JP JOURNAL OF PARAPSYCHOLOGY

Volume 74 / Number 1 Spring 2010

We would like to thank the following persons for their work in translating abstracts for the Journal: Evanne Solvinto and Renaud Evrard (French); Carlos Alvarado and Sergio Schilling (Spanish); and Eberhard Bauer (German).

GUEST EDITORIAL

Reflections on the Experimenter Problem in Parapsychology

This has been a very difficult editorial to write, as I want to advance our field, but fear I will inadvertently offend my valued colleagues, so please, give me some space here.

I began my first formal parapsychology experiment in the fall of 1956, while still a student at MIT, trying to produce veridical out-of-body experiences with hypnosis. I was thrilled to be supported by my first grant, from the Parapsychology Foundation. With the retrospective wisdom of age, I can say the design and execution was quite good for a self-taught young man hardly out of his teens, and although the planned evaluation was crude, our field hadn't really gotten good at the art of objective evaluation of qualitative results back then. At a more personal level, for more than 50 years I have been able to use the appellation so exalted in scientific circles, Experimenter, E. I will stick with the abbreviation E here, for not only are acronyms and abbreviations part of the jargon of science, E carries implications of status, intelligence, and objectivity in science, and I want us to keep those imputed qualities in mind as I share some reflections. I have functioned in many E roles since those early days and have done a lot of thinking about my role and that of others as Es. Some of these are difficult to express in a way that does not arouse emotional feelings and resistance, so please bear with me, knowing that my intention is to help advance our field.

Worldview and Experimental Design

The classical, materialistic worldview that modern science evolved and is largely rooted in is very convenient for developing current sciences. Materialism assumes that nothing exists but material objects, whose properties can be or are known, interacting in lawful ways through known kinds of physical energies. A billiard ball, as a classic example, lies still on the table top, subject to the law of inertia. When it is hit it acquires the energy of movement in a precise way determined by the angle it was struck at, the material it's composed of, the material of the cue, the intensity and exact direction of the force it was struck with, and so on. The law of inertia says a still object remains still until acted upon by a force, then once in motion remains in motion until acted upon by other forces. So the ball rolls, energy slowly being drained by the friction of the table top, and then, for this example, eventually it comes to rest again. Every parameter of material objects and forces can be measured and/or calculated with precision, and Newtonian mechanics ties it all together for both precise understanding and practical control. Physics is an "easy"

science in allowing all relevant forces and matters to be measured and calculated.

Let a person sit on that table top and life gets much more complicated. Like a material object with physical inertia, he or she may sit there unmoving until struck by something, but just as or more likely, may move on his or her own volition, unrelated to known external forces or stimuli. And if you hit the person with a pool cue, you are likely to get a far more complex reaction than rolling across the table! The study of people, psychology, is a much "harder" science than a field like physics, as it's often quite difficult or impossible to specify either the nature of the "material," the person, or the "forces"—the mechanical forces, stimuli, and communications to the person.

We have made some progress in developing a psychology by making things somewhat more like a physics experiment. We can put our test subject, S, in an isolated, quiet room, that is, with no other external stimuli/forces applied to the person than what the Experimenter, E, says to him or her, and we can deliberately limit what we will observe, such as only the person's verbal responses to what E says, ignoring the physical movements. S is still being "moved" in ways by his or her own internal stimuli, thoughts, and feelings, and his or her particular interpretations of what E says, so this makes a single trial in this psychological experiment much more variable in outcome than a physics experiment hitting a billiard ball, but we can cleverly do this experiment with many Ss and average the results, dismissing all the real, live idiosyncrasies of our Ss as "noise," and so we may find, on average, a significant relationship between stimulus and response. We don't get the precision of, "If the cue strikes the ball head on with exactly .3 foot pounds of force, the ball always rolls 3.5 ± .08 feet before friction brings it to a halt," but we might find a statistically significant tendency for Ss to speak in a certain way when E says certain things to them.

Note the importance—or lack of it—of E in these two scientific endeavors. In the classic physics study, all that is required of E is that he or she can set up the apparatus so the ball is hit with a measured force and that he or she can accurately measure how far it rolls. Other than that, the various human qualities of E are irrelevant. In the psychology experiment, on the other hand, the sensorily perceivable qualities of E may be very important and might have more effect on what S does than the formal experimental stimulation. In this paper I am going to specifically talk about SPE, E as sensorily perceived by the subject, as opposed to E, the concept of any kind of experimenter. If you are S, for example, and SPE reminds you strongly of someone you greatly dislike, you might be able to overcome this and just try to be a "good" experimental S, but then you might not and your responses will be colored by this mood. We want to study the relationship between the stimulation and the response, though, not have it biased by Ss' reaction to SPE. One solution might be to have SPE act in as neutral a fashion as possible—which may itself be upsetting to some Ss—or to run the experiment over and over with a wide variety of SPEs and Ss, trusting that, in the long run,

these idiosyncratic, human reactions balance each other out. That course has much to be said for it when you have lots of SPEs and Ss available.

Now let's do a parapsychological study. The basic design is simple. Using all we know about the physics of the material world, set up a situation where an event of interest cannot happen. For example, ask an S, acting, hopefully, as a psi-using percipient, to tell you the order of a randomized deck of cards being looked at in a locked room in a building a mile away by an agent trying to mentally send their identity. S may guess at cards in our materialistic worldview, and will score somewhere around chance level. There is no visual line of sight to identify the cards, no telephone or radio link, and even if the agent shouted out the identity of each card, at a mile range inside a separate building, our reliable knowledge of the physical world tells us that the percipient cannot hear the agent because the agent's words attenuate in intensity roughly with the square of the distance between the buildings and quickly fall below the inherent noise level of the air (Brownian motion), so information cannot be retrieved by the percipient from the agent's shouts. If we get significant identifications often enough to reasonably dismiss chance, we have a paranormal phenomenon, what we usually call telepathy. This has happened in many experiments in our field.

What is SPE's role in a parapsychological study? Everything about SPE in a psychological study applies, of course; this is a human interaction. From these physically mediated interactions we would expect variation in how successful different SPEs might be in eliciting psi from their Ss. Some long-term or short-term characteristics of various SPEs might put Ss in cooperative, psi-facilitating states, others in psi-inhibitory states, others in neutral states with no real effect on Ss' psi performances. For Ss with very strong psi abilities, SPEs' characteristics and styles of interaction might not make much difference; for Ss whose psi abilities are more delicate, SPEs' characteristics might be crucial to activating or suppressing them at any given time.

Variation in Psi Results for Different Es

From its earliest beginnings, the field of experimental parapsychology has had some Es who usually find statistically significant amounts of psi manifesting in their experiments, and others who usually get mostly chance results—we'll ignore Es in the middle of this distribution for simplicity. This was acknowledged early in the field by J. B. Rhine, who advised those who wanted to work in parapsychology to do some preliminary experiments to see if psi manifested in their studies: if not, they were advised to work in the more conceptual, rather than experimental, regions of our field.

My impression after half a century is that experimenter effects (E effects or E biases) are occasionally mentioned in both informal and formal communications among parapsychologists, but the mention is usually brief,

and seldom is anything formally done to explicate it or deal with it. We know that for a few of our colleagues, psi results in their experiments are routine, occasionally with large effect sizes as well as statistical significance, whereas for others, psi results are usually nonsignificant or of very small magnitude and hard to replicate. At least one experimenter, the late John Beloff, as an extreme example, had done so many experiments with chance results that many of us teased him that he was "psi-icidal"; he must be somehow suppressing psi among his subjects, for he ought to have had at least a few artifactually significant results by chance alone in all the studies he'd done!

In the 1960s there was considerable stir in mainstream psychology about E effects, specifically SPE effects, pioneered by the works of psychologist Robert Rosenthal, as well as by psychiatrist and leading hypnosis researcher Martin Orne, who termed them the implicit *demand characteristics* of experiments. Both argued that many apparent empirical findings of psychology about human nature could well be artifacts of SPE influence, with subjects implicitly figuring out what the SPEs wanted as a "correct" outcome of the experiment and complying. As mainstream investigators, they thought of this E influence as mediated by sensory cues or sensory characteristics of the experimental environment, strictly what we are calling SPE influence. As these can be controlled by suitable neutral environments and/or double-blind procedures, this kind of SPE effect is controllable. One of my own early studies on hypnosis at Stanford showed such bias, for example, but the study was redone in a way that eliminated the sensory biasing problem by using tape recorded instructions, so this aspect of the "SPE" was the same for Ss in all conditions.

I believe the concept of SPE bias and its supporting evidence was quite threatening to psychologists, however, and, to my amazement at the time, interest in SPE effects quickly disappeared from mainstream literature. I am not so amazed now, as I better appreciate the widespread irrational resistances of otherwise educated and intelligent people, and how we may consciously believe we seek truth at all costs but are actually strongly invested in protecting what we think we already know.

Note that the logic of conducting psi experiments, though, demands that in addition to SPE effects, E effects which may be mediated by psi, what we could call ΨE effects *must* be considered. If you're postulating that the subjects may use psi, how can you not postulate the ΨE may (unconsciously) use psi? We have accumulated a lot of experimental evidence that psi can be used without the user being aware of it, such as Stanford's PMIR effects or, more recently, presentiment effects. I believe that many mainstream investigators realize that this potential for ΨE bias is one of the implications of psi and is thus one of the reasons underlying irrational rejection of parapsychological evidence. Some, perhaps even many, of the conventional psychological effects that constitute the knowledge basis of psychology may actually be psi-mediated artifacts of ΨE bias. Deny the existence of psi, then you won't have to (consciously) worry about it as a source of bias in your

studies. It would not be the first time in history people have responded to a difficult problem by ignoring or suppressing it ...

Social Resistances to the Recognition of SPE and ΨE Effects

I have long felt that there is some irrational and formidable resistance, both conscious and unconscious, to recognizing, much less dealing with, SPE and ΨE effects. This is true for science/psychology in general, but we'll concentrate on parapsychology here. Let's start by looking at this on a social level.

First, there is the general social ethos of the scientific community. We scientists discover the fundamental laws of nature, laws independent of, more fundamental than, our mere human nature and whims. We can prove this by noting that these laws can be confirmed by any competent scientist, replicability. Take a fundamental law of electricity, for example, Ohm's law, which states that the voltage drop, V, across a resistor of value R is directly proportional to the current intensity, I, flowing through the circuit:

$$V = IR$$

Anyone who can operate simple electrical instruments can verify this relationship. I did so while still a teenager learning about electricity.

The idea that our data, our observations are affected by the characteristics of an SPE or ΨE, can imply, then, that you're not really discovering anything fundamental or lawful; what you're doing is prescientific at best and kind of stupid, or you're fooling yourself, at worst. For a parapsychologist to admit that he or she is possibly part of her experiment, then, can be seen as admitting to low, prescientific status and an implicit stupidity. As our field has been desperate to gain recognition as a part of mainstream science for more than a century, this is a hard admission for us to make—even if, as I believe, *it must be admitted. Es are part of our experiments, and to refuse to face up to that means uncontrolled variability, lack of understanding, and poor replicability*—factors which themselves are seized upon by critics to argue that there are no psi effects to begin with. This lack of recognition is particularly galling because our standards of experimentation, such as routine use of double-blind conditions, are notably higher than in most other fields of science.

Second, there is the individual identification of one's self as a Scientist. This is a very prestigious social role, which is why I capitalize the initial s in Scientist here. It could be expressed as "*I* am a Scientist; that means *I* am exceptionally intelligent, unbiased, and a seeker after Truth, superior to ordinary mortals. *I* am quite wonderful!" I've certainly felt this way at times, although I try not to get carried away with the feeling! This internalizes the norms of the broader scientific community, including the one mentioned above, that SPE and ΨE effects would be low status,

prescientific, unintelligent. Who wants to deliberately devalue one's self-image or undercut the impression of scientific competence one gives to other scientists or people in general? Once internalized, automatically identified with, norms and beliefs become much harder to recognize as norms and beliefs; rather, they become simple, automatically functioning *truths*. To some extent this internalization is conscious, to some extent automatic and unconscious, varying with individuals and situations.

Now let's look at possible psychological factors at an individual E level.

Individual Resistances

Suppose you are an E who often gets chance or trivial psi effects?

If you do not recognize or accept that SPEs of ΨEs (you) are part of the experiment, it feels like there is no problem. You may accept that psi exists, but in any particular experiment, you got no psi results as a result of extraneous factors beyond your control. The subject selection was messed up, or the room was too hot, or recent events gave subjects the wrong attitude, and so on. You are not responsible for the poor results, you're not to blame, and they do not reflect on you personally. You are still a Scientist, an E.

But if you are part of the experiment, unconsciously influencing it by psi, then you may be at least partially responsible for nonsignificant results, and that may say something about you. Perhaps at a nonconscious level that something is that you are afraid of psi (I certainly am at times!), or you don't like these particular Ss and are happy to see them "fail," for example? Maybe you're envious of a colleague's results and want to disprove them? Do you want to know these kinds of somethings? Do you want colleagues to think about those somethings? Do you want to lose the exalted status of E, the ruler, and be the same as those lower-level beings, subjects, Ss, who are subject to your rule?

I am being deliberately provocative in my language here as the E problem is so important—and so easily repressed ... But I have strong reservations about being provocative; I don't want to offend colleagues who are clearly fine people, yet I think it is so important that we look at all aspects of the E problem

Suppose you do have some feeling that some aspect of your self, your personality, your SPE self or your ΨE self, may be responsible for nonsignificant results? Intellectually, what to do is obvious. If you are indeed devoted to seeking truth, as your identity as a Scientist demands, you should investigate yourself (and other Es), including aspects of yourself that may be currently unconscious, to see how this works.

But wait! Under that civilized surface, aren't we all animals, beasts? And/or neurotic or psychotic? That's a widespread attitude in a culture where Original Sin and the Freudian Id have been dominant ideas for a long time. So exploring your unconscious for specific aspects affecting your

subjects' psi performance may consciously seem like a sensible idea, but once you start exploring, who knows what else may arise from that tricky, dangerous, shameful unconscious mind? How much do we want to hint at, much less admit to, socially condemned feelings like envy, anger, sexual desire, and so on? It's easier to deny that E is part of the experiment; that way E remains a Scientist, an exceptionally intelligent, rational being who is only searching for Truth!

Suppose, at the other extreme, you are an E who usually gets significant psi effects—perhaps even psi effects strong enough to be of practical value.

From what we know of depth psychology, this does not guarantee that, at an unconscious level, there are no conflicts about psi, so everything said above about the need to explore these effects and resistances to exploring such effects applies here. You may have as much resistance to psi as the E who routinely gets insignificant effects, but have a style of defense against dealing with the implications of this resistance that still allows your subjects to use psi. I've written elsewhere about the "religion of the .05 level," allowing statistically significant effects as long as they are actually quite weak, for example.

At the opposite extreme, though, there may be a problem of ego inflation. "*I* make the psi magic happen! *I* am not only an intelligent and superior Scientist, *I* have special powers! I am ΨE!" While this may be psychologically gratifying at one level, it may create other problems. Traditional spiritual development systems, for example, routinely warn their students not to pursue psychic powers because they may inflate the students' egos and destroy their progress on a path to wisdom and enlightenment. At a more ordinary level, literature and life experiences give us numerous warnings about the effects of hubris, and we all know of colleagues who have started out with a relatively sober interest in psi and then have gone off the deep end ...

Name Calling?

As I've touched on above, in writing this, I have worried that some of my colleagues will find these ideas personally offensive, and so reject them for irrational reasons, even though I think understanding and dealing with the E effect is of crucial importance for our field. Can't it sound like I'm saying that if you don't regularly get significant psi effects in your experiments, there is something wrong with you? You are psychologically inadequate? Or, emphasizing the feeling level, that I'm saying you are "bad?"

Like most of us, I want to be liked, so I have been hesitant in voicing these ideas over the years for the above reasons. I don't want to offend colleagues, people I generally respect and like, simply because they don't regularly get strong psi effects. But avoiding honest recognition of problems, whatever the reasons, does not get them solved.

I can hope to soften the personal implications of what I've said by admitting that I sometimes have gotten weak or nil psi effects in my own studies, and, perhaps even more strongly, note that in my life-long exploration of my own mind I have found most of the "bad" qualities we can fear and disapprove of from a primitive and bestial Id. I am no E "saint" criticizing E "sinners" for their lacks! Nor do my ideas mean that every colleague who routinely gets low results is full of unrecognized "bad" qualities. I imagine many of the E qualities that may be associated with low or high psi functioning in experiments will probably have few or no "moral" connotations. Short people do not do well at competitive basketball, for example; we don't condemn them for having "bad" genes.

I almost said "look down on them" rather than "condemn them," which appeals to my Puckish sense of humor, but we all know that humor has no place in serious scientific work, does it? Or did that tricky unconscious actually influence me to come up with "look down on" as a useful way of lightening the emotional tone so we won't lose the essential message? An encouraging reminder that there's a lot of goodness and delight to be found from exploring one's own mind?

To put it more directly, I hope any feelings of offense these ideas might generate will not be taken personally and will not keep my colleagues from serious consideration and development of them.

Is There Really Resistance to Admitting the E Effect?

This is all very intellectually interesting, you might think, but is there any actual evidence that there might be significant problems of resistance among parapsychologists with respect to E's psi involvement in their experiments?

I have raised these kinds of issues with colleagues off and on through the years. In 2008, in the course of discussing E effects and what to do about them on a prestigious internet discussion list of some 60 or so parapsychologists, many of whom have contributed to the experimental literature on psi, I asked who would consider donating a few hours of their time to beginning to study the E effect problem by taking some basic psychological tests. To my amazement—I'm an optimist and seldom apply my theoretical ideas about resistance to actual colleagues I know—almost none of them volunteered! A few rationalized that they didn't have the time. A day or so of time, compared to the years they have spent in the field, to start getting at what may be one of the most important variables affecting psi? It's hard not to think of this as resistance.

What to Do About E Effects

If we agree that E effects may be important determinants of whether and how strongly psi manifests in our experiments, what can we do

about them? Obviously we want to discover their particular nature and so hopefully be able to control them in ways that both minimize their negative contributions and maximize their positive ones. That is, we don't want unknown E effects increasing variability and unreliability in psi studies or suppressing psi altogether, and we do want to know how to select and/or train Es who will get stronger and more reliable psi effects in our studies.

The action that will definitively hinder rather than help us here is to continue to mostly ignore SPE or ΨE effects or deny them, for whatever reasons. If you have a broken leg, you don't want pain killers that will let you run around on it, further stressing and harming it; you want it set and splinted so it will heal. So what to do?

I can think of three main approaches, and I hope other experimenters will add to these possibilities. The first is to simply learn to measure various E characteristics and treat them as one more variable in experiments, the second is to create experimental rituals that are powerful enough to be strongly psi conducive in experiments in spite of E effects, and the third is to work with preselected psychic Ss who can function well under a wide variety of conditions, including various SPE and ΨE conditions.

E Characteristics as an Experimental Variable

There are significant practical problems in considering Es' characteristics as an experimental variable to be measured and studied. One is that there are very few Es doing parapsychological research, so it will be hard to get a large enough *N* to detect any but major effects at this time in our too small field. Another is that there are huge numbers of measures available, but we don't really know, a priori, what measures would be most useful. Nevertheless, we need to start, using our best guesses and just plain empirical fooling around.

Factors theoretically affecting how an SPE/ΨE interacts with Ss, both through normal means and through E and percipient psi, include long-term factors, the "personality" qualities of Es. Many psychological tests are available to assess these. Then there are various short-term factors manifesting in particular experiments. These could include the E's mood at the start of an experimental session *and changes during it.* If all we could assess would be starting mood, that might be very helpful, but it's important to realize these moods could change during an experimental session and such changes should be recorded. Too, conscious and unconscious mood and intention should be assessed, as they may not be the same. Situational factors, including interpersonal interactions with the percipients should also be noted. An E may react to a particular subject in unique ways, for example.

I can hear many Es protesting, though, that keeping track of all these things, especially with few Es available, means we're doing "case studies" rather than "real" experiments; this is not "real" science, it's low-class science at best. How are we going to get acceptance from the mainstream

scientific community if we go in the case-study direction? How are we going to maintain our valued self-image as real Scientists?

My response is that it's too bad if it's hard on our image. Our mainstream image is generally that we are a bunch of fools or pseudo-scientists anyway, simply by virtue of taking the reality of psi seriously, and this rejection will not be changed by doing lots of experiments that have "rigorous" controls by standards that apply to easier fields, where the laws of nature do indeed matter much more than the Es, and continuing to get lots of studies with few or no significant psi effects and having only a few Es (of unknown qualities) who regularly get strong psi effects. We will get the scientific acceptance we crave when we routinely produce strong psi effects in our studies and have the "recipe"—including the needed characteristics of the Es—for making those strong psi effects happen! The observations of individuals, interacting with Es of known characteristics, may lead to the keys for making psi work better.

