychical Research,* **71**, 147–155.

LEWIS, L., & SCHMEIDLER, G. R. (1971). Alpha relations with non-intentional and purposeful extrasensory perception after feedback. *Journal of the American Society for Psychical Research,* **65**, 455–467.

MAHER, M., PERATSAKIS, D., & SCHMEIDLER, G. R. (1979). Cerebral lateralization effects in ESP processing: An attempted replication. *Journal of the American Society for Psychical Research,* **73**, 167–177.

MAHER, M., & SCHMEIDLER, G. R. (1975). Quantitative investigation of a recurrent apparition. *Journal of the American Society for Psychical Research,* **69**, 341–351.

MAHER, M., & SCHMEIDLER, G. R. (1977). Cerebral lateralization effects in ESP processing. *Journal of the American Society for Psychical Research*, **71**, 261–271.

MOSS, T., & SCHMEIDLER, G. R. (1968). Quantitative investigation of a "haunted house" with sensitives and a control group. *Journal of the American Society for Psychical Research*, **62**, 399–410.

SCHMEIDLER, G. R., & CRAIG, J. G. (1972). Moods and ESP scores in group testing. *Journal of the American Society for Psychical Research*, **66**, 280–287.

SCHMEIDLER, G. R., & LEWIS, L. (1968). A search for feedback in ESP: II. High ESP scores after two successes on triple-aspect targets. *Journal of the American Society for Psychical Research*, **62**, 255–262.

SCHMEIDLER, G. R., & LEWIS, L. (1969). A search for feedback in ESP: III. The preferential effect and the impatience effect. *Journal of the American Society for Psychical Research*, **63**, 60–68.

SCHMEIDLER, G. R., & LINDEMANN, C. (1966). ESP calls following an "ESP" test with sensory cues. *Journal of the American Society for Psychical Research*, **60**, 357–362.

SCHMEIDLER, G. R., & MAHER, M. (1981). Judges' responses to the nonverbal behavior of psi-conducive and psi-inhibitory experimenters. *Journal of the American Society for Psychical Research*, **75**, 241–257.

SCHMEIDLER, G. R., PEATMAN, J. G., & HARTLEY, E. L. (1960). Changing field relations of an ESP experiment. In J. G. Peatman & E. L. Hartley (Eds.), *Festschrift for Gardner Murphy* (pp. 94–105). Oxford, England: Harper.

Books

SCHMEIDLER, G., (Ed.) (1969). *Extrasensory perception.* Oxford, England: Atherton Press.

SCHMEIDLER, G. R., (Ed.) (1974). *Parapsychology.* Metuchen, NJ: Scarecrow Press.

SCHMEIDLER, G. R., (Ed.) (1976). *Parapsychology: Its relation to physics, biology, psychology, and psychiatry.* Oxford, England: Scarecrow Press.

SCHMEIDLER, G. R. (1988). *Parapsychology and psychology: Matches and mismatches.* Jefferson, NC: McFarland & Co.

SCHMEIDLER, G. R., & MCCONNELL, R. A. (1958). *ESP and personality characteristics.* Oxford, England: Yale University Press.

The NeuroPsience Laboratory
305 West 13th St., #2-J
New York, NY 10014, USA
ruthrein@mindspring.com

BOOK REVIEWS

UNBELIEVABLE: INVESTIGATIONS INTO GHOSTS, POLTERGEISTS, TELEPATHY, AND OTHER UNSEEN PHENOMENA, FROM THE DUKE PARAPSYCHOLOGY LABORATORY by Stacy Horn 2009. New York: HarperCollins, 2009. Pp. 289. $24.99 (hardback). ISBN978-0-06-111685-8.

"Freaky and terrifying" is the description of the current box-office film hit "Paranormal Activity" by Owne Glieberman in *Entertainment Weekly* (http://www.paranormalactivity-movie.com). The public, apparently, will never tire of hauntings, poltergeists, séances and the like. It was out of such nineteenth-century "paranormal activity" and the associated spiritualist movement—and partly in reaction against it—that psychical research and, later, parapsychology came into being. Stacy Horn's eminently readable book is, basically, a narrative of the life and career of the founder of parapsychology, J. B. Rhine. However, his name does not appear in the book's title, and the subjects listed in the subtitle go far beyond the research foci of Rhine's Parapsychology Laboratory, despite Horn's linkage of that institution to them in her title. What this book does effectively is to contextualize Rhine's vision of parapsychology as an experimental science in the broader, more emotionally intense, less scientifically controllable paranormal activities in which the general public was (and is) really interested.

In the first four chapters, Horn sketches Rhine's life and career down through World War II. This covers familiar territory in the history of parapsychology: how the Rhines became interested in parapsychology, their move to Boston and J. B.'s investigation of the medium "Margery" (an omission here is the name of Walter Franklin Prince), their move to Duke University and the work on psychical abilities carried out in the 1930s. As her relating of the Margery incident and of the efforts of John Thomas to prove that he had received contact from his deceased wife show, Horn factors into her narrative the spiritualist contexts of the inception of parapsychology. But Rhine, trained as a scientist and located by the early 1930s in the psychology department of Duke University, was bent on transforming these paranormal activities into a legitimate experimental science by testing for psychical abilities such as telepathy and clairvoyance and evaluating the results by quantitative statistical methods. This, too, is sketched out by Horn.

The remainder of the book is a kind of dialectic between accounts of sensational poltergeists and reincarnations, and moves of parapsychology toward the goal of scientific legitimacy. The connection between the two accounts lies in a number of factors: The celebrity of the parapsychology laboratory at Duke made it the natural place for perplexed or alarmed observers of paranormal activities to seek elucidation and help; Rhine's

major financial backers were more interested in the demonstration of post-mortem survival than in experimental ESP; finally, the extension of study at Duke to spontaneous cases brought some of the work of the laboratory into liaison with paranormal activities. Moreover, cultural luminaries such as Aldous Huxley and Timothy Leary sought out Rhine for reasons other than the statistical success of his ESP experiments.

But Horn also uses this dialectic to exhibit the disjunction between Rhine's science-based caution and the sensationalist but immensely interesting claims of poltergeist or mediumistic activities, etc. Rhine's reaction to the "Seaford Case," involving poltergeist phenomena, is characteristic: "The spectacle of seeing us make too much of cases that cannot be firmly identified as justifying scientific interest is not one we want to encourage and extend" (pp. 154--155).

However, as manifested in her summary of the state of the Parapsychology Laboratory in mid-twentieth century, Horn seems to feel that Rhine's scientific rectitude ended up constraining the development of parapsychology:

> In many ways, Rhine's determination to stick to the ESP plan brought them to a virtual standstill, going over the same ground repeatedly. They had yet to develop a theory for ESP. In 1950, they prepared an article based on material from 1938. . . . How many young scientists wanted to recheck twelve-year-old columns of ESP test results when there were things like poltergeists to investigate? (p. 98)

At the end of the book, she is even bleaker about end-of-the-twentieth-century parapsychology: "The story of the Parapsychology Laboratory begins and ends in stalemate. Their experiments confirmed telepathy and were never generally accepted. They looked for evidence of life after death, but the evidence was inconclusive" (p. 241). I leave it to the readers of this journal to evaluate Horn's delineation of the state of modern parapsychology.

As I noted early in this review, Horn's book is a pleasure to read and it does provide good popular cultural context for the development of parapsychology. I will say that I learned at lot from reading *Unbelievable.* Although the book lacks scholarly footnotes, Horn has provided a list of references for each chapter at the end of the book, and it is clear that she has certainly done her scholarly homework. The book is less noteworthy for its systematic historical analysis of parapsychology as a nascent or would-be scientific specialty. Other than the Duke Parapsychology Laboratory, we learn virtually nothing about the psychical research/parapsychology community either in the United States or abroad. One egregious casualty is Gardner Murphy, who appears to get only one mention (p. 44) and then without identification.

And Horn eschews any deep discussion of issues critical to the debates over parapsychological claims, such as the methods of statistical evaluation or experimental safeguards against chicanery. She does mention statistical critics William Feller and Persi Diaconis in her Epilogue, but her very brief comments on their critiques will no doubt mystify anyone not already familiar with the statistical controversy over ESP results.

Nevertheless, the book is a genuine addition to the historical literature on parapsychology and can be recommended as a good overview of one of the most interesting episodes in the history of American science—an episode that may yet be more ongoing than Horn suggests.

SEYMOUR MAUSKOPF

Department of History
Duke University
Durham, NC 27708, USA
shmaus@duke.edu

NEAR-DEATH EXPERIENCES: EXPLORING THE MIND-BODY CONNECTION by Ornella Corazza. London: Routledge, 2008. Pp. xi + 170. £17.99 (paperback). ISBN 0-415-45520.

As the title suggests, this book is concerned with the near-death experience (NDE) in relation to the mind-body connection, particularly in relation to Eastern, mostly Japanese philosophies, and it considers the similarity of NDEs with experiences occurring under the dissociative anaesthetic ketamine. These two separate approaches to the NDE issue were originally explored in depth as part of Corazza's recent doctoral thesis at SOAS in London and are now considered together in this book, though in a somewhat less integrated fashion than one might expect, as we shall see. Initially the book outlines some of the sticking points of mind-body philosophy, particularly what David Chalmers calls "the hard problem of consciousness" relating to how subjective experience arises from the objective activity of the brain. Taking as the starting point the Japanese philosopher Yuasa's conception of the whole mind-body, the introduction moves through Husserl's phenomenology to Varela's neurophenomenology, segueing into James's fields of consciousness and Sheldrake's extended mind theory, prompting Corazza to offer the notion of "the extended body" as an alternative. Incorporating, quite literally, Edward Hall's notions of the corporal extensions of humans, such as language as an extension of experience in time and space, and Weston La Barre's "evolution by prosthesis," such as the creation of submarines to allow underwater exploration, our author tantalisingly adumbrates the extended body in the Japanese tradition as a semidefinite and indefinitely varying body-space. We are also reminded that in the Eastern tradition we not only have a body but we are our bodies.

Having given the bones of the mind-body problem and the author's position on it, Corazza adds the flesh in the following chapter by providing an overview of the NDE phenomena, illuminating Ring's various stages on the way to entering the light. Consideration is also given to some of the arguments, and counterarguments, against taking these extraordinary experiences at face value. For instance, the cases of seemingly veridical out-of-body experiences (OBEs) on the operating table are dissected to a degree, and Blackmore's contention that the visual appearance of the medical setting can be recreated from the other senses, still available to the patient, is rebutted with Sabom's research demonstrating that resuscitation procedures are not easily imagined by people who have a cardiac arrest. It might have also been noted that Blackmore seems to want to have it both ways because in a later appraisal of the theory that NDEs are actually rebirth experiences, Blackmore is acknowledged for her insight that the visual system of a neonate is too underdeveloped to actually *see* a tunnel-like event at birth, though might they not reconstruct the visual experience from their other senses, supposedly much like those having NDEs while being resuscitated?

Many of the common features of the NDE are explored in further depth and illustrated with fascinating accounts from Corazza's own research, such as experiences encountering angels. But in trying to fathom the essential factors of the NDE, the author draws upon Kellehear's cross-cultural study to indicate that the transition to darkness and the meeting of beings are the only consistent features. In reflection of her own research, however, and by asking how the NDE is experienced, Corazza instead suggests that there are in fact three alternative cross-cultural features of the NDE: the first is that the experience is not a dream; the second is that the experiencers always travel to another place; and the third is that the NDE is one of the most important experiences in the person's life, and often life changing. This being the case, its not specified what then becomes of Kellehear's own universal NDE features.

Focusing on the second of her universal NDE features, in particular, the third chapter returns to the Japanese philosophies again to emphasize the importance of "place" as the subjective experience of space, identifying the relationship between body and space as a "state of between-ness." It's here, for me, that, in a rather zen manner, the definitions of concepts such as "dependent origination," *basho* (literally "place") and the "logic of place" became rather too minimalist and abstract, leaving me feeling somewhat disorientated when I encountered quotes like "... in order to perceive the object of intention, there must be something like a place, or a field of consciousness, which 'envelops' both subject and object from within" (p. 72). Nevertheless, the connection to fields of consciousness brought me back to familiar ground once again when Sheldrake's morphogenetic fields were discussed, leading gracefully into the vagaries of Grinberg-Zylberbaum, and the genius of Jung, Wundt, James, and Husserl, but soon taking a turn into odd territory once more. This time, Corazza draws upon

Bachelard for insight into pure experience, which had me wondering what was actually meant by the seemingly different terms of "soul" and "mind" being used, which had elsewhere in the book been used interchangeably, along with the terms "spirit," "consciousness," and "I." In fact, it's only in the penultimate chapter that we have any consideration given to the perplexing interchangeability of such words, and yet a little orientation at the beginning of the book about the many synonymous terms in use would have avoided some confusion.

The fourth chapter, entitled "Meeting God in a Nightclub," moves away from the mind-body philosophy and its labyrinthine terminology into the recreational and exploratory use of ketamine, which, when taken at subclinical doses, "... can produce effects similar to those described during an NDE" (p. 83). The idea that ketamine (K), mimics NDEs is not new; Grinspoon and Bakalar (1979) first suggested that the brain may make ketamine-like substances in times of stress, and Rogo (1984) indicated a possible neurochemical pathway for the NDE due to the resemblance of these experiences to some K experiences. However, little recognition is given to these earlier researchers, and instead reliance is made on Jansen's (1994) later and more complete theory, which combines both these previous ideas but topped up with some more advanced speculations about the neurochemical processes involved with this exogenous N-methyl D-Aspartate (NMDA)-antagonist. Nevertheless, solid phenomenological research to support Jansen's theory has thus far been sadly lacking, so Corazza's work admirably continues with this line of reasoning by making a formal comparison of K-induced and non-K induced NDEs so that the apparent K-NDE relationship can be more properly scrutinised—research which has been overdue for more than 20 years.

This seminal systematic K-NDE comparison, seemingly published for the first time here, takes the form of interviews with two groups of 36 people, the first of which were collected by Peter Fenwick and consist of those having NDEs through cardiac arrest or other life-threatening circumstances, and the second of which comprises people having near death-like experiences on ketamine. It is not specified how the former group were sampled, but the latter group were taken from a group of 65 ketamine-using volunteers who had had a near death-like experience, and were selected for the final group of 36 only if they met Greyson's NDE scale criteria. Although the means of recruitment isn't specified clearly in the text, a request was made by the author for participants who felt they had had an NDE on ketamine. Later on, we learn that only about 12% of K-users are expected to have an NDE on ketamine, according to Jansen, so we are only dealing with a subsection of K-users—a point that could have been made much clearer. An account follows of the prevalence and quality of a number of features of the NDE among Corazza's select K group, and we find that many of these experiential features appear, both qualitatively and quantitatively (by percentage of the group reporting each experience),

to be very similar for both the K-NDE and life-threatening/cardiac NDE groups, at least for the experiences of time perception, visionary frequency, life review, understanding of the universe, sense of peace, vividness of the experience, extrasensory perception, and OBEs.

Unfortunately, the way in which the replies were reported left me with the question of whether or not the K respondents—most of whom had taken the substance between 10 and 2,000 times—were reporting their experiences in relation to a small number of specific NDEs that they had had with K or whether they were reporting features of experiences they had had with K generally. Given that this detail is unspecified, it is not entirely clear how much faith we can have in the *quantitative* comparison of the NDE features, given that no natural NDEer will have had 2,000 life-threatening experiences on which to base the comparative K-NDE report, coupled with the fact that only a few K-users report near death-like experiences. It's a lack of specific details such as this that make me wonder why this research has apparently not been published in a peer-reviewed journal prior to publication in this book. Important as this research is, gaps in the specified methodology may call for a reinterpretation of the results such that the prevalence of K-induced NDE features may not be as numerous as we are given to believe. Certainly Greyson-approved NDEs occur at least to some degree on ketamine, but the actual rate of occurrence may be considerably less on K than those occurring in genuinely life-threatening circumstances, such as with cardiac sufferers, say, but it is hard to tell from the details of the method given here. Qualitatively, however, these experiences certainly share many features in common.

There are, however, also certain experiential differences reported between the two groups. Ketamine users, for instance, were much less likely to encounter deceased or religious beings, which might have been attributed to the lack of genuine life-threatening circumstances with K, except for the fact that those in the K group were convinced that they were dead or dying. The experience of light was also much lower in the K group, although they were far more likely to report a sense of unity with the universe, with some even "taking part" in its fabrication. Finally, far fewer in the K group approached "a point of no return," possibly due to the foreknowledge that one was descending on a journey and should return, or possibly due to a lack of depth in that descent, dependent upon the dose. Despite the differences, however, the book reports two cases of people who have experienced both K and non-K NDEs who report that they are, experientially, the same.

In light of these findings, some speculation is given over to the authenticity of the K-induced NDE and its implications for the non-K NDE, taking in Fontana's point that K does not reproduce NDEs but merely facilitates them. Unfortunately though, it isn't stated directly whether or not the study reported in this book supports the similarity of K-induced and non-K induced NDEs to any comparable degree, but instead an elucidation

is made of some of the theories that have been put forward to explain NDEs generally, mistakenly considering Jansen's K-NDE theory to be neuro-reductionistic. Jansen (1997), in fact, suggested that his model does not preclude spiritual explanations and that he actually came to believe in a soul through his ketamine research. Nevertheless, ample space is given over to those perspectives that do run counter to mind-brain identity reductionism, such as those of Varela, Nagel, and Damasio, although this appears to be at the expense of the survivalist hypothesis of the NDE, which, surprisingly, receives rather less than a page. This lack of generosity appears to be because the survivalist hypothesis is considered to be inadequate in accounting for "... the cases of those who do not experience a state of temporary death ... in the ketamine study" (p. 124). However, as a medical doctor, Jansen has argued that there is no such thing as temporary death—you either die (permanently) or you don't—and that this distinction between K-induced and non-K induced experiences of near-deathness is ill founded. According to Jansen (1997, p. 87), "... there is no reason to suspect that the NDE mechanism would never be activated spontaneously" without the actual threat of death. And indeed Corazza acknowledges that NDEs can be triggered by various non-life threatening means, such as during rapid acceleration. Surely, then, ketamine also qualifies as an occasional NDE trigger, and perhaps the author would be in agreement with this, but her final position on the K-NDE association is not clear, so we can't be sure.

What is clear is that Corazza proposes a radical rethink to the way we conceptualise the mind-body relationship so that it is more in accord with Eastern philosophies, bringing the body back into focus in the debate. Again, however, we run into conceptual difficulties in discussion of Nagatomo's body-as-spirit theory as an alternative to dualism, because the term "spirit" is undefined, and without definitions, the theory begins to look like the mind-body identity reductionism we are warned off in the previous chapter. Other Eastern body perspectives are expanded upon, but it's Yasua's concept of the body as being composed of four main information subsystems that made its mark on me. The first three subsystems are the sensory-motor system, the kinaesthetic system, and the autonomic nervous system, but the fourth is composed of the unconscious quasi-body, which in Eastern traditions is referred to as "ki" (or "qi" or "chi").