Experimental Rituals That Outweigh E Effects

Some generally successful Es, such as Stephan Schwartz, stress that the entire experimental team is the mileu of an experiment, and carrying out psychological procedures, "rituals," that aim for success in manifesting psi, is crucial to actualizing such results. We indeed have some evidence that an individual's psi can be helped or hindered by others via psi, so this approach is obviously of considerable importance to explore and develop. I do not have a very good feel for group dynamics, though, so shall say no more about it here.

Trained, Talented, Psychic Subjects

There have been occasional Ss who have shown individually significant psi abilities in a variety of circumstances, working with various Es. Working with such talented Ss can overcome many of the problems of E effects. This is not as common as I think it should be in parapsychological studies, though, which, like psychology in general, tend to use Ss of no preselected ability. As I like to remind people, for better (generalizability) or worse (lack of exceptional talent), psychology can be semihumorously defined as the study of college sophomores by former college sophomores for the benefit of future college sophomores. With the level of manifested psi too often being quite low in such populations, I don't expect to see much progress in understanding or utilizing psi this way.

Returning to the example of my personal verification of Ohm's law given earlier, I think it likely that PK could, in principle, affect an electrical current, and so could create apparent "violations" of Ohm's law. But with the magnitude of micro-PK typically seen in parapsychological experiments, such deviations from Ohm's law in a circuit could only be

detected with extremely sensitive instruments. If we worked with trained, talented Ss who could produce very strong effects, we could discover our "Ohm's laws" of parapsychology, for the E effect would usually be reduced to barely detectable noise.

The Positive Prospects

I fear these reflections may be discouraging to my colleagues, being about more work to do, difficult work, when we already have enough troubles. So let me end on a positive note.

Ideally, I would like to say that my own experience as an E demonstrates the points I have been proposing here, namely that increasing knowledge of my own nature and of SPE and ΨE effects has been reflected by a steadily increasing level of psi results in my studies, but it's not that simple. I haven't done that many experimental studies, nor have I kept track of my assessed level of personal insight so I could compare the two variables over time.

Since my discovery of E bias effects in that hypnosis study almost 50 years ago, though, I have tried to always assume I am personally biased in designing or running an experiment, even though my conscious goal is the classic scientific one of getting clear, low-noise, unbiased data and logically and creatively thinking about it. I doubt that I have always been successful in knowing all the biases, hopes, and fears I brought to any particular experiment, but I do believe that admitting the possibility of such E effects has helped me design and run experiments where the possibilities of E bias effects happening are less. This has been part of a larger, life-long personal psychological study of myself, which itself has been an adventure. As touched on above, I've sometimes discovered than I'm much worse than I would like to think I am ... and sometimes much better! All in all, this psychological self-study has been quite gratifying and I think I'm a better person for it. If I were continuing to do psi experiments, rather than being semiretired and near the end of my active career, I believe I would be a better experimenter because of my growing self-knowledge. It's too late for me to "prove" that, but my hope is that others may be at least partly inspired by these reflections, added to their own experiences and thoughts, and discover what is and isn't important about E effects in parapsychology and how to use them to best advantage.

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THE POSSIBLE FUTURE OF PARAPSYCHOLOGY WITH SOME HELP FROM REGRESSION TO THE HYPNOTIC PAST[1]

By Etzel Cardeña

> When from our present advanced standpoint we look back upon the past stages of human thought, whether it be scientific thought or theological thought, we are amazed that a universe which appears to us of so vast and mysterious a complication should ever have seemed to any one so little and plain a thing. (James, 1896, p. 327)

In 1882, the SPR was founded to investigate "that large body of debatable phenomena designated by such terms as mesmeric, psychical, and spiritualistic," and to do so "in the same spirit of exact and unimpassioned enquiry which has enabled Science to solve so many problems" (Gauld, 1968, p. 137). Since then, the scientific acceptance of hypnosis (partly based on "mesmerism") and that of parapsychology have diverged widely. Nonetheless, many threads link hypnotic and ostensible psi phenomena and experiences both in the past and the present. One shows a consistent positive correlation between hypnotizability (the ability to respond to suggestions in a hypnotic context) and reports of anomalous experiences (some of which specifically refer to potential psi phenomena) (Pekala & Cardeña, 2000). That these reports are not just the product of faulty reasoning or strange beliefs is supported by meta-analyses in which a hypnotic procedure was found to be associated with higher scoring than a control comparison (Schecter, 1984; Stanford & Stein, 1994, Van de Castle, 1969), and by a recent study (Tressoldi & Del Prete, 2007). Besides the fact that hypnotizability is related to higher reports of psi experiences and beliefs, hypnosis involves a number of elements that may make the appearance or recognition of psi phenomena more likely, including: (a) an inward focus (Honorton, 1977), (b) a reduction of critical, evaluative thinking (Cardeña & Spiegel, 1991), and (c) spontaneous experiences of transcendence during "deep hypnosis" (Cardeña, 2005).

It is also relevant to discuss briefly the small to modest positive correlation between hypnotizability and dissociative tendencies (e.g., Butler & Bryant, 1997). This correlation hides the fact that whereas low hypnotizables very rarely manifest dissociation, a subset of high

[1] This article is the Presidential Address delivered at the 52nd Annual Convention of the Parapsychological Association, held at the University of Washington, Seattle, WA, USA, August 6–9, 2009. I am grateful for the ideas and editorial assistance of Wendy Cousins.

hypnotizables is very dissociative (Putnam, Helmers, Horowitz, & Trickett, 1995). Dissociation refers to phenomena in which there is an absence of integration in psychological processes that are normally associated, as in the case of memory or identity, and/or to alterations of consciousness characterized by a sense of disconnection with the self or the environment (Cardeña, 1994). Dissociative tendencies are related more strongly to psi experiences and beliefs than hypnotizability (Pekala & Cardeña, 2000). Also, dissociative phenomena such as mediumship, in which there may be a sense of disconnection with motor activity or with the individual's ordinary sense of identity, have had close connections to psi phenomena throughout history (Cardeña, 1998; Gauld, 1986). Furthermore, a recent study in our lab suggests that the relationship between hypnotizability and psi abilities may be mediated by dissociative tendencies (Cardeña, Marcusson-Clavertz, & Wasmuth, 2009).

Coming back to hypnosis and psi, a very long history binds them together. Shamanism, traceable to the Paleolithic era has been related to potential psi phenomena such as DMILS and includes techniques akin to hypnosis (Cardeña & Krippner, 2010). Although many shamanic performances involve what might be called trickery in order to bring about a change in the client, anthropologists have observed what might be ostensible psi phenomena in the context of traditional healing (Kelly & Locke, 2009). Probably the first written reference to hypnotic techniques is the Leyden Demotic Magical Papyrus, which describes eye and attention fixation techniques, interestingly enough in the context of trying to predict future events! The papyrus dates to around the third century BCE, although some sections can be traced to about 1000 years earlier (Griffith & Thompson, 1974/1904). Much closer to our time, reports of mesmerism or animal magnetism, initiated at the end of the 18th Century and which would eventually morph itself into what we currently call hypnosis, abound with descriptions of ostensible psi phenomena, including telepathy, clairvoyance of illnesses, hypnosis at a distance, and so forth (e.g., Crabtree, 1988; Dingwall, 1967–1968). Consistent with this possible link, part of the theoretical underpinning that in the late 18th century Franz Anton Mesmer gave to his theory, undergirded by both his notion of planetary influences and magnetism of his time, is quite congenial to psi phenomena. In one of his principles, Mesmer states that animal magnetism can take place at a remote distance, without the need of any intermediary substance (Hull, 1933), an idea that nowadays may be called *nonlocal*. After Mesmer's disciple, the Marquis de Puységur, changed the manifestations and understanding of mesmeric phenomena, the beginning of a new vision of the mind started to emerge, one in which a hidden self, accessible through hypnosis, has greater knowledge about the individual and the world (Ellenberger, 1970). Some of this knowledge, for instance about future events, would be consistent with psi phenomena.

Many if not most of the descriptions about what previously passed as mesmerically/hypnotically induced psi phenomena would not pass current methodological muster. For instance, in most (but not all) observations there was almost no control for information that might be conveyed nonverbally. Yet, there are fairly meticulous descriptions of some events, such as apparent GESP feats associated with the hypnotized Alexis Didier, that were convincing not only to the foremost magician/mentalist of the time, Houdin, but remain as difficult to explain through conventional mechanisms now as then (Gauld, 1992). With regard to those observations, Dingwall's (1967–1968) conclusion seems appropriate: "an attitude of suspended judgment both as regards the past and the present is perhaps the most judicial" (V. 1, p. 297).

The link between potential psi phenomena and hypnosis remained strong through the beginning of the 20th century. It is worth remembering that many of the founders of the Society for Psychical Research and the scientific study of psi phenomena (e.g., Gurney, James, Myers) spent a considerable amount of their time learning about, discussing, and researching hypnosis and related phenomena (Gauld, 1968). Yet if we see the current status and prestige of hypnosis as compared with that of parapsychology, it is evident that the conjoint twins separated and have followed quite distinct paths. Hypnosis is not at the cusp of "respectable" topics in the behavioral sciences, yet it has shed away the poisonous cloak that still covers parapsychology. Mainstream psychological and medical journals publish articles about hypnosis without complaints, research is funded by mainstream agencies and is carried out in some top universities such as Stanford, Harvard, or the University of London, and hypnosis has been recognized as an empirically validated treatment for a number of conditions (Lynn, Kirsch, Barabasz, Cardeña, & Patterson, 2000). Although of course there is also charlatanism and fraud around hypnosis, as happens with parapsychology, it seems to have secured a foothold in academia and clinical practice (psychology, medicine, and dentistry) and has been recognized by major psychological and medical organizations in the US and the UK, among other countries. In what remains of this paper, I will propose that the hypnosis field has used some strategies that have helped it have a better outcome than has been so far the case with parapsychology. In what follows I do not imply that hypnosis has been perfect on the points discussed or that parapsychology has not engaged with them at times, but rather that the longer and more consistent use by hypnosis of these strategies helps explain in part its greater success.

Before that, it is worth using a couple of citations to show that similar kinds of problems to the ones that parapsychology continues to struggle with nowadays also haunted hypnosis just a few decades ago. The great French psychologists Binet and Féré wrote that: "The problem of hypnotism bristles with difficulties.… Although nothing is more simple than the invention of dramatic experiments … it is on the other hand very difficult

… to find the true formula of the experiment which will give its results with convincing accuracy" (Hull, 1933, p. vii). And despite the fact that the great neurologist Jean Martin Charcot had brought hypnosis out of academic darkness, things had not greatly improved some 50 years later. The eminent psychologist Clark L. Hull wrote about hypnosis: "… the inherent difficulty of the problems involved, the fundamental elusiveness of the phenomena, and the consequent subtlety in the experimental controls. These difficulties are so great that to enter seriously on a program of investigation in this field … is almost to court scientific disaster" (1933, p. 403). To understand some of the reasons that explain the greater success of hypnosis, we need to do a little hypnotic regression into the past, so sit comfortably, breathe deeply, and notice how the calendar starts going backwards in time …

A Long History of Creative Methodology and Programmatic Research

The first divergence is that hypnosis has had a longer and continued history of good research methodology. Already in 1784, the Royal Commission's study of animal magnetism, which included such personalities as Benjamin Franklin, the great chemist Lavoisier, and others, used inventive control designs. The most famous experiment tested whether a *sensitive* 12-year-old boy could distinguish between "magnetized" and nonmagnetized trees at Franklin's estate at Passy. This was also probably the first example of a *blind* control insofar as the boy used a blindfold. The fact that the boy reacted strongly to the nonmagnetized trees supported the commissioners' report that the effect of animal magnetism was produced by *imagination* rather than by some magnetic fluid (Crabtree, 1993). I have seen films of parapsychology research two centuries later that did not use even basic controls for potential sensory leakage, although, of course, the best research in parapsychology uses good experimental controls.

Even more relevant may be the long history of programmatic research carried out in hypnosis. Hull's research with his affiliates resulted in 32 publications and additional internal reports, not to mention his suggestion for an additional 102 studies that should be carried out (Hull, 1993). Closer to our time, T. X. Barber (1995) and Ernest Hilgard (1968) also conducted dozens if not hundreds of studies that helped give the field much greater credence. In parapsychology, with few exceptions such as the successful but unfortunately too brief program of Chuck Honorton, many psi researchers tend to jump from one topic to the next, as Watt (2005) pointed out earlier, disregarding the fact that in other disciplines investigating a complex issue does require many studies, each one trying to build on previous ones.

Hypnosis and Parapsychology Are Transdisciplinary Topics, Not Disciplines

The late Bob Morris spoke for the integration of parapsychology into more mainstream disciplines such as psychology, and a number of his

students at the University of Edinburgh continue to research psi phenomena in various universities. Although perhaps necessary at the time, it is unfortunate that we have been burdened with the term "parapsychology" with its exclusive referent to psychology and its implication of a discipline as such. Going further than that integration is the proposal that research on psi phenomena requires expertise from various areas, certainly psychology, but also physics, biology, sociology, history, and magic, to name a few, and is thus a transdisciplinary subject rather than an independent discipline. In the same way that no academic or professional in the health sciences takes seriously a "doctorate" in hypnology, people who obtained a doctorate in parapsychology have found themselves in a professional *cul-de-sac*. The principles of good scientific research and thinking can be learned in the traditional disciplines and can then, with appropriate changes (e.g., methodology in parapsychology typically requires more safeguards than those found in, say, traditional psychology), be applied to study psi. On the other hand, there are areas in which parapsychology has not followed what are considered professional standards in mainstream disciplines. For instance, until the PA Board voted recently not to have full papers as Proceedings, a common practice for many researchers had been to only publish in these Proceedings the results of their work. I became acutely aware of this problem when I wrote a paper referring to a number of interesting studies, all of which had only been published in that form, something that does not happen in mainstream science.

First Establish Credibility in Another Topic

As much as we might deplore the bias in academia against parapsychology, it is probably here to stay at least for the foreseeable future. After learning good scientific thinking and methodology, which will be of great use in psi research, it also makes strategic sense to become an expert in a less controversial (and hopefully related) topic. Before I became a graduate student, I recall that Charley Tart advised all potential applicants the same thing, and after all these years I can vouch for it. I am quite certain that because a number of colleagues respect my work in other areas (e.g., dissociation, hypnosis), when they hear that I also do work in psi they at least are willing to hear more about it than if they lacked a previous vision of me as a competent researcher and theoretician. Hypnosis has partly differed from parapsychology in that some of its most important contributors first became world-recognized experts in different topics. Hull, one of the most influential psychologists of all time, wrote his dissertation on concept formation before doing any work on hypnosis and was one of the most important psychologists of his time. Something very similar can be said of Ernest Hilgard, who first became a foremost learning psychologist before he started his decades-long work on hypnosis. T. X. Barber, another important figure in hypnosis, was at least as well known as a methodologist

as he was as a hypnosis researcher, and so on. Unfortunately, besides Daryl Bem and a few others, not many researchers can claim a broad recognition independently of their psi work.

Welcome the Honest Critic

From at least the beginning of animal magnetism, hypnosis has been a very contentious phenomenon. Already at the time of Mesmer there was a conflict between those who believed that the explanation lay on some kind of substance (i.e., animal magnetism) versus those who promulgated more mundane explanations such as imagination and emotional fervor. Even after the animal magnetism explanation was discredited, other contentious debates continued among those who promulgated the idea that hypnosis implied psychopathology (e.g., Charcot) versus those who thought it was based on suggestion, and more recently the debate has centered on whether hypnotic phenomena entail a special state of consciousness or not (e.g., Kirsch & Lynn, 1995). These debates have been very healthy for the topic. For instance, the work by T. X. Barber clearly showed that many phenomena that had been assumed to be the exclusive province of hypnosis could also be effected through different mechanisms such as strong social encouragement, which did not prevent the same Barber some years later from proposing a theory of hypnosis based on fantasy proneness or dissociativity as basic processes in two of three subgroups of highly hypnotizable individuals (Barber, 1999).

Despite the vehemence with which some of these hypnosis positions have been held, I have never heard someone in hypnosis state that those who held a different view should be ostracized or somehow excluded from the field. I have had a different experience in parapsychology where even in a discussion among the Board of Directors it was explicitly stated that those who doubt the "objective" validity of psi phenomena should not be members of the Parapsychological Association, besides various other comments in PA conferences condemning in harsh terms those who do not toe the line, and some books in the field that do not even mention reasonable criticisms of the positions they espouse. Of course I am not defending those who report dishonestly their research or that of others, but those who are not as persuaded by the evidence as some of us are. They should be treated as the loyal opposition in our endeavor to get a grasp on this difficult area; honest brokers such as Marcello Truzzi can help the field enormously.

Consistent with the demonization of the "opposition" is the acquiescence of behavior by "insiders" that would not be tolerated otherwise. Alas, I have experienced personally unprofessional and boorish behavior from people within parapsychology that I have not seen in other areas (psychology, anthropology, psychiatry). This may have partly to do with a sense that given the marginal status of parapsychology people that

hold the *right* view within the field should be tolerated no matter what, whereas those outside are by definition enemies who should be kept out. At the beginning of the SPR, the contribution of skeptics such as Podmore showed that it was not just a partisan club. In recent history, one of the high points in psi research occurred when a proponent and a critic worked together on guidelines for ganzfeld research (Hyman & Honorton, 1986). On professional and ethical grounds, the field should encourage more collaborations and greater dialogue with those who hold a different perspective than ours, as long as they are ethical, honest, and respectful.

Research Process

I will now move to what are more research process oriented issues. The first one is that discrepant findings in hypnosis have sometimes been clarified because the level of descriptive specificity about the procedures used allowed the detection of subtle but important differences. A good example is the apparent inconsistent finding that hypnotic suggestions to not detect a stimulus seemed to both increase and decrease the P300 (i.e., a brain response occurring around 300 ms after stimulus presentation). This discrepancy was resolved when the two different suggestions were compared. One stated that the person would not be able to smell anything at all (which produced an increase in P300 since participants might have been surprised to find out that they could smell something). The other one, instead, suggested the blocking of a stimulus by imaging an alternative one (i.e., a cardboard box) and produced a decrease in P300, probably because of the alternate imagery suggested (Spiegel & Barabasz, 1988). In contrast, sometimes in the psi literature information about specific phrasing or other procedural issues lacks this type of specificity; paradoxically the most important lack refers to those aspects that most of us think undergird psi phenomena: consciousness and relationship. For instance, in all of the ganzfeld literature there is almost no information on the moods and states of consciousness that participants experienced during the procedure. Also, other than the few studies directly evaluating whether people emotionally close perform better than strangers, there is almost no information on the sense of emotional closeness between each experimenter and each participant, despite the literature showing the importance of emotions in psi processes (for a review see Cardeña, 2008). It is the case that this absence pervades most of the behavioral sciences, but here is an opportunity where parapsychology could lead rather than follow mainstream methodology.

With respect to consideration of psychological variables, oftentimes psi research seems to disregard the individual differences literature in psychology, and experiments are conducted without obtaining much information about personality and cognitive traits and processes that could

illuminate the different effects obtained. Although an older literature had found that personality traits generally did not seem to predict success in psi tasks (Palmer, 1977), there are reasons to revisit this issue. One of them is the finding that interactions (the joint action of two or more variables) of traits by traits by situations often explain greater variance than looking at main effects alone. It is also the case that the same procedure may have opposite experiential and brain effects depending on the predisposition of the person. We found precisely this in a study of the neurophenomenology of hypnosis in which a hypnotic procedure produced distinctly different phenomenological and brain effects depending on the level of hypnotizability (Cardeña, Lehmann, Jönsson, Terhune, & Faber, 2007).

A couple of unusual examples in the psi literature suggest how a failure to look at interactions might hide significant effects. A study on PK found that high or low personal lability had significant effects on machinery, depending on the high or low lability of the latter (Holt & Roe, 2006). Similarly, in a recent study we found that whereas hypnotizability alone did not seem to be significantly associated with a precognitive test, hypnotizability in interaction with low dissociativity evidenced a significant psi effect (Cardeña et al., 2009; see also Cardeña, 2006).

We also have evidence that some groups may score much better than others (e.g., Schlitz & Honorton, 1992), but need much more research on what specific processes, under which specific situations, and for which specific groups may be psi conducive. For instance, Carpenter (2004) discovered that self-transcendent experiences in the ganzfeld are related to psi scoring, yet this kind of work is almost nonexistent in the field. If indeed at least a number, if not most, of psi researchers have decided to ask process questions rather than just to try to obtain significant results in a psi study, it behooves us to conduct programmatic research that looks at what types of experiences and cognitive processes, in what personality traits, and under what conditions are psi conducive.