As an extension to the mind-body relationship, the addition of the ki-energy body in this context rightly offers a new dimension to the debate, but sadly, I failed to see how these Eastern philosophies contribute "... a non-dualist, non-reductionist view of the NDE" (p. 1). For instance, Nagatomo seemingly takes on a classically dualist stance by stating that "... we must allow the body to speak to the mind. . . ." (p. 135), and Yasua similarly states that, "the *ki*-meridian system is related closely to both the mind and body" (p. 138), clearly indicating a traditional mind-body split, albeit with an added third element—the ki. What then do these Eastern perspectives offer to the NDE, and what do they have to say about the

ketamine experience? It's answers to these questions that I found hard to fathom because they are not explicitly stated.

Tantric philosophy, for instance, similarly proposes an energetic body, called Shakti, which complements and opposes consciousness, called Shiva, and although they are distinct, they are inseparable, as is found in the adage that "Shiva without Shakti is shaba (a corpse)." At death, it is said that both Shiva and Shakti (or sometimes Sha-"ki") leave the body, much like the ancient Egyptian tradition, which specifies that both the "ba" (consciousness) and "ka" (the energy body) leave the corporal body at death and progress to the after life. But, essentially, both these ancient mystery traditions view the mind and body as separate, with the energy body as a third factor connecting the former two. Regrettably, however, within the Eastern philosophies put forward in this book, an indication of the fate of the mind (or consciousness, soul, spirit, or I, depending on the page) at death or near death is never really forthcoming, nor is the bearing of such philosophies on the findings of Corazza's timely and fascinating ketamine-NDE research. This is somewhat disappointing, because the elaboration of the relationship of body to "place," the apparent insistence on a sense of place in NDEs, and the importance given to the energetic body could usefully offer something new to the NDE debate, but *what*, exactly, is never quite gleaned. Given that this was originally a PhD thesis, I get the feeling that this slim book has been rather too heavily edited and that the missing pieces of the jigsaw probably remain on the publisher's editing desk. Nevertheless, the remaining book certainly provokes much new thought on the ongoing NDE debate, but left this reader with more questions than answers, although perhaps that is appropriate.

References

GRINSPOON, L., & BAKALAR, J. (1979). *Psychedelic drugs reconsidered.* New York: Basic Books.

JANSEN, K. L. R. (1997). Response to commentaries on "The ketamine model of the near-death experience." *Journal of Near-Death Studies,* **16**, 79–95.

ROGO, D. S. (1984). Ketamine and the near-death experience. *Anabiosis: The Journal of Near-Death Studies,* **4**, 87–96.

DAVID LUKE

The Beckley Foundation
Beckley Park
Oxford, OX3 9SY, UK
drdluke@gmail.com

SCIENCE UNDER SIEGE: DEFENDING SCIENCE, EXPOSING PSEUDOSCIENCE. Edited by Kendrick Frazier. Amherst, NY: Prometheus Books, 2009. Pp. 203. $21.98 (paperback). ISBN 978-1-59102-715-7.

Science Under Siege is a collection of articles previously published in *The Skeptical Inquirer* (*SI*), most of them within the last 5 years. In some cases, updated commentary is provided, and some new material, including a transcript of a question and answer session following a keynote address by Carl Sagan, is included.

The first contribution, by Paul Kurtz, the chairman of the Committee for Skeptical Inquiry (CSI), formerly known as the Committee for Scientific Investigation of Claims of the Paranormal (CSICOP), is a review of the accomplishments in the first 30 years of *SI*. Among them, Kurtz cites the now infamous investigation of Michel Gauquelin's astrological research. Curiously, Kurtz does not mention the controversies over CSICOP's own botched investigation in what has become known as the STARBABY scandal.

Kurtz notes that popular interest in the paranormal has declined, at least as measured by book sales.

The next contribution is by the book's editor, Kendrick Frazier (also the editor of *SI*). Like Kurtz, he notes a reduction in irrationalism since the founding of CSICOP 30 years ago. He notes that a much more insidious threat today is the rise of religious fundamentalism. Indeed, the retitling of CSICOP as CSI was intended to reflect a wider focus on all types of irrationalism. This is reflected in the fact that only 10 of the 39 contributions in the book deal directly with parapsychology. I will focus on those that are either directly or indirectly relevant to parapsychology.

The first such contribution is Mario Bunge's "The Philosophy Behind Pseudoscience." Bunge classifies both psychoanalysis and computational psychology as pseudosciences. He asserts that whether or not one studies the brain depends on one's philosophy, noting in particular that idealists will not consider brain states. He states that psychoanalysis posits an immaterial soul that interacts with the physical body. This may come as a surprise to readers of Freud's *The Future of an Illusion* and *Civilization and Its Discontents*, in which he dismisses religion as a pathetic infantile fantasy.

Bunge further notes that psychoanalysts do not perform empirical tests and that Freud explicitly divorces psychoanalysis from experimental psychology and neuroscience. This ignores Freud's 1885 *Project for a Future Psychology*, in which he lays out his vision for a unification of psychoanalysis and neuroscience.

Bunge asserts that computational psychologists also ignore the brain, asserting that all that matters are the computations themselves, not the hardware or "wetware" that implements them.

While Bunge criticizes others for their prejudgment, he asserts that "there is no genuine explanation without mechanism" and that "all

mechanisms are material" (p. 246). It is good to see that Bunge is taking his own advice and is keeping an open mind.

Finally, he classifies both sociobiology and work relating to the anthropic principle in cosmology as borderline examples of pseudoscience.

The next contribution is Bruce Flamm's "The Columbus University Miracle Study." The study in question found that women who were prayed for became pregnant (via *in vitro* fertilization) at twice the rate as women who were not the recipients of prayer. Flamm notes that two of the three authors listed on the report had only minimal or no involvement in the study and that the third author, Daniel Wirth, has been charged with bilking the cable provider Adelphia out of $2.1 million. He reports that Wirth has used aliases (such as John Wayne Truelove, the name of a child who died at the age of 5) and that his research associate Joseph Horvath has also been accused of fraud, bringing the total amount to $3.4 million between them. Thus, Flamm concludes that the primary researcher in the Columbia study may not be entirely trustworthy.

The next contribution is by Amir Raz, a neuroscientist new to parapsychology, who reviews a conference entitled "Meeting of the Minds" held in Vancouver, aimed at inspiring a dialogue between parapsychology and its critics. Raz offers little of substance, but his essay is followed by a review of the same conference by Ray Hyman, a more seasoned critic. Hyman focuses on lack of replicability of experimental results in his rejection of the claims of parapsychology.

Stanley Jeffers then presents a skeptical review of the research on the psychokinetic influence on random event generators (REGs) conducted by the Princeton Engineering Anomalies (PEAR) research group. Jeffers notes that the cumulative control baseline in the PEAR experiments is significant at the 0.04 level, which brings into question the randomness of the REG. In some instances, the variance in the baseline data of the PEAR REG is significantly low, which also calls into question the randomness of the REG used in the PEAR research

Harriet Hall (the "SkepDoc") discusses Gary Schwartz's experiments on psychic healing. In her rejection of Schwartz's findings, she cites nonsignificant findings in research on the effectiveness of therapeutic touch.

The next article is Ray Hyman's "Testing Natasha." I can think of no better summary than that contained in the following letter I submitted to the Editor of *SI* at the time, which was not published:

> To the Editor:
> Re: "Testing Natasha"
>
> In Ray Hyman's piece "Testing Natasha" (*SI, May/June 2005*), a new milestone in the debunking of

paranormal claims appears to have been crossed. Once, critics were content to wait for a parapsychologist to commit a methodological gaffe and then pounce on the experiment's shortcomings to refute the reality of the claimed effect. In a sense, the critics were content to act like a pack of hungry jackals (or noble lions, depending on one's political preference) circling a herd of antelopes (the parapsychological research community) waiting for a sick calf (methodologically incompetent researcher) to separate from the herd.

Hyman and his coworkers (Richard Wiseman and Andrew Skolnick) were apparently not content to wait for a sick calf to emerge on its own. They manufactured one of their own by designing and conducting a highly flawed experiment in which Natasha Demkina (the "Girl with the X-ray Eyes") attempted to match seven human subjects to their medical diagnoses. Natasha was not screened from the subjects, but could observe them at close hand. This procedure allows many sensory cues that would enable Natasha to match diagnoses to subjects (e.g., the person who was missing a large portion of her left lung might for instance breathe with somewhat more difficulty than the other subjects). Few members of the serious parapsychological research community would run a study with such poor safeguards against sensory cues (although, alas, some would). Hyman then attacks his own study on the basis of the methodological flaws just outlined. Thus, with no sick antelope in sight, Hyman has taken upon himself both the role of sick antelope and devouring lion. As he notes, there is really no way the psi hypothesis could be upheld given the methodological deficiencies of the experiment that he has both designed and attacked. One therefore wonders what point there is in running such a study.

Another point of interest is that Hyman, Weisman and Skolnick declared the experiment a failure in any event, insofar as Natasha successfully matched "only" four of the seven subjects' diagnoses to the appropriate subject, whereas Hyman et al. had prespecified five correct matches as the criterion for success (by chance, one would expect only one correct match). However, assuming the experiment had been conducted with appropriate safeguards against sensory cues, the probability of matching four or more diagnoses to the correct subject is 0.01825 under the permutation-matching distribution. (The probability of four or more correct matches under

the binomial distribution is 0.01015; however, due to the violation of the assumption of independence of trials in Hyman et al.'s experiment, it is not really appropriate to use the binomial distribution in this case.) Thus, there is less than a 2% chance that Natasha could have done as well as she did by luck. Something more is involved (most likely, the amateurish design of Hyman, Weisman and Skolnick's experiment, which failed to isolate Natasha from sensory cues). It is, strictly speaking, true that the results of the experiment fell just short of statistical significance at the 0.01 level (presumably the unstated basis for Hyman's et al.'s five-match criterion for a successful experiment). However, given such a small number of trials, it would surely be more appropriate to use a 2% significance level to increase the power of the analysis.