The evaluation of precise variable interactions leads directly to a greater regard for individual differences and the thorough analysis of individual cases. This approach may reveal meaningful patterns, whether they are generalizable to other individuals or not, and has already served psychology, medicine, and neurology remarkably well (e.g., Ramachandran & Blakeslee, 1999). Despite the initial, and regretfully almost forgotten nowadays, interest by the early psi researchers on a thorough investigation of gifted individuals such as the medium Mrs. Piper, current parapsychology has mostly followed the trend of conservative psychology to almost exclusively focus on aggregate, nomothetic approaches, although they are not generalizable for one, many, or all members of the group (Bakan, 1967). This approach is questionable even for the natural sciences. One of the most eminent biologists of our time, Richard Lewontin (1994), describes biology in terms of a historical enterprise because of the complexities of the variables involved and the importance of random events, and criticizes

the facile acceptance of control/prediction as the foremost models of the natural sciences.

Although hypnosis has partly suffered from an over reliance on the nomothetic approach, it has learned that some phenomena are manifested only by particular individuals even among composite groups such as "high hypnotizables" (Terhune, Cardeña, & Lindgren, submitted for publication). With regard to psi, it seems that we cannot learn much more from general samples with few if any especially talented individuals, and under the very low motivation found in an experiment as compared with a "crisis" event. Parapsychology could consider using the strategy that hypnosis does of screening large groups of people to then concentrate on the few exceptional ones that manifest interesting phenomena such as positive or negative hallucinations, or to target groups likely to have these individuals, as shown recently in a study with meditators (Roney-Dougal & Solvin, 2006). The PEAR lab data also obtained far more impressive results with some very gifted individuals than with unselected groups at large (Dunne & Jahn, 2005). As Jessica Utts (1996) has mentioned, it is easier to find the rare talented remote viewer than to train untalented ones, yet parapsychology to a large degree keeps looking for the key where there is light (i.e., studying unselected groups of undergraduate volunteers), rather than where it fell, to use the Sufi parable.

To better understand exceptional individuals also requires that unusual performances be investigated further after the original data collection. It is a truism nowadays that hypnosis does not enhance physical performance beyond conventional methods, yet there are examples in the literature that challenge this notion. Johnson and Kramer (1961) described the case of "Charles," whom they further studied after his performance during a group study went well beyond what researchers had expected. He was asked to bench-press a 47-lb barbell. In prestudy, he could do 130 reps, but under hypnosis he could do 180, 230, 333 and 390 reps, whereas a comparison weight lifter could only achieve 90, 94, 92 and 92 reps. The lesson here would be that psi experiments should *not* end when the group data are collected, but remain open for the investigation of individuals who perform noticeably better or worse than chance. It is the case that sometimes participants who have scored significantly in one run will not do so in the next (cf. Wallach, Kohls, Stillfried, Hinterberger, & Schmidt, 2009), but that is also what we have seen when researching even the most talented mediums, who have days "on" and "off." Before concluding that psi phenomena by their nature cannot be replicated (cf. Wallach et al., 2009), I think that testing potentially talented individuals not only once or twice, but many times, while measuring their phenomenology and perhaps also their physiology is indispensable. Also, some meta-analyses such as that of psi and dreams show a level of replicability that compares well with that of various areas in psychology and other disciplines (e.g., Sherwood & Roe, 2003).

Developing a Measure of Psi Talent

The success of hypnosis research lies to a large degree in its ability to evaluate individuals as to their level of hypnotizability. Although not perfect (Woody, Barnier, & McConkey, 2005), measures of hypnotizability, developed through a decades-long process, have successfully identified those who respond to hypnotic suggestions and tend to report spontaneous anomalous experiences from those who do not, allowing for various psychological and neurocognitive studies comparing those high and low in hypnotizability. Nothing remotely like that exists in the psi field. It could be that by its very nature psi is so elusive that it is not possible to evaluate this ability. I want to propose, however, that such a conclusion is premature. In hypnosis it took many decades to produce valid and reliable measures of hypnotizability, and I do not think that such a concerted effort has occurred in parapsychology. Only by following some of the suggestions mentioned here, such as engaging in a long-term, collaborative program of study to try to create such measures by looking at traits but also traits by traits by context interactions will we be justified to conclude at some point whether a measure of psi abilities can actually be developed or not. We already know that some traits are clearly associated with reports of psi beliefs and experiences (e.g., dissociation, hypnotizability, transliminality; see Cardeña & Terhune, 2008), and a program of studies evaluating those who are high in these traits is a reasonable first step in this search. There are tantalizing reports here and there of people who are much better psi performers, but I do not believe that there has been a long-term, systematic effort to find out what differentiates them from others. There is hardly a potentially more rewarding task for the field than this, although I imagine that it will take a concerted effort from a number of researchers and a certain amount of years.

Practical Benefits

As with the discussion of a measure of ability, this is another topic that will demand considerable effort and ingenuity. One of the reasons that hypnosis has gained such traction is that there is growing evidence that hypnotic techniques are empirically supported techniques to treat various medical/psychological ailments. Thus, while academics continued to debate whether hypnosis was scientific or not, many people found out that it helped them with their problems. Without a question, the search for practical uses for psi has been more problematic because of its elusive and sometimes maddeningly contradictory effects, but the field should not give up prematurely on this search. One potential but mostly unresearched area is the study of individuals that make their living out of their apparent or real psi abilities, such as dowsers, mediums, and so on. Controlled research with these individuals seems to show that they may be effective (e.g., Beischel &

Schwartz, 2007), but more needs to be done to document if and how they may help the layperson. A promising development is the interest in what the study of anomalous experiences and the differentiation between what is pathological and what is only unusual may offer to clinical work (Cardeña, Lynn, & Krippner, 2000), while also helping provide responsible and informed professional guidelines to avoid the misinformation and quackery that can plague clinical applications in both hypnosis and parapsychology.

It Takes Two to Tango

Hypnosis has been partly defined as a special kind of social interaction between two individuals (Kihlstrom, 1985), but the same thing should be said of experiments in general. In this area, generally both hypnosis and psi research have mostly suffered from the same problem, that of abstracting the researcher out of the experiment as if the latter had no effect (cf. Rosenthal, 1966). This practice, copied from the hard sciences, may be justifiable when dealing with the interaction of the experimenter and an apparatus (and even here it is arguable; Morris, 1986), but is indefensible when describing events occurring within a social system. Even if indeed the hypnotizability of the individual may be more important than that of the hypnotist, the latter will probably have an effect by, for instance, modeling what the person may experience (Cardeña, Terhune, Lööf, & Buratti, 2009). Thus it is difficult to understand why information is not obtained from the experimenter as well as from the participant, and this happens despite the literature suggesting that experimenters have an effect on psi research, whether through ordinary psychological means, through their own psi, or a combination of both (Smith, 2003). Nonetheless, only one parapsychology journal that I approached (this one) agreed that there should be some information about the experimenter(s) in research articles, whereas two other ones (*Journal of the Society for Psychical Research* and the *European Journal of Parapsychology*) did not.

Reorienting to the Present

After this survey of different topics, you can now take a deep breath and start seeing the calendar advance to the present …

I do not expect readers to agree with all the suggestions I have listed here, but if there is even only one that a researcher agrees with, I hope that s/he will implement it. Unless retired, I think that it is up to each one of us to evaluate how we may achieve further progress in the field, and this does not imply at all that the sense of excitement in this area should be sacrificed. It does demand, however, that researchers agreeing with even one of my suggestions stop complaining about the poor state of the field and work on developing these or other strategies, and reach out better to the public at large and other academics who are open minded.

That the study of psi phenomena presents enormous challenges is not news. James had already seen this: "These experiences have three characters in common: they are capricious, discontinuous, and not easily controlled; they require peculiar persons for their production; their significance seems to be wholly for personal life" (James, 1896, p. 325). I will finish these thoughts with the implications of psi for the personal and communal life. Although the possibility of psi phenomena is by no means a prerequisite for a deep regard for other sentient beings and the environment that supports us all (after all, we are interconnected in various other ways than through ostensible psi phenomena), it is a strong reinforcement for an altruistic ethic. All the choices we make have ethical implications. For instance, instead of spending time reading (and writing this article), we could have done something for the millions of sentient beings that are slaughtered or brutalized in wars, brothels, slaughterhouses, and many private homes. Thus we need to reflect on what we choose to do or not to do. Although we may argue about the specific nature of psi phenomena, its possible connections (or not) with quantum mechanics, and so forth, it seems to me that if there is anything that psi suggests, it is that we are far more inter-related than we experience consciously (Wallach et al., 2009). Thus, a defense of the helpless (human and nonhuman) and a responsible stewardship of the environment should be an obvious outcome of acquaintanceship with parapsychology, because protecting and caring for others is, in a very wonderful way, also a selfish act. If nothing else, this is a worthy enough gift from psi phenomena to all of us.

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TOWARD EVIDENCE-BASED SPIRITUALITY[1]

By Charles T. Tart

I want to thank the Parapsychological Association for the way they honored me in giving me the Charles Honorton Integrative Contributions Award for 2008. I'm sorry I couldn't attend the meeting last year to express my thanks immediately, but hopefully you who were there enjoyed the small video of thanks that I sent.

It's an obvious honor to and recognition of Chuck Honorton's work in our field for the PA to have created this award, but, to ask the eternal question, why me?

Well I admit to having been busily working away at parapsychology and related fields for some time. I had a psychophysiology laboratory for the study of sleep and dreams at University of California, Davis (UC Davis) from the beginning of my career, and many parapsychology experiments were conducted by me and my students there. My work on altered states of consciousness, meditation, spiritual growth, and the like has always had, in my mind, implications for parapsychology.

Some 15 years ago, back in 1994, I took early retirement from the University. The State of California was having big budget troubles back then—sound familiar?—so the University offered generous retirement deals to entice senior faculty to retire. I figured I could live on what they offered, and, indeed, thought of it as my "permanent government grant" to have time available for parapsychology and related interests. That part has worked out, but my other thought at the time, that in my semi-retirement I would have lots of free time to fool around with so many things that interested me, turned out to be quite a fantasy. (Making the video of my thanks I sent to the PA last year was one of those things I wanted to fool around with, but such time has been way too sparse.) Any of you thinking of retiring, be warned, it can be busier than still working!

Although officially "retired" from UC Davis, I had no intention of actually retiring. I've always liked doing research, teaching, and writing, so why would I stop? I've been teaching part-time at the Institute of Transpersonal Psychology in Palo Alto since leaving UC Davis, doing a lot of writing and speaking, and, for most of these years, running an internet discussion group of experts in postmortem survival research. Reinforcing my belief that I wasn't anywhere near "retirement," the PA honored me with an Outstanding Career Award in 1999.

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[1] This text is based on the author's invited address at the 52nd Annual Convention of the Parapsychological Association in Seattle, WA, USA, August 6–9, 2009.

This is a continuation and expansion of the invited talk in response to that Career Award that I gave to the PA at the start of this new millennium—I've been busy! The major themes of that earlier talk were the need for parapsychology and transpersonal psychology to work together, and why, on a personal level, I was proud to be *both* scientific *and* spiritual in my professional and personal approaches to life. That earlier talk to the PA was published (Tart, 2002) in the *Journal of Parapsychology*, and a comparable exhortation to transpersonal psychologists as to why they needed to work with parapsychologists was published (Tart, 2004) in the *Journal of Transpersonal Psychology*. Hopefully this talk will inspire some of our younger members with ideas about some possible and important directions our field could go in.

The PA was the first professional organization I joined—and I was very proud to be accepted—and has remained the central professional organization in my life. To be at a PA meeting is, besides hearing and being stimulated by interesting papers, to see old friends and colleagues, people who have experienced the same kind of prejudicial hardships I have in order to pursue our interests in parapsychology. So talking to you is rather more like an informal talk to "family," rather than presenting a formal, learned paper. I am also aware that I am speaking at the end of a long day of complex technical papers, so I will try to be somewhat entertaining as well as stimulating. I wish I could give you that authoritative, learned paper, based on my half century of experience, of exactly what our field needs to do to make remarkable advances, but while I have some ideas, I don't have "the Answers," so what I'll say today is more a work-in-progress report of some aspects of parapsychology that particularly interest me.

Taboo Title Slide? Taboo Topics?

At this point I planned to show a nice-looking title slide, one that gave the formal title: "Toward Evidence-Based Spirituality," author info: "Charles T. Tart," and time and place data: "Charles Honorton Integrative Contributions Award Address, PA, 2009." To honor our friend Chuck, I chose a background of pretty clouds in the sky and superimposed a photo of Chuck up in the corner. I like clouds, and thought it looked pretty.

A senior PA colleague saw it and warned me that it might be too much for a PA presentation; it looked too "spiritual" to have a wispy Chuck hovering up in the sky! Some of our colleagues have followed a purely technical strategy for gaining mainstream scientific acceptance for parapsychology, a strategy that I might oversimply describe as "leave out any references to spiritual and religious stuff or human meaning, stick to the technical, scientific analyses—*F* tests, interaction terms, effect sizes, correlation coefficients, and so on." These colleagues, I was warned, might be offended by even a visual artistic suggestion that Chuck had survived death and was somehow hovering in the sky looking down at us....

Well, I understand and sympathize with the reasoning behind that strategy. I usually use it myself when addressing audiences I think are overly attached to physical science and emotionally resistant to the possible spiritual aspects of our nature. But the reality is that psychical research and parapsychology grew out of questions about religion and spirituality, and our data are of considerable significance and meaning to these areas of life. I don't think this strategy of avoiding the spiritual implications of psi data has worked in gaining us more scientific acceptance, though. The pseudo-skeptics who attack our field aren't fooled by scientific jargon; they know psi has spiritual implications and they are against them! Judging by the blatant departures from logic and scientific reasoning so often manifested by the pseudo-skeptics in their attacks, I'd say there is a high degree of emotional energy behind their attacks, and we're not going to overcome them with rational means, even though our typical experimental procedures have long embodied the highest standards of scientific procedure.

Indeed, one of the profitable research directions I would suggest for the future is to look on at least some of the pseudoskeptics (or some scientists in general) as what I'm starting to call *spiritual beings in denial*. Without going off on a tangent here to elaborate what I mean by spiritual, let's just say that we know a lot about the psychology of denial in general, in many areas of life, and applying this psychology to understanding the intense and irrational opposition we face in some quarters might be very profitable.

Quick Preview

I'm going to cover a lot of ground in this article, so I'll list the main topic headings here. It will help in keeping the forest visible in spite of the interesting trees.

– Chuck's and my careers: some parallels
– My and others' motivation to enter parapsychology
– Transpersonal psychology
– Clarifying key terms regarding *evidence-based spirituality*
– Bringing it all together—toward integration
– *The End of Materialism* book
– Bottom line, basic conclusions
– Some speculation: Where can we go?
– Working assumptions guiding where we want to go
– In conclusion

Chuck Honorton's and My Careers: Some Parallels

In preparing a few things to say to honor Chuck (if you knew him, it's hard to get formal and say Charles Honorton), I was reminded of how

long I've been in our field when I saw the necessity of just saying who he was. Factually, I have to accept that a lot of the younger people in our field never personally knew him. Chuck and his prodigious work were so central to us for so long that it's hard to believe he died 17 years ago. So the quick overview: here's what the PA webpage says (http://www.parapsych.org/members/c_honorton.html):

> Charles Honorton (1946–1992) was, first and foremost, a parapsychologist. From his early childhood, his interests were centered on the mind, consciousness and its potentials.
>
> As a teenager, he corresponded with Dr. J. B. Rhine and, while he was still a high school student, he travelled from his home in Minnesota to Durham, North Carolina, to spend his summer months at the parapsychology Laboratory of Duke University.
>
> A good friend, a wonderful colleague, sorely missed.

Remembering Chuck and looking at this capsule bio, I was struck by how parallel our parapsychological careers were. My interests also centered on parapsychology, consciousness, and spirituality from my teenage years. It never occurred to me that I could write a physically distant, iconic figure like J. B. Rhine, but I met him my first year in college when he lectured in Boston, and we began corresponding. Looking back at that correspondence, I can see that J. B. Rhine was quite stubborn about what he thought was right, although he was very skilled at expressing it diplomatically—and I was quite stubborn too! I doubt that I had a tenth the skill of diplomacy Rhine had, though.

Fellow students and I had started a parapsychology club at MIT, and we visited Rhine's Duke lab. As I became more and more fascinated with parapsychology and disenchanted with the engineering I was majoring in, I wanted to become a parapsychologist, but knew even then how difficult it would be to make a living in our field. But I also realized I could become a psychologist, which would be close to parapsychology, and Rhine helped me transfer from MIT to Duke, where I could major in psychology. He also promised me a part-time job in his laboratory, which was a tremendously exciting prospect.

In our MIT Parapsychology Research Group, as we grandly called our student club, we also met Andrija Puharich, through the graces of Eileen Garrett, whom I had met when she lectured in Boston. We were fascinated with Puharich's experiments, for he seemed to have found an electrical method (Faraday cages in various configurations) that could selectively enhance or inhibit psi. What more interesting possibility could there be for a bunch of physics and electrical engineering majors interested in psi? We

had Puharich talk at MIT, and a group of us visited his laboratory in Glen Cove, Maine. Most of us felt his experimental procedures were basically sound, although, as in any new field, the more exploratory studies were somewhat loose. Excited—and needing summer work to help with my college expenses—I got a research assistant job with Puharich. I participated in a number of fascinating experiments, but won't go off on that tangent now.

As my correspondence with Rhine was showing, though, Rhine was the "establishment" of parapsychology, and he suspected Puharich of being incompetent, a charlatan, or both. When I arrived at Duke in the fall, I got one brief talk with Rhine, the promised job disappeared, and I was told by other lab staff I had befriended that I had been put on the list of people to be discouraged from visiting the Parapsychology Laboratory. Rhine felt that if I was immature and dumb enough to take a job with Puharich, much less defend the man's research, I was not a suitable person for the field of parapsychology. He and his colleagues had worked so very hard to create quality standards of research, and there were already too many fringe people calling themselves "parapsychologists." He didn't want one more.

This is another parallel between me and Chuck. He was as stubborn as Rhine or me when he thought he was right, and he resigned his position at Rhine's lab over some disagreement—I don't recall what it was about any more—and many other staffers resigned with him, too. Chuck and these others became some of the most creative and productive people in our field.

Of course I was angry at Rhine for not coming through on the job offer and putting me on that persona non grata list. With the wisdom of age and hindsight, though, I would have done the same thing were I in his position. I am strongly identified with our field, I've worked hard to demonstrate and encourage the highest research standards to promote our scientific acceptance, and I am at best ambivalent and often negative about wild young people who want to become "parapsychologists" when they show, to me, no basic understanding or respect of good scientific procedure. Years later, Rhine did change his mind about me and we got along fine. I would also add that I still think Rhine was wrong then on the issue of Puharich's early research. Puharich's findings that specific electrical configurations of Faraday cages could enhance or inhibit psi functioning in talented subjects was based on sound research. Insofar as it's true, it could be one of the most important findings ever in our field, allowing us to amplify weak psi, or shield it. Puharich's later involvement in many other highly controversial and fringe areas, like Nicolai Tesla's work, psychedelic drugs, and controversial Israeli psychic Uri Geller, reinforced his image in our field as just too far out and, except for one partial replication of his Faraday cage findings by me (Tart, 1988), his work has been forgotten.

The best part of my association with the Parapsychology Laboratory, though—I visited a lot to use the library and talk with other staffers, in

spite of being on the discouragement list—was the day a pretty young coed walked into the library after having heard Rhine lecture to the freshmen women and invite them to visit his lab. She asked me if I believed in ESP. She distinctly remembers my haughty reply, "It's not a matter of belief, it's a matter of evidence!" In our 52nd year of a happy marriage, Judy tells me I still use the same line a lot.

My Motivation to Enter Parapsychology

Like Chuck Honorton, my interest in parapsychology began as a teenager. In particular, it was caused by my personal conflicts between science and religion. As many people went through or are still going through similar conflicts, let me say a little about that.

I was raised as a Lutheran, through the influence of my maternal grandmother, who lived in the apartment below us for many years. My parents had no real interest in religion that I know of, but my grandmother was a regular churchgoer and she took me to Sunday school, from as young an age as I can remember. As with most kids, my grandmother was the main source of unconditional love for me, so if this religion was good enough for her, it was good enough for me! When I was 12 I attended Confirmation classes, was accepted as a church member, and went to church on my own (my grandmother unexpectedly died when I was 8) for several years. I have a photograph of me in the youth choir from back then. Me and most of the other teens and the pastor have rather frowny looking expressions: I remember that brand of Lutheranism as being big on guilt.

Two major problems arose with my simple, childhood faith, though. The first was my increasing love and knowledge of science. I was an avid reader and devoured adult books from the Trenton Public Library, especially those explaining science, and I became very aware that religion didn't make sense in terms of science. Indeed, there was a lot of nonsense under the guise of religion. Compounding these growing doubts was the special sensitivity teenagers develop to the hypocrisy of adults. Those grownups in the church were not practicing what they preached very well, yet teaching that it was the most important thing in the world! As an adult I see the word "hypocrisy" as too moralistic and strong, but as a teenager the world is very black and white, not shades of gray. (My teenage self would probably say I've sold out, of course).

With the perspective of adulthood, I see that this kind of science/religion conflict is quite common, and the uncomfortable conflict is usually "solved" in one of two ways. The first is an extremism of belief to one side or the other of the conflict. For some, their religion is The Truth and they deny and ignore any so-called science that contradicts it, perhaps seeing it as the work of the Devil. For others, they believe that materialistic science is completely correct, religion is all nonsense, often the source of great evils, and they will have nothing to do with it. The second solution is a kind of dis-

sociative compromise: religion is something accepted on special days, but has little thought given to it in the rest of "ordinary" life. As a psychologist, I know that any of these defense strategies is costly. Conscious conflict is reduced or eliminated if you deny science or religion. But if, as I believe the findings of transpersonal psychology and my own experience convince me, we have a genuine spiritual side as well as having and expanding wonderful scientific knowledge, then while lots of religion does indeed not make scientific sense, wholesale denial and suppression of either side keeps us from being whole, and involves a myriad of psychological costs. The dissociative compromise similarly exacts psychological costs from us. Mapping out these defenses, their costs, and their consequences will be a very useful line of future research.

I was lucky, for my voracious reading led me to the early psychical research literature, and so I discovered a third solution. Here I found that intelligent men and women in the late 1800s went through conflicts between science and religion just like I was going through, but they came up with an incredible idea. Instead of wholesale faith in any religion or in the completeness of any current, materialistic worldview in science, why not apply the *methods* of science—careful observation, development of hypotheses, and logical testing of these hypotheses—to the phenomena associated with religion, and so start a sorting process? Some religious ideas, beliefs, and phenomena might have a reality basis, others might indeed be partly or totally nonsensical, but we could gradually refine our religions and spiritual systems in ways compatible with scientific method.

The emphasis was on scientific *method*, not the current findings, the *corpus* of science at any time. Each era in science too often believes that its findings are the final word on Truth—but that's just typical human arrogance. The beauty of science is that its "beliefs," its theories, are *always* subject to revision as new facts are discovered or old ones refined. In my own lifetime I have seen numerous "scientific truths" overturned. It was practically dogma, for example, when I was young, that extrasolar planets were extremely rare if they existed at all, and we would probably never find any. Now they seem to find a new one every month.

Using basic scientific method to refine our knowledge of the spiritual—which includes many things we might prefer to label psychic—has been the basic theme of my career. My personal conflict between science and religion was solved—I can deal with both domains, always trying to observe carefully, conceptualize as clearly as I can, and not get so attached to my concepts that I become blind to new data. I can now succinctly express the goal of my personal and professional life as helping to develop an *evidence-based spirituality*. Or, as it's quite complicated, at least an *evidence-enriched spirituality*.

I should note too that my voracious reading included much more than science. I read Theosophical books, books on yoga, comparative religion, meditation, magic, philosophy, and so on, and so forth. So as a teen-

ager I was pouring the "wisdom of the ages" into my mind. As well as the "nonsense of the ages." I'm still doing a lot of sorting out.

Others' Motivation to Work in Parapsychology

I noted above that conflicts between science and religion were not mine alone; many people go through them. Back in 2002, I wondered how specifically spiritual motivations or attempts to resolve science/religion conflicts applied to other parapsychologists. The pseudoskeptics certainly think they are important, and often accuse us of trying to push a religious agenda on people while disguising it as science.

To look at this empirically, I did an email survey of members and associate members of the PA, with a good response rate (Tart, 2003). Table 1 shows the main results for the first question in my survey.

Table 1
Motivations for Entering parapsychology

"Did you enter the field of parapsychology because of, to some significant degree, what we might call 'spiritual' interests or motivations, i.e., important concerns with questions of meaning, spirit, connection and the like?"			
Yes	No	Partly	Unclear
36%	49%	9%	6%

If you prefer that our field seek scientific acceptance by downplaying any spiritual implications of psi data, you can cite this finding as showing that the largest group of parapsychologists surveyed deny having spiritual interests as motivation for getting into the field. If you think it's best for us to deal with the spiritual implications of our data, you can say that almost half of us (45%, the "yes" and "partly yes" responders) reported that spiritual interests were important in bringing them into the field.

Other aspects of my survey, though, found significant numbers of us feeling somewhat frustrated that the current climate of our field downplays spirituality and makes it difficult to express interests in spirituality. That ethos is one of the reasons why I often define myself as a "transpersonal psychologist," rather than as a "parapsychologist."

What Is Transpersonal Psychology?

As my 1975 book *Transpersonal Psychologies* (Tart, 1975b) helped to establish the discipline, I don't feel too presumptuous in defining what this field is. My book was actually called *Spiritual Psychologies* up until the last minute. In my personal explorations of various spiritual growth systems,

I had noticed that they all contained extensive psychologies, that is ideas about human nature, its development, cognitive functions, and so on, the kinds of things that contemporary psychology deals with. These psychologies, often stimulatingly different from current Western ideas, were usually buried in the religious aspects of the system, though, and so were unlikely to be discovered by and be of use to students of psychology. I wrote three introductory chapters about this and then had experts in various spiritual traditions write about these traditions as psychologies, rather than as religions or spiritual systems. I found the results fascinating, and naturally named the resulting anthology *Spiritual Psychologies*.

As the book was going to the printer, I got a call from my editor at Harper. He had good news and bad news. The good was that the country's biggest psychology book club wanted to adopt the book as a monthly selection! I was thrilled; this would help it make an impact. The bad news was that the club's editors stated that psychologists could not deal with the word "spiritual"; spiritual stuff was just too weird and taboo for psychologists. Could we change the title to drop that word?

And so *Spiritual Psychologies* became *Transpersonal Psychologies*. A few of us Californians were using the term "transpersonal," literally beyond the personal, as a new term to stimulate the founding of a new field to take the spiritual seriously and study it, but the vast majority of psychologists or people in general had never heard the term "transpersonal," so had no conditioned reactions against it.

So what is the field of transpersonal psychology? Here's a definition I created, with help from my colleagues, that we used in the catalog for the Institute of transpersonal psychology for a few years.

> Transpersonal psychology is a fundamental area of research, scholarship and application based on people's experiences of temporarily transcending our usual identification with our limited biological, historical, cultural and personal self and, at the deepest and most profound levels of experience possible, recognizing/being "something" of vast intelligence and compassion that encompasses/is the entire universe. From this perspective our ordinary, "normal" biological, historical, cultural and personal self is seen as an important, but quite partial (and often pathologically distorted) manifestation or expression of this much greater "something" that is our deeper origin and destination.

This covers the field well (although there are constant debates about just how to define this young and still developing field), but to put it more briefly, transpersonal psychologists think that some of what we call the spiritual might be real, and that we should find out which parts are and aren't real, study the nature of those parts, and learn to apply them more ef-

fectively to improve our world by helping people have transpersonal/spiritual experiences.

Putting it another way, transpersonal psychology is a discipline working toward developing an *evidence-based spirituality.*

How do the fields of transpersonal psychology and parapsychology relate to each other?

Parapsychology Is to Transpersonal Psychology As Physics Is to Engineering

Physics deals with the basic properties of the material universe; engineering creates useful devices and processes utilizing and working within the basic properties physics has discovered. Analogously, parapsychology discovers basic properties of human consciousness and transpersonal psychology creates effective applications. To concretize this analogy, an engineer might want to build a bridge, but physics tells him that the material he wants to use isn't strong enough for the load; he will have to use a stronger material or a different bridge design. A transpersonal psychologist might want to create a high-powered machine to increase the efficacy of psychic healing, for example, but a parapsychologist might tell her that it's a basic finding that machines don't seem to do anything psychic on their own; they just give the operators of the machines confidence and permission to use their own psychic abilities. Thus the transpersonal psychologist might be advised not to waste resources on actually increasing the power of some machine supposed to produce psychic effects, but to use the resources to increase the *appearance* of power to the users so their own psychic abilities might function better.

I've written extensively on the relations between the two fields (Tart, 1981, 1996, 1998a, 1998c, 1993, 2002, 2004).

This analogy is useful at the present time, when transpersonal psychology is more involved with applying spiritual ideas to help people than with fundamental research into the nature of spirit and consciousness, but this could change in the future and transpersonal psychology could become as basic as parapsychology.

Personally, defining myself as transpersonal psychologist gives me more semirespectable room to maneuver in than defining myself as a parapsychologist. I can legitimately show more concern for the implications of psi, especially their spiritual implications. Then my parapsychologist colleagues who want to keep trying the abstract science, leaving-all-that-spiritual-stuff-out strategy for winning mainstream acceptance can more easily distance themselves from me: "He's a transpersonal psychologist, not a parapsychologist." But note that identifying myself as a transpersonal psychologist, with a specialty interest in parapsychology, is one way in which I integrate my varied interests and try to broaden parapsychology. Under other circumstances, of course, I am quite happy to identify myself as a parapsychologist with a specialty interest in transpersonal psychology.

I say semirespectability, as for many mainstream psychologists, transpersonal psychology still means Kooky California Psychology.

"Progress" note: At my age and semipseudoretirement status, I care a lot less what the mainstream thinks of me, although I still try to move them toward a little more openness. Whether this reduced concern with my image is a boon or a menace to our field: Well, we'll see ...

So this article is centered round a theme of *evidence-based spirituality*. Let's clarify this and related terms.

Clarifying Key Terms

People are always insisting that we must have clear, unambiguous, and comprehensive definitions of key terms before we can make any progress in most fields. I think it's wonderful when we can do this in some fields, but in others I think this insistence is a stumbling block that inhibits research. The field of consciousness studies is one area like this. There is constant discussion and argument over how to define "consciousness" on the *Journal of Consciousness Studies* online list, and, frankly, I don't bother to read these discussions anymore. I don't think they are going anywhere. The process of defining things is one small part of the totality of what we refer to as consciousness. Why should we expect a small part to be able to absolutely define the whole? We can make it reasonably clear what we mean by consciousness *in specific contexts*, though, and so get on with the work. Without any claim of absolute comprehensive accuracy and definitiveness in all contexts, then, here are the ways I'm using some important terms in calling for an *evidence-based spirituality*.

Evidence

Key elements in the definition of evidence, from the *Shorter Oxford English Dictionary* (*SOED*), are "... 2 An indication, a sign; indications, signs" and "3 Facts or testimony in support of a conclusion, statement, or belief. ... Something serving as a proof." We all naively like to think there is no question about this; facts are facts, proof is proof. Thus our parapsychological data leave no doubt that various forms of psi exist, right? Well, as parapsychologists we know that it's not that simple. If a "fact" doesn't fit with someone's belief system or worldview, that person is quite likely not to see it as a fact at all.

What a person believes and accepts as evidence supporting one's beliefs can come about for many reasons other than scientific research. Authority-based beliefs are extremely common. You believe X because some Authority said it was true. Cultural conditioning is another source of belief. You absorbed what everybody "knew" to be true in the course of growing up. Hope and fear also play a huge role in determining our beliefs, as do various psychodynamic factors that have nothing to do with rationality. All these factors interact too.

There are more or less persuasive aspects of evidence. If I were to make a rough listing from least persuasive to most persuasive—a listing not everyone would agree with—I would start with personal observation near the bottom of the list (personal in that I observed something or people I know say they have observed it). But how good an observer am I? How good are they? How will my rating of my or their goodness as observers interact with what I want to believe?

We can have some disagreement here, of course. If I am enamored of myself as thinker and observer, I'm liable to put my personal experience of something at the top of the ranking of evidentiality!

Moving up the list, we come to common knowledge, adages, and anecdotes. My favorite example of common knowledge is the adage I learned as a child and that all the adults I knew accepted as true, the advice "Stuff a cold and starve a fever." That seemed clear enough. If you have a cold, eat a lot; fast if you have a fever. But while taking a course in historical linguistics, my wife Judy read that the original form of this old English saying was "Stuff a cold and starb o'fever." "Starb" is a now obsolete word derived from the German *sterben* and means "to die." So the old advice is not to stuff someone with a cold but *not* to do it, lest they die of the resulting fever! Common knowledge may not be very reliable.

Moving further up the evidentiality spectrum, we might find miscellaneous case histories that seem to show a common theme. If these were systematically collected, rather than casually collected, we'd be inclined to give them more weight, and even more weight if they had been subjected to some clear, logical analysis leading to a firm conclusion.

In this age of science, though, we give much more validity to evidence that comes from experiments. We can start with simple, crude experiments and then add factors like adequate sampling of relevant populations, single and double blinding to minimize experimenter effects, and meta-analyses over large bodies of experiments to get strongly convincing evidence of some effect.

Of course actually being able to demonstrate the effect on demand is even better!

But remember, all of these levels and kinds of evidence interact with our beliefs, so what is strong evidence to a person with one belief is dubious to one with another.

Evidence-Based

In advocating an *evidence-based spirituality*, I'm obviously drawing a parallel with evidence-based medicine. This is a fairly modern ideal that all medical practices be based on the highest quality studies that provide evidence that a particular treatment will actually be practically effective for a specific medical problem. I describe this as an ideal, rather than an accomplishment, for most medical practice is still based on tradition or on lower

quality studies that are below the contemporary "gold standard" of extensive, double-blind, placebo controlled experiments. Indeed this "gold standard" is questioned by some physicians, for while those kinds of studies may be useful for making general decisions about populations of patients, the knowledge and "clinical intuition" of an individual physician dealing with a particular patient is still vitally important. So some kinds of treatments have evidence-based support, but many do not. We don't have an evidence-based medicine yet and perhaps never will have such completely, but we certainly have an evidence-enriched medicine. Similarly with my proposal for an *evidence-based spirituality*. What we have now is almost entirely a matter of traditional lore and individual knowledge with almost no experimental studies of outcomes or effectiveness. Given how little we know about what spirituality actually is, and what is or isn't effective for individual practitioners, we will have to draw primarily on lore for a long time but, realistically, I believe we can create an evidence-enriched spirituality within a decade or two.

Spirituality

The aspects of the word "spirituality" of interest to us here, from the *SOED*, are

> ... 3 The quality or condition of being spiritual; regard for spiritual as opposed to material things; specifically the study and practice of prayer, especially as leading to union with God. .... b A spiritual as opposed to a material thing or quality. ... 4 The fact or condition of being non-physical....

I don't think we can adequately define spirituality in ordinary consciousness language, just as we can't adequately expect the part to define the whole in the case of "consciousness." Spirituality often involves altered states of consciousness (ASCs), which means possible state-specific perceptions, feelings, evaluations and actions that do not translate adequately into the state-specific functioning of ordinary consciousness. For our purposes here, "spiritual" points toward ultimate values and meanings primarily involving nonphysical aspects of reality. Note too that spiritual values are usually considered far more important than material values to the experiencer of spiritual epiphanies.

Religion

The relevant aspects of the *SOED* definition of religion involve

> ... 3 Belief in or sensing of some superhuman controlling power or powers, entitled to obedience, reverence, and worship, or in a system defining a code of living, especially as a means to achieve spiritual or material improvement;

> acceptance of such belief (especially as represented by an organized Church) as a standard of spiritual and practical life; the expression of this in worship etc. ....

In accordance with most writers, I use "spirituality" largely to refer to individual experiences and their effects on individuals, with "religion" referring more to the social organizations that form in response to spiritual experiences, but which involve numerous adaptations and compromises to fit with social structures. I am something of a loner and don't have much feel for social factors, so will say little more about religion in this talk.

God

I usually avoid using the words "God" or "gods," as they tend to tap powerful emotional sources, both positive and negative, that too seldom mix well with rational, scientific discussion. Nevertheless, many spiritual experiences, especially among Westerners, involve "God," so we parapsychologists may have to use it at times to deal adequately with the reality of human experience, whether we're worried it will scare away mainstream colleagues (or each other) or not.

The *SOED*'s relevant entry is

> 1 A superhuman person regarded as having power over nature and human fortunes; a deity. Also, the deity of a specified area of nature, human activity, etc.... 5 In Christianity and other monotheistic religions, the supreme being, regarded as the creator and ruler of the universe and source of all moral authority.

I am glad to be able to cite the most authoritative dictionary here, rather than implicitly being so arrogant as to think I can adequately define a Being or beings who are supposed to be enormously more intelligent than me. Contrariwise, I am often amused at the implicit arrogance of militant atheists who, in effect, say "I am so intelligent that I can state with absolute conviction that there cannot be any being in the universe more intelligent than me!"

As an empirical and pragmatic scientist, I usually regard "god" or "God" as a shorthand way for people to express their theories, their beliefs, about the nature of reality and a being or beings more intelligent and powerful than us ordinary humans. These theories/beliefs may be relatively automated conditioning and indoctrinations stemming from childhood training or, in some cases, new ideas or modifications of old ideas based on individual spiritual experiences. Few people recognize that their beliefs are largely *theories*, though, or are willing to subject them to the imperative of science that *all* theories are subject to modification or rejection and must have testable, observable consequences. Indeed the emotional asso-

ciations and investments connected with most people's religions and ideas of God make it threatening and heretical to even think about testing their theories.

I would note that while there is a lot of current argument and debate about science and religion in Western culture, it is almost all too theory-specific, with no recognition of this narrowness. That is, it's about God as a bearded, old Middle-Eastern patriarch as the only way of thinking about spirituality, when actually there are many kinds of spirituality and spiritual experiences that must be considered as data in developing a more comprehensive theory of the spiritual. As a psychologist, I often look at some of these debates and think, "This scientist must have had a bad childhood experience in Sunday school and is still emotionally wrought about it..."

Materialism

The relevant aspect of the *SOED*'s definition of materialism is "The doctrine that nothing exists except matter and its movements and modifications. Also, the doctrine that consciousness and will are wholly due to the operation of material agencies."

I'm not concerned with the many philosophical variants of materialistic theory here, but with what we might call classical "man-in-the-street" materialism. I usually represent this in lectures by a picture of a billiard table, with someone getting ready to strike one of the balls with a cue. This is man-in-the-street materialism: the world is made of solid hunks of stuff. That stuff just lies there, inert, until some material force whacks it, and then it flies off in accordance with just how it was whacked. In the more elegant language of the *SOED*'s definition of "inertia": "The property of a body, proportional to its mass, by virtue of which it continues in a state of rest or uniform straight motion in the absence of an external force."

I've always been intrigued by thinking that most early physicists were members of the intelligentsia or aristocracy, and those gentlemen—they were almost all men back then—played billiards. How much has the physics and mechanics of that game intrinsically shaped our ideas of the physical universe? It's also amusing to think what physics might be like if they had played golf instead of billiards. I don't play myself, but I'm told that there is a great deal of prayer for success, cursing for failure, and superstitious ritual on golf courses. Perhaps modern physics would have much more emphasis on chance, intention, prayers, and curses than on exactly how you whack the billiard ball atoms if they had played golf.

Scientism

This brings us to a key term for parapsychologists, for anyone trying to understand the social impact of modern science, and especially for people who have had spiritual aspirations or experiences but think science

has shown them to be all nonsense, namely *scientism*. The *SOED* defines it as "Excessive belief in the power of scientific knowledge and techniques, or in the applicability of the methods of physical science to other fields, especially human behavior and the social sciences."

It's a wonderful thing to be a scientist; I'm quite proud to claim that title. Why not feel proud when you and your colleagues discover better understandings of the principles which govern reality? Being human, though, justifiable pride easily slips over into rigidity and arrogance, and you start automatically thinking you understand the Laws of Nature. No more real thinking is necessary; you are at the pinnacle of understanding. Scientism stems from that. The current scientific understanding of nature becomes psychologically indentified with, emotionally attached to. With intellectual and emotional attachment, a defensiveness develops; you don't like reports of things which don't fit your superior understanding, which might question your superiority. The founders of psychical research that I referred to earlier were aware that science too easily becomes scientism, that reliable relationships too easily become The Laws and the mind closes down. The fact that some aspects of the then-current religious beliefs were contradicted by the current scientific knowledge became a rationalization for rejecting *all* of religion and spirituality as nonsense. The founders of our field had the intelligence to see that the *methods* of science—the collection of empirical data, construction of logical theories to account for that data, and constant testing and revision of such theories by new data—were indeed a very useful way of advancing knowledge, but you had to keep the essential science process open and moving, not freeze it at some current point.