In concluding, I would ask CSICOP not to manufacture any more sick antelopes; the parapsychological community already has all the staggering quadrupeds it can deal with.

In Kendrick Frazier's introduction to the present book, he notes that this investigation of psychic diagnosis by Hyman et al. was awarded CSI's Robert P. Balles Prize in Critical Thinking.

The next psi-relevant contribution is Joe Nickell's report of an investigation of the goings-on at Camp Chesterfied, a spiritualist enclave. Nickell reports that purported ghosts were played by human confederates as well as several other instances of garden-variety fraud.

Benjamin Radford reports the results of a haunting investigation in which mysterious sounds were found to originate from mundane causes such as leaf-raking.

Finally, Massimo Polidoro describes the legerdemain techniques used by him and James Randi to simulate the psychic reception of a drawing.

Consistent with the CSI's expanded focus, there are many contributions in this volume that deal primarily with issues other than psychic claims. Ann Druyan, Carl Sagan's wife, expresses the wish that the divorce between spirituality and science would end. This contribution is followed by a transcript of the question and answer period following a keynote address by Carl Sagan.

Other topics addressed include: the use of Bayesian statistical analysis to address the concern that extraordinary claims require extraordinary evidence, intelligent design, false memories, AIDS denial, global warming, the population's focus on the events of 9-11 while ignoring the many greater dangers, the anti-vaccination movement, the decentralization of electricity generation, allegations the Apollo moon landing was faked, magnet

therapy, oxygen therapy, and the "urban myth" regarding a patent officer who declared that all significant discoveries have already been made.

I believe that CSI's wider focus has made *SI* a more mature and relevant publication. CSICOP's early focus on the shakiest cases of outrageous paranormal claims led them to concentrate their debunking effort on phenomena that only the most desperate of parapsychologists would pursue.

DOUGLAS M. STOKES

424 Little Lake Drive, #3
Ann Arbor, M. 48103
Dstokes48 1 03

PHENOMENA: SECRETS OF THE SENSES by Donna M. Jackson. New York: Little, Brown and Company, 2008. Pp. 174. $16.99 (hardcover). ISBN-13: 978-0-316-16649-2.

There are many things that go "bump" in the night that amaze and astound us. *Phenomena: Secrets of the Senses* offers us a treasure trove of paranormal and amazing normal phenomena that our more than five senses experience. Written for the layperson, notably school-age children, Jackson's easy to read book offers scientific information on a variety of phenomena, some of which can be easily explained through scientific study, while others defy explanation.

The author catches our interest immediately with the true-life event of Ian Waterman, whose mind and body became disconnected. Due to a rare disease, his body was not receiving sensory feedback that allowed him to monitor his actions. As a result he suddenly was unable to feel anything he touched and did not know where the various parts of his body were until he looked at them. Through determination and over years, Waterman was able to train himself to walk, sit up, stand, and drive by looking at his limbs and concentrating on making his body move (p. 2). Such is the phenomenal interplay of the mind and body that is explored throughout this book.

Most of us think of only five senses that impact on us, sometimes six. But as we soon learn in this book, although vision, hearing, touch, taste, and smell are the most obvious primary senses, at least 21 have been identified (p. 2). Additional senses include hunger, thirst, and internal monitoring of heartbeat and levels of oxygen and carbon dioxide, among others that keep our body functioning smoothly (p. 3). The author moves into scientific explanations for a variety of aspects regarding our body and brain without becoming overly technical or abstract. Her explanations of neuroscience are easy to understand without becoming simplistic. There are intriguing photos and "Mind Tinglers" inserts that grab the reader's attention.

Interviews with various medical scientists are included to supplement the explanations or refutations of various phenomena presented. Various sensory-based afflictions are listed in which the senses go awry. For example, people with Capras syndrome lose their emotional connection to the visual awareness of friends and close relatives. As a result, they think such people are "imposters" because they do not feel the appropriate emotions upon seeing them; however, they *do* have emotional connection when hearing the same persons' voices on the phone (p. 9). Other chapters address topics such as animals' sensing of earthquakes and natural disasters, as well as even being able to sniff out cancer in humans; phantom limb sensations and ways to counter-act them; use of the tongue as a way to see and to restore balance; and computer programming that allows the blind to use sound to see.

The book is of more interest to parapsychologists when it transitions smoothly into introducing paranormal experiences, our sixth sense, as phenomena that may "have roots in our sensations and the way the brain understands them through our perceptions" (p. 9). Trying to be objective, Jackson presents both sides to seemingly paranormal events. However, at times she appears to give more weight to the skeptical side. For example, she reports on Annette Martin's psychic medium experiences that she uses to help police in the state of Washington solve murders, but she also includes skeptical psychologist Thomas Gilovich, who offers a counter argument that such events are deceptive sometimes. However, he does admit, "the truth is, it *would* be amazing if we had those powers. However wonderful this world is, it'd be even more wonderful if that stuff were true" (p. 19).

Mediumship and prophetic visions are also included. Jackson discusses Nostradamus's predictions, and the skeptics' view that any links made to his obscure visions lie in the readers' making connections after the fact (pp. 20-21). The spirit photography of William Mumler is debunked as doubly exposed film that made superimposed ghostly figures appear onto the original photos. David Myers's comments help balance this negative view of paranormal phenomena by stating how parapsychologists use scientific inquiry to help "separate bizarre ideas from those that sound bizarre but are true" (p. 22).

The ganzfeld studies at the Rhine Research Center are described. John Palmer explains the process in detail and points out that chance expectancy is only 25%, whereas the studies have resulted in an average of 33%, "which is quite significant statistically" (p. 22). However, the author lends more weight against this study and psychic phenomena through remarks by Richard Wiseman, psychology professor at the University of Hertfordshire. Originally a professional magician, he now studies deception, psychic fraud, and the paranormal. He feels that people get hurt "emotionally, physically and/or financially" from such fraud. Three of his studies are presented as refuting the existence of ESP. One involves an explanation for why people may feel a house is haunted. Using contemporary

music mixed with infrasound (sound pitched too low for humans to hear), he had an audience of 750 people describe their reactions. Twenty-two percent gave responses in keeping with strange experiences, intense sorrow and spine-tingling chills. Wiseman suggests his study shows that the low-level infrasound may be present at "hauntings," thereby creating the sensations people attribute to ghosts. Unfortunately, Jackson does not cite William Roll's impressive and extensive research on Recurrent Spontaneous Psychokinesis (RSPK) that offers support of psychic abilities in another direction.

Another study by Wiseman using a "Mind Machine" that allows the participant to predict the outcome of a random coin toss in believers and nonbelievers of ESP did not result in any supportive evidence of ESP in almost a quarter of a million trials (pp. 24-25). However, on the side of good paranormal research are years of studies using random number generators resulting in positive PK results. The research of Robert Jahn, Roger Nelson, and Brenda Dunne in the PEAR experiments on psychokinesis consisted of 33 participants who tried to use their minds to affect machines. There were over 14 million trials over a 7-year period with statistically significant results over chance findings.

Wiseman's final experiment included students and psychics who "read" three items that were each associated with three solved crimes. Results showed that the psychics were not more accurate than the students, and both groups did not perform above chance. Again, Jackson overlooks credible research with psychics conducted by the University of Virginia and at the Rhine Center that counters Wiseman's research.

Another chapter is devoted to intuition. Unusual coincidences are looked at through a scientific lens with logical explanations. And yet Jackson includes two examples that offer the possibility of divine or angelic intervention (p. 61) without skeptical counter-arguments. One is the story of Hollis Long's precognitive dream about being reunited with his former girlfriend (which eventually led to marriage and became an integral and profound part of his life's story), which is looked at as a possible nudge from a higher power (p. 61). Squire Rushnell is included in the book to help offer an explanation that such an experience is a "God Wink," which he views as a "personal signal or message, directly from a higher power" that serves the purpose of reassuring the receiver that he or she is "moving in the right direction along life's path" (p. 61). It seems hard to imagine why the author chooses to give Rushnell's work a higher level of credence and acceptability by leaving it printed unchallenged and unscathed by a skeptic's comments, as compared to the strong opposing comments that are included against each presented piece of parapsychological research.

The chapter "Dream Worlds" briefly addresses precognitive dreams but deals mainly with the historic and beneficial aspects of dreams and the study of dreaming, including a few comments by Robert Van de Castle. The chapter's opening story catches the reader's interest with a powerful life

experience that could have been fatal if not for the repetitive precognitive dreams of her savior. Rita Dwyer, a research chemist, was working in a lab developing rocket fuel in 1959 when one of the propellants exploded, setting her and the lab on fire. She could not put the flames out by herself and fully expected to die. While screaming, she heard someone call her name before she passed out. She found out weeks later that her good friend and fellow chemist Ed Butler was able to save her based on a recurring dream he had for several weeks prior to the accident. He kept dreaming of being in his office lab, in shirt sleeves, without protection on, when he would hear an explosion. He would go to the doors between his lab and Rita's and hear her screaming. In his dream, he would call her name and go in and get her. He would grab her by the foot that was not burning and pull her to safety. He would get her to the safety shower, put out the flames, and then reach for the red phone (a hotline) for help. But then at this point he would wake up each time. However, during the real event, he automatically followed the actions of his dream, rescuing Rita. When he picked up the red phone, there was a coworker on the line who could help, and it was then that he realized that his nightmare was now actually a reality. While neither scientist was superstitious or believed in dreams, Rita admitted that "all dreams bring information ... when a dream repeats itself, it wants us to pay attention" (p. 69). Unfortunately Jackson does not include the rich and statistically significant precognitive dream experiments of Stanley Krippner and Charles Honorton to help offer support to the existence of such phenomena. I would have liked, in general, the inclusion of more supporting scientific research on some of the paranormal phenomena included in the book. It felt as though the chapters were often weighted more heavily in favor of the refuting skeptical researchers whose studies are offered as irrefutable evidence, even though they appeared less than impressive.

All in all *Phenomena* is an enjoyable book for children and adults. At the least, it succeeds in introducing readers to the existence of psychic phenomena and parapsychological research being conducted.

ATHENA A. DREWES

Marist College
Poughkeepsie, NY, USA
adrewes@hvc.rr.com

SPIRITS WITH SCALPELS: THE CULTURAL BIOLOGY OF RELIGIOUS HEALING by Sidney M. Greenfield. Walnut Creek, CA: Left Coast Press, 2008. Pp. 239. $24.95 (paperback). ISBN 978-1-59874-368-5.

When scholars have published a host of articles over their careers on different studies of a particular topic, like "healing traditions of Brazil,"

they often present these articles in a book. One approach is to reprint the articles as an anthology and add an introduction. Another approach is to try to unify the various articles as chapters of a book on that topic under an overarching question or problem. Hopefully, the author revises each article to fit or address that problem more directly than in the original published versions. He or she may also add additional chapters to help make the connections. This second, more difficult strategy is what anthropologist Sidney M. Greenfield attempts in *Spirits With Scalpels.* He draws from his previously published articles for the meat of each of the three main parts of his book. Eleven of these articles are listed on the copyright page (p. 2), the same articles published in his Brazilian anthology (Greenfield, 1999) and thus published here for the third or fourth time, albeit now as chapters of a book under a unifying problem and set of questions with the goal of resolving them.

In some respects, he succeeds with this second strategy. The book is organized well around the three main parts, which are framed by a nicely crafted introduction and one-page prologue and postscript. The first two parts are on four religious healing traditions of Brazil (*Espiritismo,* Brazil's form of Spiritualism; the *Cult of the Saints,* or Popular Catholicism; *Umbanda,* one of the many African-Brazilian cults; and *Pentecostalism,* Brazilian style) The third part presents psychologist Ernest Rossi's model of the healing process, which Greenfield applies generally to the four healing traditions. The book also holds together under its broader implicit purpose of offering the general educated public a well-written introduction to those Brazilian healing traditions, including how they came about in the context of Brazil's history, with brief accounts of individual participants who sought cures in three of the four traditions. We learn about the general character of the interpretations and treatments of diverse problems, and in some cases, an individual patient's specific diagnosis and treatment. Lastly, the book offers a clearly written summary of Rossi's psychobiological model of "hypnotically facilitated therapy," which proposes a plausible pathway between a suggestion and its effect on the body. The model could potentially help explain the healing process in any religious healing tradition.

But in other respects, Greenfield's attempt to make a book out of his diverse and previously published articles does not succeed. One main reason is that the purposes and supporting details of the original published articles on the healing traditions, now the chapters of Parts I and II, depart from the two major unifying aims of the book, as expressed in the introduction. These are (1) to explain the anomalous "cures" and "recoveries" of all the healing treatments he describes and (2) to explain the anomalous treatments themselves, particularly the *invasive yet painless* surgeries of *Espirita* (Brazilian Spiritualist) mediums. Both of these he proposes to do in terms of Rossi's model, given in Part III. He clearly states these purposes, early on. For the first, he writes that "the central problem we will attempt to resolve is how to explain the recoveries" (p. 10), which, for medical science,

"... are considered anomalies in the paradigms of the sciences that inform it" (p. 10). For the second aim, he says, he "witnessed forms of therapy that could not be explained in terms of scientific medicine" [patients "cut into" but not experiencing pain](p. 13) and that he was seeking "a scientific explanation for the medically anomalous variety of treatments described" (p. 16). Such a scientific explanation of both anomalies, he says, will need to be one that challenges the current paradigms of medical science (aka Kuhn). He argues that Rossi's psychobiological model of hypnosis does just that (p. 16). Although parapsychology challenges the paradigms, he implies it is not a science, and thus he rejected it from the outset: "I rejected parapsychology and other epistemological alternatives to science from the outset and assumed instead that an explanation might be found within the framework of conventional medicine and the science that informs it" (p. 16). Ironically, he nevertheless had previously submitted his work on Rossi's model for publication in one parapsychology journal, the *International Journal of Parapsychology* (Greenfield, in press), published it before that in another, *Subtle Energies* (Greenfield, 1994), and published his only "scientific" study in the book, a survey (chapter 4 below), in yet a third, the *Journal of the Society for Psychical Research* (Greenfield, 1997).

Now, the problem is this: For those two main unifying purposes of the book, the chapters on the healing traditions in Parts I and II (and their original published articles) are missing the kind of data and detail on the treatments and the "cures" that he needs to present to ground his Rossi-based hypnosis explanation of them (Part III). It was not his purpose to collect such data in his original historical and ethnographic studies, which he did before his work on Rossi's model. But there's the rub. Thus, for instance, we are never given any *medical* confirmation of just what the problems were of the patients he describes and whether they were in fact healed or not, or which were and which were not by exactly which treatment. We do get very solid, general descriptions of the rituals and religious ideology, details on the invasiveness of the mediumistic surgeries, and individual stories of healing that led patients to join a religious tradition. But these don't furnish information on the healing process per se. Thus, the problem is this disjuncture between Greenfield's descriptions of the healing traditions, the data, as it were, in Parts I and II, and his post hoc application of a theoretical model of the healing process in Part III to the anomalous treatments and ostensible "cures" of patients in those traditions. This is where the strategy of drawing on diverse articles for this book does not succeed.

The strategy's effectiveness is also diminished by yet a third, albeit implicit, purpose that shifts one's attention from the other two main unifying aims. This is to explain why and how patients move from one healing tradition to another in Brazil's "religious marketplace." He tells us in the introduction that he will offer "a model of how popular religions in Brazil interrelate with each other as competitors for the same pool of potential worshippers" (p. 16). For his analysis, he draws on the anthropological

model of the "reciprocal exchange of gifts" (p. 16). Indeed, the chapters on the three traditions covered in Part II (*Cult of the Saints, Umbanda,* and *Pentacostalism*) focus on this sociological problem. While very important to the sociology of religion, as he says, this third purpose and supporting details depart from the unifying aims of explaining the treatments and recoveries. Chapter 12, on *Pentacostalism,* doesn't even present any particular cases of healing at all!

In a sense we end up with three "books." One is on the amazing *Espirita* surgeries, Part I, reflected in the title, *Spirits With Scalpels;* a second is on the three other traditions, Part II, all on *spirits without scalpels,* and concerning the sociological dynamics of a religious marketplace; and the third is on Rossi's psychobiological model of hypnotherapy, Part III, referred to in the book's subtitle, *The Cultural Biology of Religious Healing in Brazil.* These he tries to tie together, and does so in very general terms, as I outlined above, but without the required grounding in supporting evidence for his theoretical answer to the unifying question of how the anomalous treatments and recoveries are accomplished.

As I mentioned, Part I is devoted primarily to the breath-taking and seemingly anomalous surgical treatments by possessed mediums of *Kardecian Espiritismo,* a Brazilian form of Alan Kardec's French Spiritualism, with a chapter on nonsurgical treatments toward the end. The bulk of the part consists of four specific cases of *Espirita* mediumistic surgeons, with brief mention of a sample of ostensibly successful surgeries from each: José Carlos Ribeiro in Fortaleza (chapter 1); Edson Queiroz in Recife (chapter 2); Antônio de Oliveira Rios in Palmelo, a rural town in Goiás state (chapter 3); and Maurício Magalhães in Campo Grande (chapter 4). In the first three chapters, there is much more detail on the surgeries themselves (and Greenfield's reaction to them) as the "phenomenon" to be explained, than on individual patients, their histories and reactions, and whether or not they were healed.

The emphasis of his narrative throughout those first three chapters is on his own jaw-dropping, close-up observations, and the urgent questions raised, of shockingly invasive surgeries by nonmedically trained mediums, one with but a first-grade education, in trance and possessed by "spirit doctors." To watch possessed healers move from one patient to another without washing their hands or instruments, insert their or others' fingers into open wounds to shove out a tumor, and even intentionally introduce dirt to make a point about how spirit work transcends germ theory stunned him, as did, all the more, the reports of no postoperative infections from those he talked with. And to see and film (e.g., Greenfield & Gray, 1988) patients sliced into, jabbed in the eyes and spinal cord with needles (spinal tapping), and jig-sawed open across their chests (pictures of these from his videos on pp. 41 and 56) without any anesthesia and yet reporting little discomfort or no pain at all drove Greenfield to devote much of his career to *Espirita* surgeries and to ask how in the world this could possibly be done?

But Greenfield's enthralling folk-surgery travelogue alone does not help answer that question. He does present the *Espirita* beliefs about such beneficial spirits and their otherworldly effects on human suffering—beliefs which could have served as "suggestions" predisposing patients to not feeling pain from the invasive operations. But he doesn't furnish any information on just what one or another particular patient actually did or did not believe and when, in relation to the spirit surgeries that ostensibly healed them.