All the purportedly logical and scientific rejection of our principal findings on the reality of fundamental psi phenomena is a scientistic, not a scientific, rejection.

In my recent *The End of Materialism: How Evidence of the Paranormal Is Bringing Science and Spirit Together* (Tart, 2009), I frequently touch on the pathologies of cognition that keep us from advancing our knowledge, and scientism is a particularly pernicious kind of pathology. Being genuinely scientific is a high-class position in our society, but scientism is the delusion that one is being genuinely scientific, being genuinely skeptical in the sense of open-mindedly searching for better explanations of reality, while actually being simply prejudiced. As delusions go, scientism is about as high-class as you get.

Toward Integration

I'm speaking to you as a result of receiving the Charles Honorton *Integrative* Contributions Award, so let me now focus more on the topic I've been building up to, integration—or lack of it—of my other interests and our field of parapsychology.

I'd like to claim that all aspects of my professional and personal work have reinforced and contributed to all other aspects, that it's all synergistically integrate—but I can't. One major reason, of relevance to most of us, for what we might call tactical isolation of parapsychological aspects of our work, is the intense and irrational prejudice we face in scientific circles just for being parapsychologists. We often have to be careful of what we say to what audience.

To make a romantic comparison, my career has been somewhat like the old 50s TV show *I Led Three Lives*. If any of you are old enough to remember it, the protagonist of that series, Herbert Philbrick, was an advertising executive by day, but also a secret member of the Communist party, and also a counter-intelligence agent for the FBI. My professional life has not been that dramatic, but I have led three lives that I usually kept pretty separate for the different audiences they were presented to. (Note that I'm talking about my public professional lives here, but privately my professional and private interests were often all contributing to one another.)

One life was as experimenter and investigator of consciousness, particularly ASCs like hypnosis, dreams, and drug-induced ASCs. My first book in 1969, *Altered States of Consciousness: A Book of Readings* (Tart, 1969), turned out to be very timely and helped establish the study of ASCs as a respectable part of psychology and psychiatry. This book was pivotal in helping establish my "respectable" credentials and gain me tenure at UC Davis. I don't need to tell this group how useful tenure is if you want to devote even part of your work to parapsychology! Having a steady job that I couldn't easily be fired from (although I could be hassled in various ways) was a great foundation to do parapsychological work from.

I always devoted a substantial part of my professional work to studying consciousness. My most creative contribution was probably my proposal to create *state-specific sciences* (Tart, 1972, 1988b), to use the different perceptual and thinking perspectives in various ASCs to do scientific work and so expand our view of reality. Little practical application has come of this proposal yet. I like to think I was ahead of my time, although I sometimes consider the hypothesis that perhaps the idea, as some critics who insisted that science can only be done in "normal" consciousness claimed, didn't really make sense. My systems approach to understanding ASCs (Tart, 1975a) was also, I believe, a major contribution, but, like the state-specific sciences proposal, either ahead of its time or (I hope not) somehow flawed.

Chuck Honorton and I often discussed ASCs, and he told me that my ASC book and work was one of the inspirations that started him on ganzfeld studies, so this may be one of the best contributions and integrations of this line of my work into parapsychology. In general, I see in almost all of our social/psychological conditioning that there is no psi as occurring in our ordinary state of consciousness, so, aside from their specific qualities, ASCs give us states where that conditioning is weaker and so psi may not be so inhibited.

My second professional life was as a parapsychologist. My second scientific publication (Tart, 1963) was on physiological correlates of psi cognition, that experiment, "legendary" among my colleagues, that nobody quite seems to personally want to repeat, where the experimenter, acting hopefully as a telepathic agent, is given severe electrical shocks at intervals while correlates are looked for in the percipient's physiology. It was excruciatingly painful to me as experimenter/agent, but well worth it in terms of data and ideas produced. I should note, though, that I haven't been motivated to be shocked again.

Three other lines of work, one almost unknown, have been my major contributions within our field. One was the application of basic learning theory to multiple-choice ESP guessing studies (Tart, 1966), arguing that lack of immediate feedback in massed trials constituted a classical psychological extinction paradigm, and, as would be then predicted, the decline effect was common in our studies. *In a percipient with enough psi talent to begin with*, the learning process ought to overcome the inherent extinction effects created by being right by chance alone that occur in any multiple-choice mass trials study, so increases in scoring, learning, should be apparent. My initial studies strongly supported this (Tart, 1976); my second study, with significantly less talented percipients did not produce learning, as would be expected (Tart & Redington, 1979). Curiously—or as a sign of unconscious resistance to strong psi functioning, something else I have written about at length (Tart, 1982, 1984, 1986, 1999; Tart & LaBore, 1986)—the few colleagues who followed up my work with feedback studies ignored my clearly stated requirement of using talented percipients to begin with, and, as predicted, got no results.

A second contribution was my psychophysiological study of a young woman who could have out-of-body experiences (OBEs), which demonstrated the feasibility of taking an exotic experience like the OBE and studying it in a laboratory setting (Tart, 1968).

My third, almost unknown, contribution concerned the military proposal in the late 70s and early 80s to build the MX missile system, a multibillion dollar project to constantly shuttle intercontinental nuclear ballistic missiles (ICBMs) among concealing silos connected by an elaborate railway system. There would be lots of silos that hid the missiles, many more than the actual missiles we could afford to build. The idea was that it would discourage the Soviets from launching a first-strike attack: They couldn't afford to build enough ICBMs to hit all the silos, so enough of our missiles would survive to destroy the Soviet Union. We believed in those days—it all seems rather insane now—that the doctrine of Mutually Assured Destruction, MAD, would keep either superpower from initiating nuclear Armageddon. The cost for MX was going to be absolutely enormous, of course.

I was working as a consultant for the remote viewing project (Targ & Puthoff, 1977) at SRI International at the time. This required a Top

Secret security clearance, since much of the funding for the research came from the military and intelligence communities, so we had a lot of government connections. Hal Puthoff took that data from my first UC Davis study of immediate feedback as a way of learning better ESP abilities, applied the level of psi functioning shown in it to the statistical problem of which of the proposed missile silos to target with a limited number of ICBMs, and mathematically showed that using psi to facilitate your targeting made the odds of the Soviets successfully wiping out all of our missiles much higher—a first strike might well be worth it. We knew that the Soviets were devoting significant resources to parapsychological research, so ... Hal told me that he communicated this analysis to high-level people in Washington and it was an important reason for cancelling the proposed MX missile system. I'll probably never know how much of this story about the effect of the analysis in Washington is true, but I like to believe it.

My third professional life was as a transpersonal psychologist. My *Transpersonal Psychologies* book, mentioned earlier (Tart, 1975b) when we discussed the term "spiritual," helped to establish this field, and I have also written several books on mindfulness practice, adapting some old spiritual ideas to work more effectively in the modern world. I have been teaching at the Institute of Transpersonal Psychology since retiring from UC Davis, and it's been most satisfying. I teach a course on mindfulness, one on altered states, and one on basic parapsychology. I regard the parapsychology course as especially important in the intellectual preparation of transpersonal psychologists, so when they are criticized as California Kooks with a PhD who believe in psychic and spiritual stuff, they can cite our numerous studies of psi to show a scientific foundation for the transpersonal approach.

My Current Integration: The End of Materialism Book

Being in the latter part, indeed perhaps being near the end, of my career, I decided that integrating my various strands of knowledge in a way that might be helpful to people was more important than pursuing new discoveries and refinements, so I spent the last 3 years writing my integrative book *The End of Materialism: How Evidence of the Paranormal Is Bringing Science and Spirit Together* (Tart, 2009). I have no delusions that this is the final word on these subjects, of course; it's just the best sense I can currently make of the relationship between science and spirituality, mainly through implications of the data of parapsychology. I'm a pragmatic empiricist, though, so any ideas in the book are always subject to change as further data comes in.

The End of Materialism, a title I owe to Matthew Gilbert at the Institute of Noetic Sciences, the copublisher of the book, is a dynamic, attention-catching title that I am quite charmed with. You can appreciate some of the flavor of the book, though, with other very accurate, but not as catchy, possible titles it could have had, such as *Implications of Parapsychological*

Findings for the Spiritual Life or *The End of Dismissive Materialism* or *Scientific Foundations of Transpersonal Psychology.*

The book is not a comprehensive survey of the latest and greatest in parapsychological research: I refer readers who want that to books like Dean Radin's (Radin, 1997, 2006). Nor is it a "sophisticated" discussion of the finer points of our research, such as whether clairvoyance could be better explained as precognition of future sensory feedback about the identity of targets. It's not particularly aimed at parapsychologically sophisticated readers like this audience, although I think you could find many interesting ideas in it. Indeed, it's deliberately "old-fashioned" in most of its discussion of experimental data to make it easier for the average person to follow. Or, perhaps I use older studies mainly because I'm "old-fashioned?" (;-)

My primary identities as the author of *The End of Materialism* were first as a psychologist concerned with helping to alleviate useless suffering, second and more specifically as a transpersonal psychologist interested in people's spiritual development, and third as an educator, where my primary tasks are to share useful information with people and stimulate their thinking.

It's also a much more personal book than my previous writings, using my own studies of some phenomena to illustrate them to the reader. They are not the methodologically best studies I know of in the literature, but they establish a more personal connection between author and reader. I've slowly learned, despite my education/brainwashing about the "objective" impersonality of science, that people learn more from personal connections.

Who is *The End of Materialism* written for then? For the many people who have spiritual aspirations, or who have had spiritual experiences, but believe they have to repress or deny them because they "know" that Science long ago proved that all religious and spiritual beliefs were nonsense, or neurotic, or both. These people are suffering and the suffering is not only useless, it is based on the false beliefs of scientism that they have been indoctrinated in.

There are certainly many beliefs classified as religious or spiritual that are factually incorrect or that satisfy neurotic needs—as there are in all areas of life. But my argument is that this total, blanket dismissal of any possible reality to the spiritual is bad science. It's scientism, it's dogmatic, pseudo-scientific adherence to a doctrine of materialism, not real science. When you use real science, essential science, as we do in our field of parapsychology, you discover that certain psi effects have a very high certainty of being objectively real. They cannot be explained by dismissive materialism, and they are the sort of qualities we might expect a "spiritual" being to have. Therefore it is reasonable to be *both* scientific *and* spiritual in one's outlook. That's my personal integration of my scientific and spiritual lives, and I want to show others they can overcome needless worry and suffering about being dumb or neurotic because of their spiritual interests because of reasonable implications of our parapsychological data.

What's in the book? After introductory material about spontaneous cases (I use one of my own experiences to help form that bond with the reader), then a discussion of how we can properly use science to discover and refine knowledge, and then a look at various emotional and cognitive obstacles that distort and inhibit scientific progress, I survey parapsychological data under two main categories, the Big Five and the Many Maybes. The Big Five are the foundational findings of our field, the phenomena we have so much and varied evidence for that, I argue, no reasonable person could doubt their existence. I then discuss telepathy, clairvoyance, precognition, PK, and psychic healing as our fundamental Big Five.

A sophisticated audience like this can have many discussions of whether some of these Big Five are really versions of another, like maybe psychic healing is "merely" a form of PK. Perhaps it's really a Big Four, a Big Three, or a Big X, but, as I mentioned above, the book, my integration, is aimed at ordinary people.

I then move on to the Many Maybes category, phenomena for which I think we have enough evidence to argue that they might exist and have enormous implications for life if they do exist—and certainly should be further investigated, not ignored—but where there just isn't enough definitive evidence that most of us would feel comfortable saying they have been *proven* to exist. The ones I discuss are postcognition, OBEs, near-death experiences (NDEs), and evidence bearing on postmortem survival, such as after-death communications (ADCs), mediumistic communications, and reincarnation cases. You could all add more Many Maybes, of course, but I didn't want to overwhelm readers of the book, just to show enough findings to flesh out the idea ("flesh out" seems like a funny phrase when we're talking about postmortem survival ...) that there's good scientific evidence that we have the kinds of qualities we would expect spiritual beings to have.

Bottom Line, Basic Conclusions

I'll draw two primary conclusions from the book, and my and our work, relevant to integration.

First, Dismissive Materialism is scientifically inadequate as a total explanation of human life. It's an overgeneralized philosophy that simply does not account for our parapsychological data, and any scientific theory that does not account for all the data, while claiming to, is inadequate at best.

Note carefully that I'm not dismissing materialism as a working scientific theory per se. It's very useful in many areas of science, especially those we call the physical sciences, to assume that stuff is material and obeys certain physical laws. What I am rejecting is Dismissive Materialism's claim to totality, its automatic dismissal of all data, observations about spirituality in particular, that don't fit in with it. I'm especially rejecting the harm it

does to real human beings by automatically dismissing any and all of their spiritual longings and experiences as inherent nonsense.

Second, our parapsychological data forces me to conclude that people sometimes show the kinds of qualities we might expect "spiritual" beings to have. I'll just briefly touch on these, since my time here is running out, drawing only on the Big Five. For example, people occasionally show evidence of telepathic contacts with other humans. Isn't telepathy just the sort of "carrier mechanism" we would need to take the transmission to a distant or nonphysical entity in prayer seriously?

We have some data suggesting that people may sometimes have psi contact with animals too. Well, is it too unreasonable then to think that psi contact might occur with nonphysical, spiritual entities?

As another example, people sometimes show clairvoyant contact with distant or shielded aspects of physical reality. A primary spiritual experience is the mystical experience of Unity, of feeling at very deep levels of being that one is an integral part of, united with, all life or all the universe. Dismissive Materialists would say this must be a malfunctioning of brain circuits that make us aware of our biological boundaries, the skin encapsulating us. Well maybe sometimes, but the existence of clairvoyance, and the fact that with present knowledge we cannot put any limits on what is accessible clairvoyantly, must make us think that perhaps the feeling of unity with the rest of life or the world has some reality to it, rather than being nothing but brain malfunctioning.

Precognition ... Well, it's hard for me to speculate about the spiritual aspects of precognition, since the phenomenon makes no intellectual sense at all to me, in spite of the fact that the evidence forces me to admit that it exists. It's at least a good reminder that our understanding of the universe is a lot less comprehensive than we would like to believe it is.

As a final example, people sometimes affect physical processes through intention alone, PK, and/or affect biological systems, psychic healing. Well, would "spiritual beings" be of much interest to us if they couldn't affect the ordinary reality we live in?

So, I reiterate my general conclusion: It's reasonable to be *both* scientifically *and* spiritually inclined.

And my general reservation: All areas of human life have lots of nonsense in them; developing discrimination is absolutely necessary.

Some Speculations: Where Can We Go?

It's personally very satisfying to me to be able to tell people that, based on my own and my colleagues' scientific work over many decades, it's reasonable to be *both* scientific *and* spiritual. But my call for discrimination is even more important. The worst thing that could come from our work would be for people to take the attitude that *anything* that is labeled spiritual or religious or psychic is certainly true! To begin discerning the

true from the false, the spiritually enriching from the delusion-enhancing, the true-enough-to-be-useful from the used-to-be-good-in-past-times-but-doesn't-work-with-moderns, we have to study and experiment with religion, spirituality, ASCs, parapsychology, and so on. This idea of "experimenting with" will be threatening to those who are overly attached to their particular religious and spiritual systems, of course, but knowledge advances by questioning received wisdom and experimenting with new possibilities.

To mention just the principal fields that will contribute to our experimentation and discernment, parapsychology is primary, of course, in determining what does and doesn't have some reality basis, but we will draw extensively on psychology and transpersonal psychology, on the physical sciences, on consciousness studies, especially of ASCs, and on sociology and social psychology since we are social creatures, strongly affected by our cultural and group milieus. Traditional religions and spiritual systems will interact with all of the above, both as sources of inspiration and wisdom and as diversions and emotionally loaded blockages. Plus ... many other areas of knowledge we can't even think of yet.

I will, of course, sound the traditional academic refrain: We need more research on everything! And it's true!

Working Assumptions Guiding Where We Want to Go

Research always takes place in an intellectual and emotional milieu, of course, so I think it would be useful to briefly sketch the research assumptions that I see enriching our knowledge of the spiritual:

– We are "spiritual" beings in some real and important sense.
– We make a lot of mistakes and suffer a lot through ignorance and prejudice; in traditional spiritual terms, we are "Fallen."
– We have a capacity to learn and improve.
– Present spiritual systems and religion are a mixture of the valid/important on the one hand and nonsensical/neurotic/subverted on the other.
– We can at least come closer to truth even if we may never know about Truth in some absolute sense.
– We can enrich and refine our spiritual practices by research.
– We can create an *evidence-enriched spirituality.*
– Creating an evidence-enriched spirituality is one of the most important activities we can undertake, if not *the* most important!

There's a big and exciting job awaiting us!

Now let me finish up by giving some more concrete examples of the kinds of research we might undertake in developing an evidence-enriched spirituality.

Making Meditation More Effective

From the perspective of Dismissive Materialism, the various forms of meditation practice are relatively inefficient ways of rearranging chemical and electrical patterns in the brain to achieve certain ends. Someday we'll understand the brain well enough to do this far more efficiently through direct chemical or electrical means, so research on the chemical and electrical properties of the desired brain states should be most efficient in reaching our goals.

But if, as our parapsychological data shows, we humans are something more than just our brain functioning, the traditional spiritual claims that meditation practices can lead to real spiritual goals, to something presumably more than merely chemical and electrical rearrangement of brain patterns, then meditation practices are much more important. The experience of contact and union with all of life, for example, is sometimes induced by various meditation practices, and we've raised the possibility earlier that this may involve some sort of genuine clairvoyant contact with the universe, not simply a malfunctioning of the brain mechanisms that keep us aware of our biological boundaries. Since also learning to make psi function more reliably is an important goal in our research, and we have some indications that meditation practices may aid this, meditation research is of importance to our field. And perhaps learning to make clairvoyance function more reliably will lead to more Unity experiences ...

Speaking some years ago with Shinzen Young, a pioneer in adapting Eastern meditation techniques to make them more effective for Westerners (Young, 2005 and http://shinzen.org), I asked a question about how effective meditation training was. He noted that his experience, and that of other meditation teachers he knew, was that almost everyone who was taught basic meditation in classes or retreats found it rewarding and intended to make it a regular part of their lives. If you came back a year later, though, if 5% of them were still meditating, you were doing very well as a meditation teacher!

As a university teacher, I was horrified! If I were running a college and 95% of my students left in the first year, I'd think there were major problems with our teaching style and effectiveness. Maybe Western teachers weren't very good yet? Shinzen assured me that it was this way in the East with traditional meditation teachers too, 95% dropping out within a year. They "explained" it as the workings of karma. If you had good enough karma from your previous lives, you would seek out a meditation teacher; then you would stick with the meditation and get somewhere. But most people have poor karma, so naturally they don't stick around. Maybe if they are good in this and their next lives, they will come around again for meditation instruction a few lifetimes down the road.

Well maybe. But this idea of good and poor karma also struck me as functioning as an excellent rationalization to avoid facing up to the fact

that even "good" meditation teachers don't know how to teach meditation very effectively. Thus our need to learn how to teach more effectively, and I think fairly straightforward psychological research can accomplish that goal by studying what kinds of meditation techniques work for what kinds of people, then directing people to methods and teachers appropriate for their type.

The Reality, or Lack of It, of Nonphysical Worlds

A spiritual world with "spirits" in it implies some kind of world or worlds where those spirits exist. Back in 1986, at our Rohnert Park convention, I proposed a general methodology for exploring the reality or lack of it of various "nonphysical worlds," a proposal subsequently published (Tart, 1987). In ordinary, physical reality, if someone tells us of the existence of some particular place, call it World X, we can sometimes go there ourselves to verify the existence of that "world." If we can't go ourselves, we can compare the accounts of various travelers who claim to have been to World X. If these accounts are very consistent *and* we can plausibly rule out other factors creating pseudoconsistency, such as their all having read each others' accounts before talking with us, we have evidence that World X exists in some relatively real way.

For example, my friend Robert A. Monroe wrote extensively about his many OBEs (Monroe, 1971, 1985, 1994). Occasionally he experienced traveling to ordinary world locales, and he could verify later that his recall of what he had observed there was objectively correct. Most of the time, though, he could not recognize where he was even though it seemed like some place in our ordinary world, or he experienced some unusual, clearly not-of-this-world place. A few times, when he employed a particular and, for him, unusual technique for leaving his body, though, he found himself in a world of experience that seemed perfectly real, was consistent in its general features from OBE to OBE, and while seeming to be a physical world, was clearly not our world. The technique? Instead of his usual waiting for or inducing a state of "vibrations" in his body and then floating up and out, when the vibrations began he turned himself 180° around the long axis of his physical body, so it felt as if he were lying prone on his bed, within the space of his physical body, rather than on his back. He then reached over his head with his arms, felt a wall there with a hole in it, and pulled himself through the hole.