Regarding the anomalous "cures" or recoveries, Greenfield doesn't present any details on the cases of healings themselves to begin to get at that question, beyond an occasional later meeting with a patient, often by chance, who professed he or she was doing well ... and thus, was "healed." The problem with patients' expressions of "doing well" after their treatments is underscored in the unsuccessful case he describes in chapter 5 (see below), where the patient also reported feeling better after treatments, but then died (pp. 67 and 71). Finkler (1983, 1985) found that a positive response to treatments and healers was much greater than the actual medical efficacy of their treatments among patients of Mexican Spiritualists. Similarly, Lynch (1996, 2005) discovered that patients of the Brazilian *Espirita* surgeon Maurício Magalhães, whom Greenfield also studied (see below), expressed more positive satisfaction with the experience than with their being "cured."

Chapter 4 is much less on that fourth surgeon, Maurício Magalhães, and his needle-puncturing treatments in Campo Grande, and more on a survey Greenfield did of this healer's patients using the questionnaire that Darrell Lynch (1996, 2005), a graduate student at the time, developed for his own study of the same healer in Fortaleza (2005, p. 13), though not acknowledged in Greenfield's text here. Greenfield portrays his survey work as an effort to fill the "follow-up" gap in his previous research of the mediumistic surgeons by inquiring whether this healer's patients were healed or not. Unfortunately, the patients surveyed were those sought by the healer's wife campaigning for his election to a political office, and may well have been patients with successful, or more likely successful, outcomes (or why else vote for him?). But in any case, though a stab at what is needed, the survey only offers a small sample of 32 patients' self-reporting of their problems and whether the treatment "helped" (p. 61) or "cured" them, and not medical confirmation of their "cures" and of "what?" Moreover, most of them were treated by medical doctors for the same problems (p. 61), further confounding the meaning of the interviewee responses. Oddly, Greenfield does not mention or discuss Lynch's (1996, 2005) more thorough study of the patients of the same healer in another city, Fortaleza.

In chapter 5, Greenfield presents an unsuccessful case of mediumistic surgery by Edson Queiroz. This surgery is of a Brazilian colleague, who was *not* cured of his colon cancer, but who, Greenfield argues, benefitted from the uplifting and meaning-imparting religious

dimensions of the *Espirita* teachings. Although this is a favorite chapter for me as a moving account of an individual's philosophical transformation during his last days, again it veers us away from what such a failure might illumine about what works, what doesn't, and why in these mediumistic surgeries in relation to what he will analyze according to Rossi's model of hypnosis.

Finally, Greenfield turns to the nonsurgical "disobsession" form of *Espirita* healing in chapter 6 for what we might call psychogenic problems. *Espiritismo* attributes such cases to "obsession" by (i.e., the influence of) upset spirits in the sufferer's past life. For the one female patient whose treatment he details, we do not know if her mental health improved or not. Chapter 7 concludes Part I with a discussion of *Espirita* efforts to gain followers by the inexplicably painless surgeries, as one possessed medium told him. For *Espirita* doctors, the real cause and treatment are at the spiritual level.

The comment foreshadows Greenfield's main concern in Part II, which, as I explained earlier, is how healers in all the Brazilian healing traditions use seemingly inexplicable treatments and cures to gain members or believers in a competitive religious marketplace. He begins in chapter 8 with an overview of Brazil's historical development of the religious marketplace and then describes three of its religious healing traditions in more detail, with exemplary religious activities: the prereformation *Cult of the Saints*, chapter 9, and a pilgrimage he filmed to the shrine of St. Francis of Assisi in Canindé, Ceará state by those fulfilling their obligation to the saint for having already healed them or their loved ones; one of the diverse African-influenced religions, *Umbanda*, chapter 11 (after a general introduction to African-derived religions in Brazil in chapter 10), and an *Umbanda* ceremony that involves consultations with possessed mediums and prescriptions for healing rites; and Evangelical Protestantism (*Pentecostalism*), chapter 12, the fastest growing religion in Brazil, which Greenfield ties to socioeconomic changes. As in *Pentecostalism* elsewhere in the world, Jesus is invoked to heal. But in Brazil, the *Pentecostal* sects also focus on healing by exorcising the various spirits of the other healing traditions to which converts previously belonged. Chapter 13 concludes Part II with a discussion of the competition for followers in this religious field or marketplace. Healing by the supernaturals for their problems is exchanged for patients' affiliation and devotion.

In Part III, Greenfield finally turns to the book's "central problem" of "how to explain the recoveries" (p. 10) and the "medically anomalous variety of treatments" (p. 16). He first prepares his reader for his answer, "hypnosis," with an introduction in chapter 14 to Descartes' mind-body dualism, science's consequent exclusive focus on the natural, the body in this case, and the relegation of the rest to the "supernatural" and the "mind" that Descartes-based science wouldn't study. Then, in chapter 15, he traces the transformation of that paradigm into a quest for the nature of the mind-body interaction.

Chapters 16 and 17 are the key to the book's aims. In the first, Greenfield presents psychologist Ernest L. Rossi's psychobiological model of hypnosis as the explanation for how patients' beliefs and suggestions by a therapist or healer about the causes of malaise (e.g., the role of spirits) affect bodily systems at the cellular level, turning on certain genes that eventually activate endorphins, our natural painkillers, and/or the immune system to heal diseases. In the second, Greenfield reviews the benefits of hypnotically facilitated therapy and, following Rossi and the observations of the famous hypnotist Milton Erikson, argues that, like all of us, Brazilian patients regularly go in and out of hypnotiform trances during each 90–120 minute ultradian cycle throughout the day. He says that these natural trances make one highly susceptible to suggestions and thus are critical for inducing first-time patients' receptivity to beliefs and healers' suggestions in pretreatment experiences.

Greenfield's analysis of patients' healing in the four traditions in chapter 18, the final chapter, is contingent on the occurrence of this ultradian period ASC for first-time patients to a healer and his religious ceremony. He seeks to identify, for instance, and naively in my view, when patients would be exposed to beliefs and suggestions in some 90–120 minute period in an *Umbanda* ritual or during pretreatment lectures and testimonials in *Espirita* and *Pentecostal* sessions to assure their "hitting" those hypersuggestible moments of the ultradian cycle. If so exposed for such a period, they would (automatically, he implies) internalize the presented beliefs and suggestions in the ultradian ASC and thereby activate Rossi's healing process. According to Rossi's model, those beliefs and suggestions, once internalized, are encoded in the brain and then transduced into the coding of the body's pain-killing and self-healing processes. The *Cult of the Saints* is more problematic, since they do not go to a healer or healing ritual but, instead, privately petition the saint in prayers to heal them or loved ones. Greenfield argues that pilgrims must internalize their beliefs about the saint and his or her healing powers as they grow up, and then, in the act of the petition to be healed, these lifetime beliefs are summoned and the encoding and transducing into bodily systems begins. But that hardly says anything new.

Clearly, as I said at the outset, this is a book geared to a generally educated public, and it succeeds in introducing the Brazilian healing traditions and such dramatic treatments as the *Espirita* mediumistic surgeries that Greenfield has documented so well, and in an engaging style that makes it a delight to read. But the problems I have outlined here and the disjunctures in content and purposes among the three parts, as well as between them and the overarching aims of explaining the anomalous treatments and "cures," will frustrate scholars and more careful readers. So, too, will the lack of any solid grounding of his theoretical explanations (not even a single case with pre- and posttreatment medical documentation) discourage social and behavioral scientists and other *JP*

readers from entertaining the otherwise provocative possibilities of Rossi's model. I recommend the book for the former, popular audience, but with considerable reservations to social and behavioral scientists and other *JP* readers.

References

Finkler, K. (1983). Studying outcomes of Mexican Spiritualist therapy. In L. Romanucci-Ross, D. Moerman, & L. Tancredi (Eds.), *The anthropology of medicine from culture to method* (pp. 81–102). South Hadly, MA: Bergin and Garvey.

Finkler, K. (1985). *Spiritualist healers in Mexico: Successes and failures of alternative therapeutics.* South Hadley, MA: Bergin and Garvey.

Greenfield, S. M., & Gray, J. (1988). *The return of Dr. Fritz: Healing by the spirits in Brazil.* Video documentary. Milwaukee: University of Wisconsin-Milwaukee.

Greenfield, S. M. (1994). A model explaining Brazilian Spiritist surgeries and other unusual, religious-based healings. *Subtle Energies,* **5**, 109–141.

Greenfield, S. M. (1997). The patients of Dr. Fritz: Assessments of treatment by a Brazilian Spiritist healer. *Journal of the Society for Psychical Research,* **61**, 372–383.

Greenfield, S. M. (1999). *Cirurgias do além: Pesquisas antropológicas sobre curas espirituais.* [Surgeries from the otherworld: Anthropological research on spiritual cures] Petrópolis, Brazil: Editora Vozes.

Greenfield, S. M. (in press). The cultural biology of Brazilian Spiritist surgery and other non-biomedical healing. *International Journal of Parapsychology,* **13**.

Lynch, D. W. (1996). Patient satisfaction with Spiritist healing in Brazil. Masters thesis. University of Tennessee, Knoxville.

Lynch, D. W. (2005). Patient preparation and perceived outcomes of Spiritist healing in Brazil. *Anthropology of Consciousness,* **15**, 10–14.

Patric V. Giesler

Department of Sociology/Anthropology
Gustavus Adolphus College
St. Peter, MN 56082, USA
pgiesler@gac.edu

THE OUTLINE OF PARAPSYCHOLOGY by Jesse Hong Xiong. Lanham, MD:

University Press of America, 2008. Pp xi + 368. $39.95 (paperback). ISBN-10: 0-7618-4043-5 ISBN-13: 978-0-7618-4043-5.