I don't have time to go into detail here—it's described in Monroe (1971, pp. 86–100), but if we could find other voluntary OBErs, or train people to be OBErs, *and we could be sure they had never heard of Monroe's description*, we could ask them to employ the same method and see if they gave descriptions of their experiences that were consistent with Monroe's account and consistent with each other. Perhaps they wouldn't ... but if they did, wouldn't that be interesting?

Researching NDEs

Back in 1975, when Raymond Moody published his *Life After Life* (Moody, 1975) book on NDEs, I became fascinated by NDEs as ASCs. In fact, I usually differentiate OBEs and NDEs by reference to the experiencer's state of consciousness. In most OBEs the experiencers report on how clear, ordinary, and rational their consciousness was throughout the experience, as if they were normally awake, but just happened to be elsewhere than where their physical bodies were. NDEs often start with this kind of OBE, but typically the functioning of consciousness alters radically and there are new, ineffable (in ordinary language) styles of perception and knowing, the hallmarks of an ASC.

What was even more interesting about NDEs back then, though, was their ostensibly parapsychological aspect. The vast majority of people had never heard of NDEs. They had little or no expectations, or only traditional religious expectations, about what dying would be like. Yet NDErs from all walks of life gave quite consistent accounts of their experience, often contradicting traditional religious teachings. If NDEs were only "subjective," semiarbitrary products of a dying brain's malfunctioning, we would expect great individual differences in the experiences, and their content would largely reflect the beliefs and social conditioning of the people having them. This consistency was, on the other hand, what we would expect if there was something "real" and universal about the dying process.

The enormous popularity of Moody's and others' books about NDEs has vitiated most content comparisons of NDEs happening today, though. So many people have now read an article or book or seen a TV documentary about NDEs that, insofar as NDEs are at least partly subjective, consistency among new accounts has been programmed in by ordinary means. We could say our NDE observers are now more likely to be strongly biased by past knowledge than they used to be.

Nevertheless, we have not yet reached the stage of detailed phenomenological studies of NDEs, detailed questioning by skilled professionals to help experiencers go somewhat further on detailed description of the usually ineffable. The discovery of deeper levels of consistency in NDEs would be as interesting as the discovery of consistency in nonphysical worlds accounts discussed above ...

Operation of Karma

The concept of reincarnation is almost always inherently coupled with the idea of *karma*. Karma, for the *SOED*, is defined so: "In Buddhism and Hinduism, the sum of a person's actions, especially intentional actions, regarded as determining that person's future states of existence." If we can collect and study evidence on the reality, or lack of it, of reincarnation, one

of the Many Maybes in *The End of Materialism*, can we study karma? There's a "noise" factor: Hindu and Buddhist concepts of karma point out that we have many karmic tendencies from our many past lives, so particular manifestations of karma, the "ripening of karmic seeds," depends on appropriate circumstances in this life occurring.

Let's say we have a child, then, who claims to remember a past life and provides sufficient details to identify an appropriate past person that the child seems to be a reincarnation of. The "theory of karma"—for we can take Hindu and Buddhist beliefs as working theories—says we shouldn't expect the child, as he or she grows up, to be exactly like the previous identified personality. Circumstances may not have occurred in this lifetime that would activate all of the previous personality's karmic propensities. But we would expect a general, statistically significant correlation.

The late Ian Stevenson and his successors at the Division of Personality Studies at the University of Virginia have, I believe, about 4,000 such cases of childhood recollections of apparent past lives, and about 2,000 of these cases have been computer-coded and entered into a database. I expect to see all sorts of interesting findings emerge from analyses of this database.

But here's one particular, informal "study" already done that suggests the falsity of one of those "theories" derived from Buddhism.

A teaching I have heard from a number of Buddhist teachers over the years, especially Tibetan Buddhists, is that there are a number of "realms" or conditions of existence a deceased person may reincarnate into. One of these, the human realm that we live in, is the most favorable for working toward enlightenment; it has the right balances of pleasure, pain, and intelligence. But, the teaching goes, it is very difficult to have or gain enough "good" karma to be reborn in the human realm rather than some other realm. The analogy given is to imagine the world is one big ocean, and floating in that ocean is a 6-foot diameter ring. A turtle lives underwater in that ocean, and once every 1,000 years swims up to the surface to take a breath. What are the odds that the turtle will just happen to surface within the ring? The moral is that unless you work very hard in this life to accumulate good karma, you are extremely unlikely to be born as a human being in the next life.

If this theory of the rareness of appropriate karma were true, I would predict, then, that among those children who recall previous lives the great majority of them would be reincarnations of holy or saintly people, people who had accumulated the needed appropriate good karma. When I informally asked my friends and colleagues at the Division of Personality Studies on our internet discussion group for a general impression, though, they thought there were maybe half a dozen or so cases of yogis, nuns, or otherwise obviously religious previous personalities among the 2,000 cases they had already coded. The overwhelming majority of previous personalities were ordinary people.

I can sympathize with the probable motivation behind the ring and turtle story: make good use of this human incarnation for your spiritual development. But I think the literal truth of this aspect of reincarnation theory is incorrect.

OK, that's enough of far-out ideas. Far out compared to our typical quantitative lab studies, but not so far out when your goal is to develop an *evidence-based* or *evidence-enriched spirituality.*

In Conclusion

I've been working in our field for more than half a century now, and I'm still fascinated by it. I'm fascinated at what we might call the "techno-nerd" level; I love the details and cleverness of experimental design, and I'm even more fascinated by the implications of our findings, and pleased that they can, in my integration, help reduce the useless suffering created by Dismissive Materialism.

I'm not satisfied with this article, though. I've been changing its content right up until the last minute; there are so many interesting ideas I'd like to pass on and there just isn't time! But I'm not done with our field, and for many of you younger colleagues, you're just in the early beginnings of a fascinating career.

I've gone on too long, but my last bit of advice is to remember, no matter how fascinating the techno-nerd side of parapsychology is, that it's also very much about the spiritual nature of human beings—and there's little that's more important than that.

Thank you again for honoring me with the Honorton Award! I look forward to the many interesting ideas and findings that will enrich our field as they are brought in by others from other fields.

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DÉJÀ VU: ORIGINS AND PHENOMENOLOGY: IMPLICATIONS OF THE FOUR SUBTYPES FOR FUTURE RESEARCH

By Vernon M. Neppe

ABSTRACT: An analysis of déjà vu subtypes is done in accordance with Neppe's universally accepted operational definition of déjà vu (any subjectively inappropriate impression of familiarity of the present experience with an undefined past), the 30 different circumstances for "déjà experience" and the 50 postulated explanations for déjà vu. Neppe hypothesized and then demonstrated 4 phenomenologically distinct nosological subtypes representing 4 different, distinct populations motivating 4 etiologically distinct kinds of déjà vu: subjective paranormal experience (SPE) déjà vu (in subjective paranormal experients), associative déjà vu (in ostensible "normals" or subjective paranormal nonexperients and also in nonepileptic temporal lobe dysfunction and nontemporal lobe epilepsy patients), psychotic déjà vu (in schizophrenics) and temporal lobe epileptic déjà vu in temporal lobe epliptic patients. The approach used serves as a model for phenomenologically relevant analyses in neuroscience, psychology, psychopathology, and parapsychology. This allows standardized, relevant recordings and also requires development of further appropriate questionnaires to ensure phenomenological homogeneity in further research and meta-analyses. Subjective paranormal experience déjà vu has implications for precognition, reincarnation, and dreaming.

Keywords: déjà vu, déjà experiences, multidimensional scaling, nosological subtypes, phenomenological approach, population differentiation, SPE

What Is Déjà Vu?

> We have all some experience of a feeling that comes over us occasionally of what we are saying and doing having been said and done before, in a remote time—of our having been surrounded, dim ages ago, by the same faces, objects, and circumstances—of our knowing perfectly what will be said next, as if we suddenly remembered it.
>
> (Dickens, 1850, Ch. 39).

What is *déjà vu?* For the layperson, it is, technically, the "as if" experience, as if I have "already seen" it before. But in reality, it is far broader. So, déjà vu may literally mean already seen, but it can also mean already heard, already met, already visited, and numerous other "already" experiences. It is not "I have done it before and I know exactly when; I recognize that I'm doing it again." The reason why that is not déjà vu is because the recognition is consequent on a real familiarity, whereas with déjà vu, the familiarity is inappropriate—it doesn't fit.

The formal, recognized scientific definition of déjà vu, which has become accepted world-wide, appears to be quoted in every major article on the subject and derives from my own PhD (Med) thesis at the University of Witwatersrand, Johannesburg, South Africa (Neppe, 1981c). Déjà vu is "any subjectively inappropriate impression of familiarity of the present experience with an undefined past." The definition was reflected in my 1983 book *The Psychology of Déjà Vu: Have I Been Here Before?* (Neppe, 1983h). Every one of these words is relevant and the definition will be revisited throughout this paper.

When Was Déjà Vu First Described?

Déjà vu goes back a long time and the historical landmarks are worth noting: Pythagoras 2400 years ago supposedly described the phenomenon, which was also reported by Ovid some 400 years later (Funkhouser, 2006). St. Augustine (416/2002) was responsible for the first explanation of déjà vu some 1600 years ago, when he said it was due to some deceitful spirits. The first book referring to this phenomenon, describing it even before David Copperfield, though not yet naming it, was Sir Walter Scott's (1815) *Guy Mannering*. A poet also described the phenomenon during the mid-19th century—Dante Gabriel Rossetti in his 1854 poem *Sudden Light* (Schacter, 2001). The first attempt at scientific explanation of this phenomenon comes from A. L. Wigan (1844) in his book *Duality of the Mind*, in which he explained the phenomenon as delays in the different functions of the cerebral hemispheres. The first thesis on the subject was French, from Bernard-Leroy (1898).

When Did the Term Déjà Vu Officially Arrive?

It derives from France in the late 19th century, and books will tell you the official name was given by F. L. Arnaud (1896). (Try as we may, we cannot locate Arnaud's first name). Arnaud described it as *sensation du déjà vu* and argued that it was distinct from other memory distortions, as it was just a bad judgment—misattributing the current to the past (Schacter, 2001). But in fact, 20 years earlier, Emile Boirac (1876) described *le sentiment du déjà vu*. A string of French writers—Boirac, Arnaud, Ribot, Fouillee, Lalande, Ferenczi, Ribot, Loti, Gilles, Kindberg, Méré, Dugas, Le Lorrain, and Leroy—all used the term, consolidating its appeal (Neppe, 1983d). This was important because there had been a debate of the idea in an 1893 special issue of *Revue Philosophique* of whether one paramnesia alone existed (Dugas, 1894; Lalande, 1893).

In the meantime there were some alternative "pretender" terms deriving from recognition of false memory, or false recognition by leading pioneering psychologists and philosophers: Bernard-Leroy, Biervliet, Dugas, Freud, Heymans, and Laurent all referred to it as *fausse reconnaissance* or *fausse mémoire* (Arnaud, 1896; Dugas, 1894; Funkhouser, 2006; Neppe,

1983h). Henry Bergson (1908), who pioneered a great deal in terms of parapsychological thinking, called it *souvenir du present*, and Bourdon came back to it, calling it *reconnaissance des phénomènes nouveaux*. Montesano, in Italy, realized this was the *falsa intuizione di ricordo*, and Emil Kraepelin in Germany used some German terms, *Erinnerungsfälschungen* and *Fälschen Wiedererkennen*, from Lehmann and Linwurzky (Funkhouser, 2006; Neppe, 1983h).

You will notice that the 19th century pioneers therefore used terms that were mainly French, but Italian and German also had their terms, though there were none yet in English. Almost every subsequent term on the subject has been in French, and modern researchers have continued this tradition.

The Lancet, the still famous English medical publication, became the first scientific journal to describe déjà vu and reflected the coloring of the culture at that point in time (Crichton-Browne, 1895). It was somewhat esoteric and a source of pride to have this experience, as Crichton-Brown described:

> No doubt these dreamy states are very common amongst us at the present day, but it will, I am sure, be found on enquiry that they are by no means all-embracing, and while they abound among the educated, the refined and the neurotic classes, they are comparatively rare among the prosaic and the stolid masses of our people. (Crichton-Browne, 1895, pp. 73–75)

Between then and now, a vast literature has accumulated on this entirely subjective phenomenon. Another famous literary example was found in a fictionalized explanation in Joseph Heller's (1961) famous novel *Catch-22*. Did Wigan's ideas 117 years before of a hemispheric difference causing what was effectively déjà vu, influence Heller's character? "Yossarian shook his head and explained that déjà vu was just a momentary infinitesimal lag in the operation of two coactive sensory nerve centers that commonly functioned simultaneously" (p. 268).

The stimulus for the modern differentiation into demonstrable subtypes began in 1971. While a medical student in 1971, I was intrigued by several contradictory paradoxes. I learned in my psychiatry course that déjà vu was symptomatic of temporal lobe epilepsy, yet my further research showed that 70% of the population had this experience (Neppe, 1983f). I also wondered whether so-called "psychics" were having a different kind of experience (Neppe, 1983c).

By 1979, one of the problems was that there was a lack of consistency in screening for and eliciting the déjà vu phenomena, and this made data interpretation difficult (Neppe, 1981a, 1981b, 1981c). Because there were only 12 kinds of déjà experiences, such as, *déjà fait* (already done), *déjà pensé* (already thought), and *déjà raconté* (already told), it was difficult to describe

the déjà phenomenon. As of 1979 nothing really existed to screen for the phenomenon. Most déjà vu studies were based on only one question.

When Did the Modern Era of Déjà Vu Study Begin?

By 1979, when I began my research, we had certain known principles. The literature supported déjà vu occurring at least once in a lifetime in about two thirds of ostensibly "normal" individuals; this information as of today still appears to be correct. Secondly, déjà vu was regarded medically as common in patients with temporal lobe epilepsy. Thirdly, I knew subjective paranormal experients frequently reported déjà vu, yet no adequate studies had been developed for this population. Fourthly, I wondered whether or not psychotics could actually be diagnosed on the basis of some of their peculiar interpretations of their experiences of déjà vu, but there were no data in the area.

My attempt to resolve this bafflement led to a four-volume Doctor of Philosophy thesis (Neppe, 1981c), the first academic book on déjà vu, *The Psychology of Déjà Vu: Have I Been Here Before?* (Neppe, 1983h), and the so-called *Déjà vu Trilogy* of three books (2006–2007) in which I revised the *Psychology of Déjà Vu* as *Déjà Vu Revisited* (Neppe, 2006d). I then added an extremely comprehensive update, *Déjà Vu: A Second Look,* with my subeditor, Art Funkhouser (Neppe & Funkhouser, 2006). This book was motivated by my desire to amplify in a chapter what had happened in déjà vu since 1983 when I wrote the *Psychology of Déjà Vu* (Neppe, 1983h), but I wrote so much that effectively it became a whole new book. Finally, because of the vast number of different descriptions of déjà vu, we needed a glossary, particularly as deja is written with accents in French. Consequently, I developed the third book in the trilogy, namely *Déjà Vu Glossary, and Library* (Neppe, 2007).

And so, the major scientific books on the subject are my four déjà vu books: the first scientific book on the subject in 1983 and three more in 2006. Alan Brown (2004) wrote a book that effectively focused on the Neppe subtype of *associative déjà vu*, largely rejecting any other kind and doubting that déjà vu as a subjective paranormal experience could occur (Neppe, 2006l). This limits the strength of this book.

Déjà Experiences

The modern approach initiated by my 1979–1981 thesis work had a historical base: By 1979, there were eleven different kinds of déjà vu experiences, which I termed *déjà experiences* (Neppe, 1981c). Between 1979 and 1981, I subsequently described ten more kinds of déjà experiences (Neppe, 1981c, 1983d). Coincidentally, Art Funkhouser in Switzerland developed two of these terms quite separately—*déjà rêvé* became the already dreamt experience and *déjà visité* referred to already visiting a locality (Funkhouser, 1981). As shown in Table 1, both are very relevant terms because they could

reflect the subjective experience of paranormality, which therefore may imply that they are variants of subjective paranormal experiences (Neppe, 2006c).

Table 1
The 21 Different Kinds of Déjà Vu Experiences (as of 1981)

Developed before 1979	
déjà vu	already seen (traditional global term for all déjà experiences)
déjà entendu	already heard
déjà éprouvé	already experienced [already felt]
déjà fait	already done
déjà pensé	already thought
déjà raconté	already recounted [already told]
déjà senti	already felt, smelled
déjà su	already known (intellectually)
déjà trouvé	already found (met)
déjà vécu	already lived through
déjà voulu	already desired [already wanted]
Developed between 1979 and 1981 by Neppe	
déjà arrivé	already happened
déjà connu	already known (personal knowing)
déjà dit	already said/spoken (content of speech)
déjà goûté	already tasted
déjà lu	already read
déjà parlé	already spoken (act of speech)
déjà pressenti	already "sensed" (as in "knew" it would happen; a presentiment)
déjà rencontré	already met; specifically relates to interpersonal situations
déjà rêvé	already dreamt *
déjà visité	already visited [a locality]*

*Developed independently by Neppe and Funkhouser in 1981

The work on terminology continued and by 2006 I had developed eight more terms (Neppe, 2006e, 2006f), and Funkhouser invented one more in 2009. Thus, currently, there are 30 different déjà experiences described officially. These are not *different kinds* of déjà vu—not different subtypes. Instead, they are *different circumstances* described as déjà experiences.

Ironically enough, reexamining the old literature during 2009, Funkhouser (and to a lesser degree myself) located some unused century-

old déjà experiences. There are two rather unusual terms that we do not use today, which we discovered only in August 2009 in preparation for a presentation to the Parapsychological Association. I am indebted to Dr. Funkhouser in this regard. Eugene Bernard-Leroy (1898) in his doctoral thesis wrote about the *déjà prevu* experience, which is best described as "already foreseen." However, it has apparently never been used other than by Bernard-Leroy, although we have the English word "previewing." It's possibly close to *déjà pressenti*, or already "sensed"—my (1981) term for "already precognized"—as in "knew" it would happen, presentiment. Should we be using both terms, or is there no place for duplicating information?

Also *déjà revécu*, which means "already lived through" or "already relived," was used by Peillaube (1910). Though we haven't used it since then, I think it is a very good term because it could imply the reincarnation sense that a person may be experiencing. Possibly *déjà revécu* is a subgroup of *déjà vécu*, as the latter term means not only that one has already lived through it, but one can fully experience and recollect it entirely. Lalande's (1893) *déjà vécu* could be divided into a relived experience and a recollected-entirely experience. Rather ironically, too, the term *déjà rêvé*, developed as "already dreamt" by both Funkhouser and me in 1981— "I must have dreamt it, and now it's happening"—turned up in our search backward, mentioned by Alfred Fouillee (1885).

Any of the numerous new terms in deja vu must be valuable with significant empirical or theoretical scientific contributions. The older *déjà vu experiences* derive from the French terms. Additionally, we located *déjà articulé* (already articulated) from Lamaître (1908) referring to an article of his of 1905; Vignolli (1894) used *déjà percu* (already perceived); and also Lalande (1893) used *déjà passé* (already passed).

Technically, therefore, we have 35 terms, of which 30 *déjà experiences* are still used. Clearly, there is substantial misuse of the term in common usage (reflecting sometimes repetition of an event but well remembered by all), and the jokes linked with it are for fun, not science, such as *déjà boo*, the feeling that I have been frightened like this before (Mineart & Bell, 2005). These jokes reflect the unacceptable. They serve only one purpose, humor. They are neither parsimonious, the simplest and most logical explanation, nor educational.

Hypothesis Testing

My approach in 1979 reflected the several key questions that needed to be asked leading to hypotheses requiring testing. Consequently, we have the answers at this point. First, are there different déjà subtypes or is there just a single way to adequately explain all déjà vu experiences? In other words, are the déjà vu experiences of those patients reflecting temporal lobe epilepsy the same as those of schizophrenics? Are these the same as in "psychics" and can these not be distinguished from those in the 70% of the population of ordinary people who have these experiences?

Next, could déjà vu be classified as one kind of subjective paranormal experience? We knew that it was a subjective experience by definition, but can part of it, or all of it, be regarded as subjectively paranormal? And several questions always become relevant in these contexts: What is its relationship to reincarnation or to past life memories? What links are there with actualized precognition? And finally, in that regard, can we apply the methodology used for analyzing déjà vu phenomena to other areas of parapsychology?