Xiong presents an absorbing but self-confessed "unoriginal" take on parapsychology, its history and impact on science and society. The title

of this quite voluminous and broad-ranging book is puzzling in its use of the definite article "The" (as in "The Outline . . ."), and at first, I suspected a metaphysical sense in its usage. For reasons that will soon become apparent, I quickly put it down to a simple grammatical *faux pas*. As it happens, it would have been more appropriate to use the indefinite article "an" (as in "An Outline . . .") because Xiong's book is just one of many in this ilk, and I don't think it is so grand that it will stand forever-more as a *foundational* text on parapsychology, as one might claim for Wolman's (1977) *Handbook of Parapsychology*. However, the book is easy to read, and what must be mentioned is Xiong's attempt at establishing a "framework and system of parapsychology" (p. ix). By the end of the book, my main concern was whether Xiong had been successful in his attempt, even with such "pearls of wisdom" as his recommendation that parapsychology would be best served by predominantly testing "star" subjects and focusing less on ordinary people—an oversight he notes in Rhine's work. With that instance alone as a "framework" or "systemic" principle, he might be on the right track, but participant preference is surely a matter dependent upon context. But at this point in the book, and then later, Xiong does admit that other aims are served by testing novices or naïve participants, which is no more than what parapsychology is currently doing anyway.

I shall come back to the "framework/system" issue later because one thing which I must get out the way up front is the poor English used throughout Xiong's book. For example: "despite plenty of evidence have accumulated . . ." (p. vii) and the peculiar and inappropriate use of the word "psychics" (when he meant "psychical research") as an interchangeable term for "parapsychology" (p. 3). My consternation was exacerbated in the discovery after only a few pages that the English translation of the original Chinese was supposedly corrected by two English-speakers, Jennifer Gerlach and Lamar Powell. (If their input is evident anywhere, it is only later in chapter 7 where the theoretical issues discussed are so complex that assistance would have been needed most, and as a result of good editing, the chapter reads quite well.) The reader may quickly overlook the problem with the English for the sake of the content. For example, a good point on the politico-philosophical reason why parapsychology is not a mainstream science is presented—it (i.e., parapsychology) would affect the "existing science framework and religion faiths, and change people's outlooks on the world and life" (p. vii). These points have been made many times over in better-written texts—evidence, as already stated, that Xiong makes no attempt to be original. But the fact remains that the ideological and bureaucratic resistance to parapsychology *is* its fundamental "bugbear." Besides, it is never inappropriate to repeat it, and often, in the hope that eventually the right people may come across this truth and maybe realise the folly of their ways. That being said, Xiong never offends, and his direct and honest tone can only be respected and appreciated.

Naturally the book starts with an introduction (subtitled What Is Parapsychology?), which takes us on a lightning-quick socio-historical tour to parapsychology's various and momentous "landmarks." This first "chapter" (not actually numbered as a chapter) attempts to explain the nature of parapsychology, which it more or less achieves with some borrowed definitions and a potted history—not such a bad thing. Many of the right names are mentioned—Dessoir, Rhine, Radin, Thouless—but there seems to be no real chronology in the telling—we go from Isaac Newton to Stephen Hawking, from *Handbook of Parapsychology* to the Society for Psychical Research, from the AAAS to CSICOP (not referred to by its current acronym, CSI), all of which seem to be thrown together to make a rather odd-tasting but tantalising stew. It is odd-tasting because the right information is there but often not in the right place, and it is tantalising because the right information is still useful and worth reading despite the jumpiness.

In chapter 1, Xiong launches into a "concise history" of parapsychology. This chapter picks up where the introduction leaves off. The author breaks the history down into chunky, oddly-named periods—"Prehistoric" (covers Greek, Judeo-Christian, Medieval, and Renaissance times), "Rudimental" (covers mesmerism and spiritualism, etc.), "Forming" (covers the "Societies," etc.), "Experimental" (covers the Rhinean era, etc.), and "Developing" (covers free-response, RNG/REG research, etc.). Xiong also manages to introduce briefly other big names, past and present, including the Fox sisters, D. D. Home, Palladino, Piper and Margery, Patrick Price, Keith Harary, Ted Serios, Ingo Swann, Uri Geller, and Joe McMoneagle (these and others are given greater focus later in the book). The status of parapsychological research in some non-English speaking countries is also discussed. This chapter too, seems rather potted and a tad encyclopaedic, as do most chapters—indeed, some chapters include voluminous quotes from other sources. But I enjoyed the entry on Robert Hare, American chemist and past AAAS president. The fact that he set out to debunk but ultimately endorsed spiritualism, only to be howled down and forced to resign as AAAS president speaks volumes. These sorts of stories abound. Also, Xiong's reviews of work and research in non-English-speaking countries are worthy.

Chapter 2 is on methodology. Xiong goes into a light discussion about experimentation, observation, and investigation. We also get a run-down on basic problems, including a good-sized section on fraud illustrated with the well-known Levy case.

Chapter 3 is on ESP and chapter 4 is on PK. The author covers a lot of ground here, which is hardly surprising given that they constitute the greatest part of parapsychology. But for any author there is always the question of knowing how far to go. Xiong spares the reader the tedious minutiae and quite successfully delivers the facts. On ESP, we not only get descriptions and examples of telepathy and clairvoyance, and so on,

but also featured are dowsing, clairsentience, xenoglossy, and the not commonly featured "clairtaste" and "clairsmell." And on PK, we get not only a run-down on Schmidt's RNG experiments, but also a brief account of an experiment Schmidt conducted with his pet cat on how it might be possible for an animal to warm itself by using "an-psi" to turn on a 200-watt lamp linked to an RNG. One criticism that must be made is that the chapter could have been updated for this English translation. For example, while Radin and Nelson's (1987) favourable RNG meta-analysis is mentioned, reference to the less favourable meta-analysis in *Psychological Bulletin* by Bösch, Steinkamp, and Boller (2006) is missing. Indeed, Xiong's source for this material is Radin (2006), in which the *Psychological Bulletin* article in question is also not mentioned. Of course, the time of writing of both authors' works may have predated the Bösch et al. study, but my point is that there was a 2-year window of opportunity for Xiong to update his book given that its publication date is 2008. Poltergeist phenomena, thoughtography, levitation, and a host of intriguing other phenomena are covered in this chapter. We also get expanded accounts on some of the big names already mentioned.

Chapter 5 is on discarnate entities. This is the biggest chapter in the book, and the author spends a great deal of time on various topics. Again there are huge slabs of text from other sources, but the material is salient, thus demonstrating Xiong's apt research skills. While whole books are devoted to the immensely complex topic of the afterlife, the chapter could have been a little longer by expanding the section on the biological aspects and implications of reincarnation in the subsection on children's spontaneous memories of their previous lives. Although there are skeptics happy to run with the overly casual (indeed lame) argument from coincidence, the anatomical evidence of birthmarks and birth defects is the most difficult to explain away, and it provides good physical support for the accounts given by the children.

Chapter 6 is Altered States of Consciousness (ASC). This is a brief chapter on the correlation between ASCs and psi. The chapter covers dreams, drugs, hypnosis, and internal attention states such as relaxation, meditation, and sensory deprivation. Not surprisingly, this chapter describes the origins of the ganzfeld—spelled "ganzfield," "ganzfled," and "ganzfed," all on the one page (p. 288), but correctly spelled in the index. Of note is the section on psychopathy and psi. Featured here are key names and short bios on Jule Eisenbud, Jan Ehrenwald, Montague Ullman, and others. It seems that there was a chance here for Xiong to delve more deeply into consciousness and its relationship to psi, but he failed to take it up (more on that shortly).

The final chapter (chapter 7) looks at various theories that attempt to explain psi. Theories by F. W. H. Myers, William James, and John Eccles are described, as are the usual theories to do with brain capability, electromagnetic radiation, quantum mechanics (QM), and synchronicity.

These entries are reasonably well described in the space allotted. It is not a particularly big chapter compared to other chapters in the book, and it is clear that Xiong has tried to write for a lay readership rather than scholars, and that means we can get only a cursory outline of some of the most influential or popular theories that might explain paranormal phenomena, but the conspicuous absence of the more heavy-going theories is probably explained by the fact that Xiong is a philosopher, not a physicist, physician, or psychologist, or even parapsychologist, and thus lacked the capacity to go beyond the basics. For example, the important work by Roger Penrose and Stuart Hammeroff on microtubules as a possible source of human consciousness, which goes a considerable way toward explaining out-of-body experiences (OBE) and near-death experiences (NDE), is not mentioned at all, even though OBE and NDE get considerable coverage in chapter 5.

As I read through to the end of the book, I felt that Xiong had more or less set up a reasonably good "framework" for the subject of parapsychology, as was his intention. But I take him to mean that his aim was primarily "structural" in the sense of wanting to cover everything, chapter by chapter, in a chronological and/or socio-historical manner. However, as for the "system," I am at a loss at fathoming his intention.