Causes of Déjà Vu

What causes déjà vu to occur? I realized it was very likely that we were not dealing with a uniform phenomenon, and this has been supported by my latest review of the literature, in which I found exactly 50 different explanations for the déjà vu phenomenon (Neppe 2009). Many causes are very similar and others are idiosyncratic and, although postulated, are unlikely to have a basis in reality (Neppe, 1983g, 1987a, 1987b, 1987c, 1987d, 2006g, 2006h; Neppe & Funkhouser, 2006). Of these 50 reported mechanisms or causes of déjà vu, we could probably more appropriately divide them into eight major categories, as reflected in Table 2 (Neppe, 2006c).

Table 2
A Major Broad Categorization of the Postulated 50 Causes of Déjà Vu

a.	Disorders of memory: restricted paramnesia (partial forgetting), redintegration (part reinstates the whole)
b.	Error in recognition: recognition disorder, not memory
c.	Ego defense: repression of anxiety: "I've been through this before and I came out okay, so I don't need to feel stress."
d.	Ego-state disorder: derealization, depersonalization, twilight state
e.	Psychotic misinterpretation of reality: peculiar, idiosyncratic meaning
f.	Sense of time distortion: temporal perceptual delay
g.	Epileptic firing: abnormal electrical activity within the brain.
h.	Subjective paranormal experience, e.g., precognitive dreams, reincarnation, retrocognition, presentiment, etheric reduplication

Fundamentally, we could divide the proposed causes of déjà vu into *psychological causes*, which would include the *memory* distortions, the *psychodynamic* components (for example, the anxiety defenses), and the *psychotic* elements. We could perceive déjà vu as due to *cerebral* misinterpretations (including *paroxysmal firing*) or to delay across the different *hemispheres* or to *focal* abnormalities (for example, abnormal functioning of a particular area of the brain). Other explanations could invoke the *paranormal* causes, including reincarnation, precognition, and distortions of time. These are well discussed in my more recent book *Déjà vu: A Second Look* (Neppe & Funkhouser, 2006).

I shall now amplify a few of these causes below.

Ostensibly Normal Individuals and Common Explanations

Disorders of memory. Most studied is the vast area of disorders of memory relating mainly to the ostensibly normal person and more recently to patients with paramnesias, such as in Alzheimer's disease (Moulin, Conway, Thompson, James, & Jones, 2005). Memory disorders in déjà vu include three fundamental concepts:

- The *restricted paramnesia*, as described in the classical work of Banister and Zangwill (1941a, 1941b): Essentially this is partial forgetting. One is exposed to certain stimuli, does not remember every detail, then comes into a situation where there is some component of that stimulus. This has been done at an olfactory level as well as at a visual level, and it appears that aspects are familiar.
- Another variation, so-called *redintegration* (not reintegration), where quite literally the "part reinstates the whole," producing a déjà vu impression. This may be commonly combined dynamically: You're anxious about a meeting with your boss, walk into his office and see a little picture there that you've actually seen before. This familiarity then pervades the whole place (Neppe, 1983b, 2006k, 2006l, 2006m).
- Recognition is the third "memory" component, the one focused on in modern research. This can be tested by how people recognize certain facets, but not the whole, something like a picture. Are there components pertaining to more recognizable information (Neppe, 1983h; Reed, 1979)? This to some degree reflects the modern researcher Alan Brown's (2003) approach. But these are approaches in ostensibly normal individuals. Memory has components pertaining to registration, recall, recognition, and retention. And on the recognition side (Thompson, Moulin, Conway, & Jones, 2004), errors may be produced resulting in déjà vu in normal individuals, and some work is now being done in Britain on persons with Alzheimer's disease (Moulin et al., 2005).

However, the descriptions are incomplete phenomenologically, reflecting an area where disorders of memory are highly relevant but may not fit the classical definition of déjà vu.

Distortions of interpretation. Several theories explain déjà vu by distortions of interpretation. These pertain to ego-state disorders such as derealization and depersonalization, and they also include the so-called twilight state of impairments of consciousness (Siomopoulos, 1972). These are predominantly linked up with distortions and the same kind of theoretical framework that one sometimes sees in out-of-body experiences (Neppe, 2002, 2009).

Ego defense interpretations. Ego defenses are also used to explain déjà vu: "I've been through this before; it's all right, I don't need to feel stress"; effectively: "I have a sense of relief because of my déjà vu experiences." So one represses the bad side, the anxiety (Boesky, 1973; Neppe, 1983b).

Of course, memory disturbances, mistaken interpretations, and ego defenses can all occur in combinations in the ordinary, ostensibly normal individual, and they are linked with what I call "associative déjà vu," where associations induce the déjà vu.

Seizure Disorders

Epileptic firing, classically in temporal lobe epilepsy, consequent to abnormal electrical activity in the brain produces an experience which evokes familiarity because the same firing was occurring before. In fact, the experience is familiar because the same pattern is being re-evoked in the brain as part of the stereotypical seizure, but there is a strange sense that it cannot be so (Neppe, 1981d, 1982, 1986).

Psychotic Disorders (Including Schizophrenia)

The third group we must consider are those who have psychotic misinterpretations of reality. This occurs in individuals with psychotic conditions such as schizophrenia. These patients exhibit special features in their déjà vu experiences, including peculiar, idiosyncratic meanings. Their interpretations are very often self-referential, where they are totally misinterpreting information and directing it to involve themselves. However, it is fascinating that one cannot elicit psychotic thinking until one starts asking about déjà vu. Yet, one finds there is then such illogicality in the connections of their thought associations that it becomes more obvious (Neppe, 1981d, 2006g).

Parapsychological Bases (Time Distortions)

The sense of time distortion is an important one, particularly in the parapsychological sense because there is the delay component of something

happening but at a later or earlier point, and picking this out creates an inappropriate familiarity sense.

In the subjective paranormal experient, this is intense: They regard themselves as aware of the present, the past, or the future. This is different from Wigan's (1844) initial hemispheric explanation implying a momentary temporal perceptual delay.

There are variants of explanations: The real subjective paranormal experience, the precognitive dreams, reincarnation, retrocognition, and also presentiments of immediate precognition seconds later versus delayed precognition, which may be minutes, hours, days, weeks, months, or years later, and I posit that there possibly is a different mechanism than presentiment (Adachi et al., 2003; Kohn, 1983; Neppe, 1983a, 1983b, 1983d, 2006b, 2006g). Table 3 reflects the seven different kinds of déjà experiences that are parapsychologically relevant.

Table 3
Different Parapsychologically Relevant Déjà Experiences With Year of Development

déjà pressenti	already "sensed"—as in "knew" it would happen; presentiment (Neppe, 1981c),
déjà retrosenti	already sensed the past (Neppe, 2006e)
déjà preconnaître	already precognized (Neppe, 2009)
déjà prevu	already foreseen (Leroy, 1898) — not used
déjà rêvé	already dreamt (Fouillee, 1885; Funkhouser, 1981; Neppe, 1981c)
déjà vécu	already lived through, fully experienced/recollected in its entirety (Lalande, 1893)
déjà revécu	already lived through (Peillaube, 1910, p. 513)—not used

The Landmark Differentiation

The year 1979 turned out to be a landmark because my own research began at that time, and with respect, with that came what can be perceived as the modern shift of déjà vu classification.

My key question then was: Is déjà vu a single phenomenon or phenomenologically distinct in several populations and if so, in what way? I needed to develop a measuring instrument, and I developed the Neppe Déjà Vu Questionnaire, with which I would analyze déjà vu in detail phenomenologically (Neppe, 1981c, 1983d). There were several components: The Déjà Vu Screening Questionnaire screens for the

many déjà experiences in a broad readership; then the Déjà Vu Detailed Questionnaire is used to elicit qualitative differences in possible subtypes; it is administered individually to analyze phenomenological specifics in different populations. This was combined with detailed interviews where specific examples of déjà vu were required.

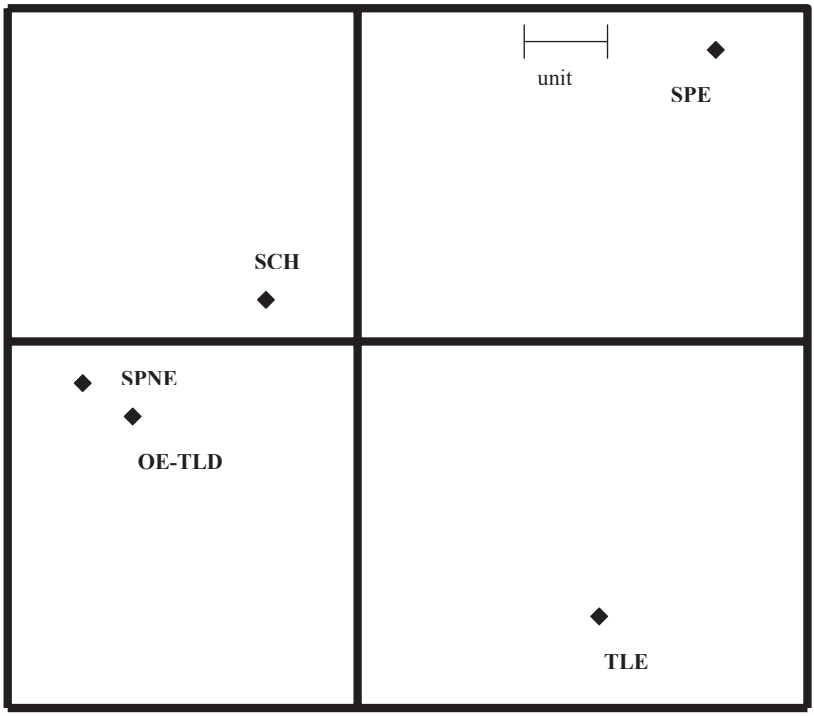
I hypothesized that there are four phenomenologically distinct nosological subtypes, and I needed to use comparative populations. I used two distinct populations:

- A *neuropsychiatric population* of *temporal lobe epileptics* compared with schizophrenics. The schizophrenics reflected the psychosis. The temporal lobe epileptics reflected a subtype of all epileptics that I thought would be specific, so I also included other nontemporal lobe epileptics as well as those who were not epileptic but had temporal lobe dysfunction. I hypothesized that the experiences of these nontemporal lobe epileptics would appear rather like the "normal" kind of déjà vu, as there would be no firing specifically in the area of the brain that would cause them to experience this déjà vu awareness that it had happened before. It was very important to differentiate this, because otherwise one would ask: if a person has a seizure and has a particular aura, but the aura is frontal lobe, for instance, could the person be experiencing the same aura and thinking that it is déjà vu? Would it be that this subgroup of epileptics would know it was not déjà vu because they would experience the appropriateness of the experience, and perceive it as logically different?
- The second distinct population was "ostensible normals" who had never had any psychic experiences that they interpreted as such, whom I called *subjective paranormal nonexperients*. They were compared with people who regarded themselves as "psychics," that is, those who reported subjective paranormal experiences based on specific, detailed criteria for subjective validity and specificity. The question was, did they have a distinct kind of déjà vu?

I studied the 21 then-known kinds of déjà vu experiences (circumstances) including the nine more I had described, and I subdivided them into numerous phenomenological descriptions with several items per set to ensure homogeneity of responses (Neppe, 1981c, 1983d). I was able to establish the 22 phenomenological descriptions and assign these data to the 22 different dimensions of theoretical representative space. I then applied these hypotheses by analyzing the data using multidimensional scaling and 22 different dimensions using median column geometry. I was greatly assisted by a remarkable professor of statistics, Dan Bradu (Neppe & Bradu, 2006). I was lucky enough that our data ultimately represented the defined populations in four different quadrants. With all four quadrants

represented, we were able to demonstrate that there was an existence of the four nosological subtypes, and at least four different types of déjà vu exist, as demonstrated in my book, *The Psychology of Déjà Vu* (Neppe, 1983h).

Moreover, this was predictable across diagnostic categories and we could classify these different symptom categories as qualitatively different amongst the four. I called the four: subjective paranormal déjà vu, temporal lobe epileptic déjà vu, schizophrenic (later called psychotic) déjà vu, and associative déjà vu (Neppe, 1983d, 1983h).



Coordinates

	x-axis	y-axis
Subjective paranormal nonexperients (SPNE)	-3.164	-0.320
Subjective paranormal experients (SPE)	4.076	3.443
Temporal lobe epileptics (TLE)	2.717	-3.050
Schizophrenics (SCH)	-1.013	0.601
Other epileptics/temporal-lobe disfunctions (OE-TLD)	2.615	-0.674

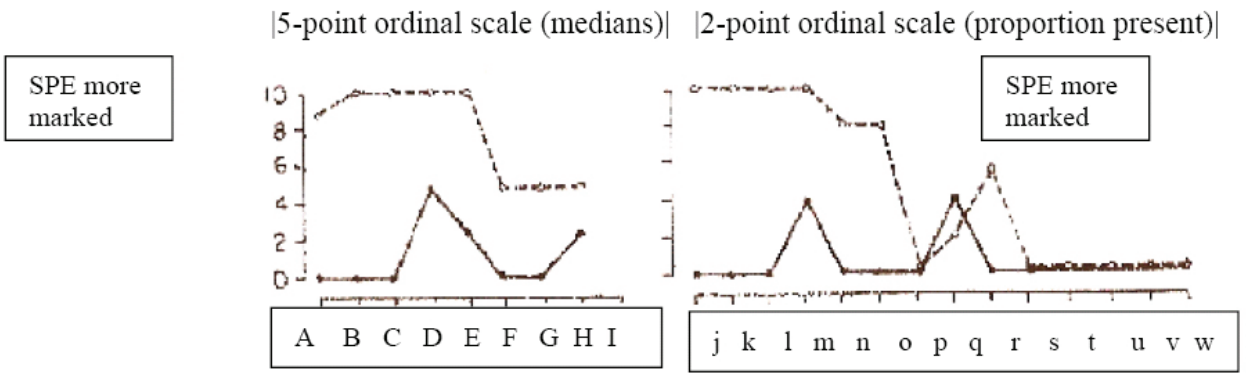
Figure 1. Graph representing the differences between the five groups based on the five-point qualitative parameters of déjà vu. (Distance between two column points approximates the Euclidean distance between the two columns as vectors in R^{22})

Figure 1 shows multidimensional scaling, with the graph showing median column geometry representing the differences between the four quadrants. This has five groups because the nontemporal lobe epileptics and the nonepileptic temporal lobe dysfunctions were studied as a separate group, and as hypothesized, this population fitted into the subjective paranormal nonexperience group. Their results were very close and this itself was very useful because it shows the linkup of the neuropsychiatric with the so-called "normal" subpopulation in this regard, implying a certain unified population. The graph represents differences between the five groups based on the five-point qualitative parameters of déjà vu. The distance between two column points approximates the Euclidean distance between the two columns as vectors in R^{22} (Neppe & Bradu, 2006).

Experts looking at this graph would argue that *psychotic déjà vu* is not too different distance-wise in R^{22} from the subjective paranormal nonexperience one. But we not only have to examine the major distance between the two, we must also keep in mind the fact that there were only a few phenomenological components that were different. In other words, psychotics were having associative déjà vu with nothing being profound, but their distinct feature and problem was that what they were adding to this was a consistent misinterpretation of reality and referential phenomena (Neppe & Bradu, 2006).

This is well reflected in the analysis of Figure 2, showing the specific dimensional features in the five subpopulations. Therefore, in the multidimensional matrix, we have the representations in the four different quadrants and we can demonstrate four aetiologically distinct kinds of déjà vu experience occurring in four different populations, as reflected in Figure 1. Moreover, when we look at this more closely, we find that there is sufficient distinctiveness to classify an individual déjà experience description as in Figure 2. Obviously, there are individual subjects that may overlap in a déjà vu subtype, so we can have a psychotic patient with temporal lobe epilepsy. And individuals may belong to more than one group: for example, a temporal lobe epileptic patient and an SP experient can theoretically overlap, although I've never seen it. Associative déjà vu can obviously occur in all groups; just because some people have temporal lobe epilepsy doesn't mean that they cannot have associative déjà vu. And when this occurs in the psychotic patient, it could post hoc "tinge" the description psychotically.

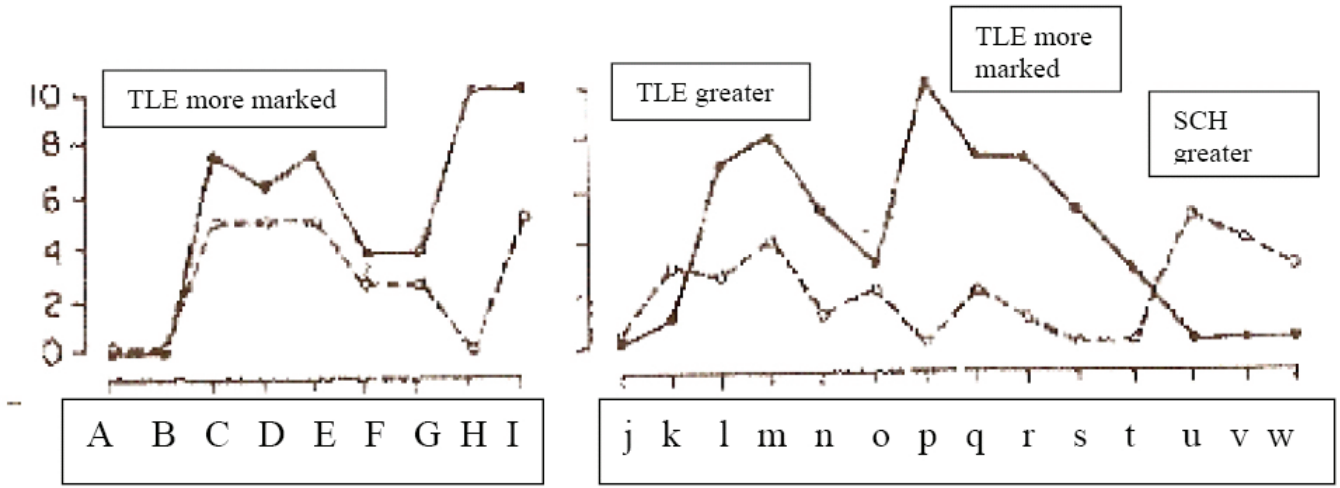
To these four, we possibly can add a fifth: I mentioned the Alzheimer study in Britain of demented patients. However, there are two problems: First, the descriptions do not necessarily fit the definition of déjà vu; and second, if they do, the variant of a full phenomenologically different subtype of déjà vu is unproven (Moulin et al., 2005). However, the descriptions are phenomenologically incomplete, and I cannot even definitely regard this as another subtype.



SPNE = Subjective paranormal nonexperients; SPE = Subjective paranormal experients

Figure 2A. "Normals" category difference in déjà vu between subjective paranormal experients and nonexperients

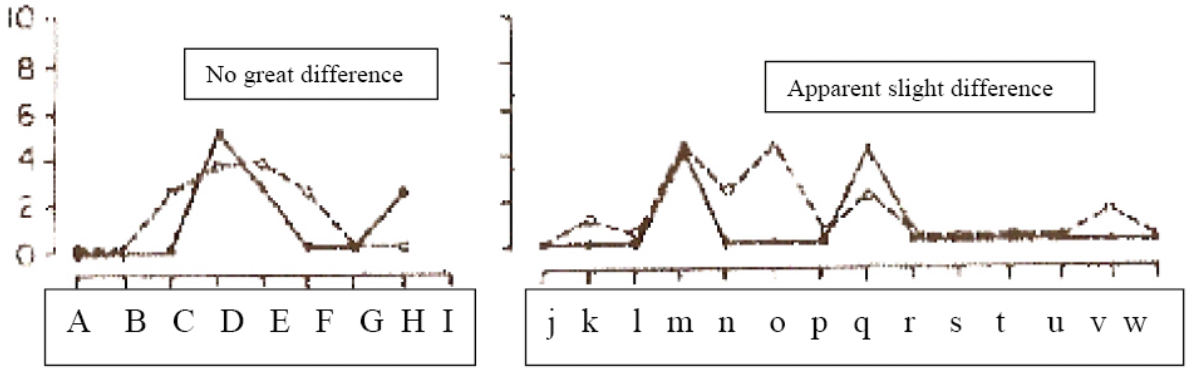
A = More than one perceptual modality of déjà vu	j = Acceptable SPE
B = SPE time-distortion	k = Claimed SPE
C = Clarity of déjà vu	l = Marked cognitive change
D = Clarity of chosen case	m = Marked environmental awareness
E = Degree of familiarity	n = Experiential growth
F = Emotional intensity	o = "Sensing" familiarity
G = Strongest point	p = Post-ictal features
H = Frequency of déjà vu	q = Special circumstances
I = Illness relating to déjà vu	r = Marked body awareness
	s = Déjà aura to seizure
	t = Jamais vu aura
	u = Psychotic special meanings
	v = Psychotic self reference
	w = Psychotic SPE



TLE = Temporal lobe epileptics; SCH = Schizophrenics

Figure 2B. "Neuropsychiatric" category difference in déjà vu between temporal lobe epileptics and schizophrenics

A = More than one perceptual modality of déjà vu	j = Acceptable SPE
B = SPE time-distortion	k = Claimed SPE
C = Clarity of déjà vu	l = Marked cognitive change
D = Clarity of chosen case	m = Marked environmental awareness
E = Degree of familiarity	n = Experiential growth
F = Emotional intensity	o = "Sensing" familiarity
G = Strongest point	p = Post-ictal features
H = Frequency of déjà vu	q = Special circumstances
I = Illness relating to déjà vu	r = Marked body awareness
	s = Déjà aura to seizure
	t = Jamais vu aura
	u = Psychotic special meanings
	v = Psychotic self reference
	w = Psychotic SPE



SPNE = Subjective paranormal nonexperients; OE-TLD = Other epileptics-temporal lobe dysfunction

Figure 2C. Illustration of similar "associative déjà vu" of the subjective paranormal nonexperients and the nonepileptic temporal lobe dysfunction – other epileptic group.