What I had hoped for in Xiong's book was an expansion on his social commentary, with some possible solutions to the ongoing crisis in parapsychology—the failure of psi research to overthrow the long-suffering ideological intolerance and prejudice that have been thrust upon it. My interest *was* piqued about halfway through chapter 7 by summaries of parapsychology and aspects of QM and their relationships with the various philosophical and religious viewpoints, especially Eastern. We are reminded that any endeavour, any intellectual journey, is not just a voyage of the mind, but also of the heart. Thus (and ultimately), Xiong speaks of a return to mystical experience, and an understanding of the nature of the Divine in discoveries of a parapsychological nature, which he relates to Eastern (and Western) belief systems by comparing specific aspects of psi with Tao, Brahmism, and (possibly too much so) Elias and Seth materials. Yet he is quick to add that the same *numinosity* resides in discoveries in the physical sciences, but he is not referring exclusively to "spooky" QM phenomena, so these discoveries may not necessarily be parapsychological, but nor does he mean them to be. This perspective left me with the feeling that the reader must believe that it will all work out, for individual researchers, for the disciplines, and for philosophy and religion, but the truth is, that outlook might be a little too philosophical for some, and it will probably be parapsychologists who find themselves waiting the longest.

At the start of the book, Xiong openly promised nothing original, but I doubt he was being fair to himself. He writes in a way that sets the thoughtful reader's mind in motion, and that has got to count for something. And for what it's worth, the information packed into this big

book does make good reading, provided the reader can get over the slight peculiarities in the translation, the grammatical flaws, the often amusing typos, and the book's unintentional quirkiness.

References

BÖSCH, H., STEINKAMP, F., & BOLLER, E. (2006). Examining psychokinesis: The interaction of human intention with random number generators. *Psychological Bulletin*, **132**, 497–523.

RADIN, D. (2006). *Entangled minds: Extrasensory experiences in a quantum reality*. New York: Paraview Pocket.

RADIN, D. I., & NELSON, R. D. (1989). Evidence for consciousness-related anomalies in random physical systems. *Foundations of Physics*, **19**, 1499–1514.

WOLMAN, B. B. (Ed.) (1986). *Handbook of parapsychology*. New York: Van Nostrand Reinhold.

LANCE STORM

School of Psychology
Deakin University
Burwood, VIC 3125
Australia
lance.storm@deakin.edu.au

PRIMARY PERCEPTION: BIOCOMMUNICATION WITH PLANTS, LIVING FOODS, AND HUMAN CELLS by Cleve Backster. Anza, CA: White Rose Millennium Press, 2003. Pp. 168. $15.95 (paperback). ISBN: 0-966435435.

The part of me that is attracted to clever, multileveled titles wants to call this book "Tracings," because it reads like a cumulative polygraph tracing of Cleve Backster's public life. Backster's writing is so direct and straightforward that one gets the impression of reading the undistorted truth, WYSIWYG, emerging directly from his viscera. But like a polygraph tracing, this book only reports the surface activities—leaving the reader to infer Backster's personal biases, hopes, or expectations, from the surface tracings. Fortunately for the reader, Backster's tracings are not difficult to interpret.

Cleve Backster is a nicely credentialed expert on human polygraph testing (lie detection). He was a pioneer in that area with the CIA, ran a private sector polygraph business and school, and contributed key techniques and methods to the field during its development. In 1966 he whimsically attached GSR electrodes to a newly watered dracaena plant in his laboratory to see if the polygraph recording would be affected as

the water rose in the plant. Instead, he found serendipitously that the chart-recording pen jumped in synchrony with a rather vicious thought that entered his mind—to burn a leaf of the plant. This was his "Aha!" experience, after which he attached electrophysiological electrodes to a variety of nonhuman things in search of "primary perception," which is his term for psi. This book summarizes these years of mostly informal experimentation.

Be clear—this is *not* a scientific treatise! Those interested in the science of biocommunication are advised to go elsewhere. Backster provides only the skimpiest of literature reviews, mentioning the names of Jagdish Chandra Bose and Harry Saxton Burr without even providing details or commentary on their works. He does not include the details of his own peer-reviewed experimental study (Backster, 1968). He briefly mentions but doesn't describe the failed replication attempts by others (Galston & Slayman, 1979; Horowitz, Lewis, & Gasteiger, 1975; Kmetz, 1977), arguing that they didn't sufficiently nurture the plant-experimenter relationship. And Backster offers no critical comments regarding his results or observations, such as their shortcomings, rival hypotheses, need for additional controls, or limitations and delimitations, as expected in a scientific discussion.

What the reader *will* find in this book is the charming story of one man's love affair with an idea and the creative ways he's found to woo it. After his initial publication, which featured plants attached to polygraphs showing deflections when healthy brine shrimp were killed, Backster happened upon variations that led to more observations. He cracked open an egg to feed his dog one night, or boiled an egg another night when he was hungry, or opened a yogurt container and started eating it—and unexpectedly observed deflections on the continuously running polygraph attached to some plant. In each case, he eventually found ways to attach electrodes to the eggs, the yogurt and, later, to human *in vitro* cellular materials, to look for polygraph deflections synchronous with the abuse of other nearby cellular materials.

Reading this book reminds me that science is but one way of arriving at truth. Just because Backster's beliefs are not yet scientifically validated (and they are not) certainly does *not* rule out their potential veracity. Backster has found his own variation on the theme of science—he seems to *want* the blessings of science but misses the mainstream by not dotting his "i"s and crossing his "t"s methodologically. He prefers to make a large number of informal, unplanned observations of spontaneous events and present them to the court of public opinion than to do the tightly controlled experiments that appeal to the higher court of science. Backster has done well in the court of public opinion. His grand idea—primary perception in plants—is apparently very appealing in this lower court. That may help explain why he's writing books like this one instead of conducting experiments that could be published in peer-reviewed journals.

In this book, Backster defends his avoidance of controlled experimentation by arguing against science's demand for repeatability, which he says destroys the spontaneity essential to demonstrating the Backster effect. He includes a section on this issue (pp. 139–141) where he argues that anyone can easily replicate his results if they assure spontaneity, by which he means, basically, to hook up the plant or cellular material for electrophysiological recording without sitting and watching it in real time (as you would for a planned experiment) but to go back later and see when something happened (a needle deflection) and examine what was occurring in the environment to explain that deflection. This is how Backster achieved numerous "high quality" observations he offers as support for the primary perception hypothesis.

The problem here is obvious to an experimental scientist—in fact, it is the *reason* we must do prospective experiments with controls the way we do. Backster's "spontaneity" technique is logically equivalent to shooting your arrow at a blank wall and drawing the target afterward! The independent and dependent variables are confused!

As an example, on pp. 116–119, Backster describes some of the "high quality" observations that appeared in his nonexperimental published report in a peer-reviewed journal (Backster & White, 1985). A leukocyte cell sample taken from the mouth of a donor was hooked up to EEG instrumentation. Later, after the donor had returned home (15 miles away), a deflection was noted on the polygraph and the timing of it was matched against what the donor reported doing at that time—watching a program entitled "World at War." Based upon this, Backster concludes, "... the donor's *in vitro* white cells in our lab reacted to the downing of the enemy aircraft." It is good science to make observations and draw one's *preliminary hypotheses* from them, but *no scientist* would ever consider stopping at that point and drawing a conclusion! A correlation was observed, without controls, and not necessarily a causation.

Backster reports that many people were impressed by such "high quality" laboratory demonstrations, and I can fully understand that. The same principle is at work in astrology circles when some pivotal event or crisis occurs in a person's life and the astrologer pulls out the birth chart and notes that some conjunction of planetary alignment "explains" the personal event. Such observations are deceptively convincing, often parading as solid scientific evidence for the veracity of astrology.

This correlation-causation issue is rather difficult to grasp intellectually. Perhaps just as difficult for the Western mind to grasp is that incomplete or inadequate evidence does *not* prove the Backster effect wrong! It could be argued that Backster's continual flow of suggestive, spontaneous, uncontrolled, less-than-conclusive observations serve a real scientific purpose here by keeping the hypothesis fresh and viable and open for discussion and for others to experiment with. But it is simply wrong for Backster to imply that the primary perception hypothesis has been scientifically validated by them.

The readers of the *Journal of Parapsychology* will probably not be the primary audience for Backster's book except perhaps for historical or sentimental reasons. I'd hesitate to recommend it to anyone new to psi research because it's not a balanced representation, and Backster himself seems not to have grasped the reality of his own "high quality" observations. Some might be turned away by the shoddy science while gullible others may be drawn in for the wrong reasons. Perhaps the best audience for this book is composed of those already inclined toward Backster's work who'd like to read Backster directly.

Backster's writing is clear and easy to read. There are very few typos—but on page 105, I had to chuckle that no editor caught the gaff that Bowman Gray Medical School is *not* at *Lake* Forest University! Scientific criticisms aside, this book can be enjoyed by a very wide audience, partly because of the lack of technical details, partly because the language is refreshingly simple, and partly because the central concept that plants communicate with humans is so very appealing to so many of us at the intuitive level. Backster's sense of humor adds a welcome lightness to the read. Another good reason to buy the book is that the profits go toward further research at the Backster Research Foundation, which is sorely needed. And any kind of research, in my opinion, is better than none at all.

References

Backster, C. (1968). Evidence of a primary perception in plant life. *International Journal of Parapsychology*, **10**, 329–348.

Backster, C., & White, S. (1985). Biocommunications capability: Human donors and in vitro leukocytes. *International Journal of Biosocial Research*, **7**, 132–146.

Galston, A. W., & Slayman, C. L. (1979). The not-so-secret life of plants. *American Scientist*, **67**, 337–344.

Horowitz, K. A., Lewis, D. C., & Gasteiger, E. L. (1975). Plant primary perception. *Science*, **189**, 478–480.

Kmetz, J. M. (1977). A study of primary perception in plants and animal life. *Journal of the American Society for Psychical Research*, **71**, 157–170.

Jerry Solfvin

Center for Indic Studies
University of Massachusetts Dartmouth
North Dartmouth, MA 02747, USA
jsolfvin@umassd.edu