A = More than one perceptual modality of déjà vu	j = Acceptable SPE
B = SPE time-distortion	k = Claimed SPE
C = Clarity of déjà vu	l = Marked cognitive change
D = Clarity of chosen case	m = Marked environmental awareness
E = Degree of familiarity	n = Experiential growth
F = Emotional intensity	o = "Sensing" familiarity
G = Strongest point	p = Post-ictal features
H = Frequency of déjà vu	q = Special circumstances
I = Illness relating to déjà vu	r = Marked body awareness
	s = Déjà aura to seizure
	t = Jamais vu aura
	u = Psychotic special meanings
	v = Psychotic self reference
	w = Psychotic SPE

Different Types of Déjà Vu

Associative Déjà Vu

Let's look at some examples. First, associative déjà vu is so called because it is associated with this vague sensation of déjà vu that happens to ordinary people, maybe 2/3 or 70% of the population if screened well for déjà experiences (Neppe, 1983e; Neppe, 2006d). It occurs only infrequently in any individual, maybe a few times in a lifetime, it lasts only a few seconds, and the experient is left with an impression of perplexity:

> Why did this happen? Was it something in my past?

They attempt to rationalize the experience, and it's at that point that we find possible distortions of memory, of remembering, or of recognition occurring. The scientist will analyze and ask: "Did the part reinstate the whole?" implying redintegration; or "Was this something that was only partially forgotten?" implying redintegration. And, of course, there is a psychological release, as reflected by the following participant, a psychiatrist, a smoker who always had guilt about his smoking and had an unconscious sense of conflict:

> The one [experience] I'm describing happened a year ago. I went into a little corner café to buy cigarettes. I had never been to that particular shop before nor had I ever seen the shopkeeper before. As I was buying it, I felt the shopkeeper and the whole situation were familiar and I had gone through this experience before. This often happens when I buy cigarettes and has occurred in several small cafés.

In this example, an inappropriate feeling of familiarity is evoked by a present situation. Yet, the situation should be familiar as he had frequently been in very similar situations. The déjà experience is repetitively evoked by the same situation that involved doing the same thing, possibly reflecting his ambivalence about smoking and his relief at the familiar impression of "I've done it before; it's okay" (Neppe, 1983h).

This kind of repetitive déjà experience relating to a very specific precipitator is highly unusual in associative déjà vu individuals. Otherwise his experience is typical, as such subjects describe no great change in emotion, do not have the postictal features we see in temporal lobe epileptic déjà vu (there is no headache, sleepiness, confusion, or consistent stereotypical related symptoms). There is no illogicality, as we see in the psychotic déjà vu, and there is no sense of distortion of the present with the past as we see in subjective paranormal experience déjà vu.

Temporal Lobe Epileptic Déjà Vu

Let's look at temporal lobe epileptic déjà vu. Here's an example:

> …. up to nine [experiences] per day for days on end. They always take the same form but the actual details will depend on where I am. While I am having the experience, it is as if I have been there before talking—this feeling of familiarity. The whole room, what the client asked me, that too was very familiar. At the same time, I got the impression of a small river in the house. She said my whole face was quite white. I continued the conversation as if nothing had happened but meanwhile everything … (sometimes it's a river, sometimes it might be chickens). Afterwards, I had a slight headache, and felt tired but not sleepy. This time I was not confused (I sometimes am), and I did not get a rotten egg smell, which I sometimes get.

In this instance, the patient has features after the episode suggesting some kind of seizure. These are called postictal features: sleepiness, confusion, headache, sometimes nausea. The déjà vu may precede frank blackouts, or seizures. Experiencers have associated epileptic features where they may be having some other kind of complex partial seizure phenomena with impaired consciousness, or simple partial episodes such as burning smells, for example; or they might have distortions in their vision, or become very irritable; and these, in turn, may lead to full blown convulsive seizures (Neppe, 1982, 1983d, 1985b, 1986; Neppe & Funkhouser, 2006).

In temporal lobe epileptic déjà vu, the déjà vu experience is repetitively exactly the same. This itself is a kind of paradox because, of course, they've had the same exact experience before—it's a stereotypical march—but they perceive the happening as inappropriate at this time. Yet they know what is happening because they know the whole sequence, and depending on their level of clouding of consciousness (meaning here complex partial seizures), they may or may not be able to describe the full events. There may also be an awareness of one's own body or of oneself. Sometimes there are significant changes of their mood—they may become dysphoric or labile—or they notice a definite change in their thinking. These cognitive-affective changes may last seconds to hours, depending on the individuals.

They will describe these events frequently and the occurrence strongly correlates with seizure control as these déjà vu experiences are actually seizures themselves.

Psychotic Déjà Vu

What about psychotic déjà vu? In this instance, the subject interprets and misinterprets his whole world in terms of a special meaning, as in this example:

> Once I saw photos of Israel where Jesus was born. It showed the crib, and the star. I felt very significant feelings. It did something for my mind. I had a warm feeling. I felt I was near home. I felt I had been there before a long time ago—centuries ago—at the time of Christ. Sometimes I feel I'm an eternal spirit, Socrates, Churchill.

Now here is the difference: They have special meanings, usually related to themselves; they understand certain components, but it is bizarre and often idiosyncratic. The self-referential quality invariably is linked with a vague knowledge and a sense of thought disorder and with it may be some frank psychotic features.

I have pointed out how such happenings are not too distant in the 22-dimensional framework from the associative déjà vu, but the differences are profound in terms of the specific analysis of certain subtypes being regularly different (Neppe, 1983h; Neppe & Funkhouser, 2006).

Subjective Paranormal Experience Déjà Vu

One of the most profound kinds of experiences is subjective paranormal experience déjà vu. It is exciting to listen to and remarkable to hear about its varied presentations. Here is an example of *déjà retrosenti*:

> I came to Johannesburg for the first time about six years ago. I had never been there before. I found I just knew how to get to places. I had an impression of knowing the place in detail, as if I had been there before. The experience is ongoing. I still just know my way around. Even today I don't use maps. The familiar section from the very first time is the older section. I can't find my way around the new suburbs; buildings or roads built recently are unfamiliar. When I go past, I may say, "What happened to that building?" I will know that there was something else there before: I will feel a little sad that it has come down. Sometimes I can state which building it was. At times I am aware that certain buildings have been pulled down … I just have a "knowledge" of certain areas that are very familiar…. Time plays no role; I cannot distinguish the past, present or future.

Here the person has this specific strong knowledge; he knows, doesn't speculate. He moves backwards in time with facility. He had profound recognition and awareness. This is typical of the subjective paranormal experience quality of their déjà vu. There is the movement forward and backward, suggesting subjective precognitive and/or retrocognitive events. There is a "very real" familiarity impression, a peak of experience not only at the onset. It may be a growth of their experiences. It is a polymodal perceptual déjà vu experience—visual, auditory, sometimes smell, taste, and this "knowing." There is an intense awareness of environment; it is clearly being remembered. They are reliving the whole impression, with the time distortion being either backward (*déjà retrosenti*) or forward (*déjà pressenti*).

Our most recent new déjà vu circumstance is *déjà preconnaître*—already precognized. Here the persons have a precognition—an awareness of what is going to be—and yet, at a later point in time, they have this same feeling of precognition again and they do not remember exactly when they had that original precognition. It's just the sense, "I'm having this and I've had it before," yet they cannot explain it. It correlates with a very important dilemma in parapsychology, namely, is subjective paranormal experience déjà vu provoking precognition, or is it actually precognitive experiences that have actualized themselves? But here it's not just "I must have dreamt it before," but in *déjà preconnaître* it is, "I'm sure I've had this premonition before but I know not where or when" (Neppe, 1983h, 2006i; Neppe & Bradu, 2006).

Subjective Paranormal Experiences and Déjà Vu

These déjà experiences hypothetically manifest more frequently in the subjective paranormal experience déjà vu subtype. Subjective paranormal experience déjà vu involves "time distortions," specific subjective paranormal awarenesses, a profound intensity, and a specific predictive "knowledge" with nonpsychotic and nonictal qualities. In other words, subjective paranormal experience déjà vu does not have features of seizure phenomena, and there is not the vagueness or delusional component, or the self-reference components, or the self-referential passivity occurring to the experient that we see in psychosis. Instead, the awareness is specific, it involves a prediction of some kind, and invariably there is a profound intensity.

Subjective paranormal experience déjà vu facilitates the key answers to the questions we asked about its specific niche: Of the (at least) four phenomenologically different déjà subtypes required to explain déjà vu, one subtype is subjective paranormal experience déjà vu. Because of its close relationship to subjective paranormal experiences, and also to subjective paranormal experients, this subtype specifically therefore becomes one kind of subjective paranormal experience, as much of a subjective paranormal experience as an out-of-body experience, or a near

death experience, or possibly even ESP. Subjective paranormal experience déjà vu is not most parsimoniously explained by reincarnation, but with some kind of movement backwards in time, retrocognition. Reincarnation is phenomenologically more detailed (Neppe, 1983d, 2006i; Neppe & Bradu, 2006).

Subjective paranormal experience déjà vu, with its predictive elements, has an undefined past; an actualized precognition results from a defined event that happened beforehand, so it becomes a demonstrable actualization of an event.

In déjà vu, there may commonly be elements of retrocognition and precognition in the same component: One knows what will happen next behind the door and yet one is able to know what had happened at some different moment in the past.

This research has major implications. It reflects the fact that detailed phenomenological analysis is necessary, that we need to develop screening questionnaires, and detailed questionnaires with individual interviews, in order to differentiate out differences. The associative déjà vu subject is the so-called normal with the vague perplexity. Temporal lobe epilepsy déjà vu has the temporal lobe phenomena, and possible temporal lobe symptoms with ictal and postictal features. The psychotic déjà vu has psychotic distortions and loosening. Subjective paranormal experience déjà vu has anomalous distortions of time and place. Each occurs phenomenologically in distinct groups: subjective paranormal experients, psychotics, so-called "normals," and temporal lobe epileptics. The subjective paranormal experience déjà vu usually involves distortions of time, this specific paranormal awareness. And there is a profound intensity. These experiences occur frequently, but not always, because some have this only occasionally, and it is correlated with other subjective paranormal experiences. These persons have a specific predictive knowledge; they know exactly as opposed to the vagueness that we see in schizophrenics, for example. Here's one reflecting the really mystical *déjà pressenti*:

> About a year ago, I innocently picked up a book. Even though I had been sleepy at the time, I suddenly felt very excited. This experience made me alert, tingling and vibrant—like a door opening, affirming things that I never knew existed, of a whole everything. Earth was too small for this comfort, wisdom and elation. I had no ego. I knew what I was reading, even though I had never read it or come across it before, and I knew what I was going to read further on. The ideas were very familiar—wow!—like opening a fridge and smelling and tasting the leg of lamb inside before you even see it. But that's much too mundane, it was not like that at all. It was almost a litany or a prayer; it was part of me. This knowledge of concepts

> was extremely familiar, but I didn't know it intellectually. It came to me intuitively. Even now when I pick up the book, it is so reassuring. I felt like the wise old woman of the universe. (Neppe, 1983h, p.120)

We move forward now to historical exploration.

Lessons From the Neppe Research

What lessons have we learned? One of the methods that we have learned is to differentiate the different subtypes of déjà vu. This is very important because we have at least four subtypes. This implies four distinct aetiologies, an extremely relevant concept, particularly as they occur in four different subpopulations and empirically validate what was theorized.

Continued Analysis

But we can continue such analysis. Of course, the development of the original Neppe Déjà Vu Questionnaire of 1981 was certainly a relevant milestone; however, the added knowledge requires a revision. This occurred in 2006, with the development of the New Neppe Déjà Vu Questionnaire (Neppe, 2006e). This has not yet been empirically validated, but the possibility of using questionnaires such as these over the Internet becomes a cogent one, as provisionally done by Funkhouser (http://silenroc.com/dejavu and www.deja-experience-research.org).

Additionally, we need to develop supporting instruments. For example, does subjective paranormal experience déjà vu correlate with other kinds of subjective paranormal experience? And if so, a natural and easy study would be looking at correlations, which requires skill at developing other kinds of questionnaires; for example, questionnaires screening for other subjective paranormal experiences, as reflected by my work, NEASTS (Neppe Experiences of Anomalous Subjective Type Screen) with its subcategories SEASTS (Screening Experiences of Anomalous Subjective Type Screen), BEASTS (Brief Experiences of Anomalous Subjective Type Screen), and DEASTS (Detailed Experiences of Anomalous Subjective Type Screen). And this, therefore, implies the need for detailed phenomenological analysis to differentiate the key subtype features. Similarly, questionnaires to screen temporal lobe disease and epilepsy need updating. This too, has happened: Originally in 1977 I developed the Neppe Temporal Lobe Questionnaire (Neppe, Ellegala, & Baker, 1991). Later, circa 1987, I revised it into its current form, the INSET (Inventory of Neppe of Symptoms of Epilepsy and the Temporal Lobe; Neppe et al., 1991). We have experience with the INSET in an estimated 1,000 patients, though a fertile area for students, graduates, PhD candidates or junior faculty is to analyze the data from the INSET, much of which is easily available.

Revisiting Our Definition

We not only can, but should, apply methodology used for analyzing déjà vu phenomena and use it in other areas of parapsychology and also in neuroscience. Consequently, the definition of déjà vu becomes more cogent—any subjectively inappropriate impression of familiarity of the present experience with an undefined past. Every one of these terms is important. If it was a defined past, it could be an actualization of a precognition. If it was appropriate in terms of the impression of familiarity, this is part of recognition; familiarity itself becomes a cognitive level as part of the learning process, and the obvious subjectivity relates to events happening to individuals at their level, but with no one else experiencing it or validating it. Has this definition of déjà vu withstood a quarter of a century of research? Yes it has: Almost every serious paper has cited this definition, and it appears consistent, reliable, and measurable.

If indeed there is a specific cognitive level for appreciating inappropriate familiarity and consequently experiencing déjà vu, when does that come about? At what age can children report this? Five years old may be the cutoff; certainly at this point this is the lowest reported age:

> I was only five years old. I can assess this because that was when we went on a holiday including Lake Tanganyika. Maybe that was the reason it stuck in my mind—because l was quite small. We went on a little launch on the lake. The adults were trying to catch crocodiles. I felt great excitement, and was also a little afraid. My child mind worried that the crocodiles would turn over the launch. We went only to a little island actually in the lake. You can imagine how small it was. As I walked on it, it looked familiar. I thought I had been there before. The whole scene seemed familiar, no specific features. I had never been on an island like this. The feeling was quite ridiculous, because there probably weren't even any houses. (Neppe, 1983h, pp. 70–71)

This is an example of *déjà vu visité* and it is important because it reflects a critical cognitive milestone for a person able to experience déjà vu.

Let's revisit some of the eight newest déjà vu experiences (Table 3; Neppe 2006e, 2006f), adding the ninth (*déjà preconnaître*—already precognized) to Table 3. I use four as illustrations. This allows appreciation of the sheer complexity of the concept, and it allows another source, other than the book *Déjà Vu: A Second Look*, to record such detail (Neppe & Funkhouser, 2006).

Table 4
The Nine Newest Examples of Déjà Vu

déjà paradoxe	already paradoxical
déjà après	already after (postictal)
déjà ésotérique	already esoteric
déjà rétrosenti	already sensed (reanimated past)
déjà halluciné	already hallucinated
déjà touché	already touched (physically)
déjà mange	already eaten
déjà senti	already smelled; rediscovered Gilles (1921)
déjà preconnaître	already precognized

Let us examine four examples of these:

Déjà Esotérique

This type includes the classic schizophrenic special meanings, self-referential ideas, and delusional misinterpretations, and a dull vagueness, added to a routine initial déjà vu (Neppe, 2006j, Neppe & Funkhouser, 2006):

> There was this guy ... When I was young, I thought about him. I thought I would see him one day. When I saw him, I knew I had seen him before ... I thought in my mind I would meet him. When I met this man, I thought folding a newspaper in half would be one of his codes to me to go through life. I realized this when he actually did fold his newspaper.… The code means I'm on his side. He was giving me a message: "Go get a shotgun." He didn't speak. This meant he didn't want anyone to know how his voice sounds—as if he is a CIA member. I know he is a member of the CIA, because if I joined the CIA I would meet guys like him (Neppe, 2003h, pp. 159-162).

And Now, Three Linked With Temporal Lobe Déjà Vu

Déjà paradoxe. This illustrates the paradox of having, in fact, experienced something before, and yet repetitively re-experiencing its inappropriate familiarity. The complexity is typical in this instance: the profound familiarity sense linked with the specific setting at that moment, and the illogical fear of a fear sensation, so typical of some complex partial seizures (Neppe, 2006j; Neppe & Funkhouser, 2006).

> Suddenly, lasting a flash of a second the whole place became familiar: the walls, the curtains, the receptionist, the counter, the ceiling. This experience was identical in quality to previous experiences I'd had, and I knew I would have a blackout.… Along with the feeling started an experience of intense, unexplained, unprecipitated fear, which lasted about thirty seconds. … During that phase everything was unfamiliar again, and I developed the intense uncontrollable desire to go away. Then I blacked out (Neppe, 2003h, p. 138-139).

Déjà après. This is clearly an example of temporal lobe epileptic déjà vu, including déjà vu with an aura, the stereotypy, the inappropriate familiarity, the other specific consistent symptoms, and the seizure itself (Neppe, 2006j; Neppe & Funkhouser, 2006):

> I was in the kitchen. Suddenly, I had a feeling of discomfort (like wanting to pass a stool). Then came a feeling of lightness. It was more a sensation—light and bubbly. I can hardly describe it. (It's so difficult: It's the same sort of feeling every time, but I don't always have an attack.) … I became aware of a sensation that wasn't normal for me: I know it's happened before and yet I don't know where. This was followed by a blackout, and after that I had a fullness in my bladder and I wanted to pass water … sensing of the whole situation of my body … there is a sense of sameness, the same sort of thing, but it's not a recognition of the fit—I was more aware of the whole kitchen, everything. I don't think of a coming fit … sometimes I get this sensation on its own—by itself—like when I meet people (Neppe, 2003h, p. 138).

Déjà halluciné. This is a truly remarkable example of a rare event—déjà vu of a hallucination, again showing a different manifestation for temporal lobe epileptic déjà vu, with the clue of loss of consciousness or complete amnesia, at the end (Neppe, 2006c):

> It happened this morning. I was lying in bed. Suddenly there were these two large white gates in front of me. They actually existed—I saw them but in reality there are no gates in the room. This occurred in a flash. I recognized the gates. They were very familiar. I felt I had gone through it all before. I don't know what happened next. Maybe I had a blackout or a fit (Neppe, 2003h, p. 144).

Temporal lobe epileptic déjà vu has these intense differences because the kind of seizures may be different and the postictal experiences are different, as opposed to the associative déjà vu with the so-called "normals": the very perplexity of the psychotic déjà vu with the psychotic distortions and the loosening of thinking and subjective paranormal experience déjà vu, where there are the anomalous specific distortions of time and place and subjective paranormal experience.

The Phenomenological Approach

We can also apply the lessons learned from déjà vu phenomenological research and generalize it to parapsychological work and neuroscience. The most important principle is that we want to appreciate that in phenomenology, like must be paired with like, and nonlike must be categorized with a variety of nonlike phenomena and then reanalyzed for correlates and differences. In other words, we need to ensure a consistency in terms of our interpretation. This may be the most important lesson of the Neppe déjà vu research, applying phenomenological research and emphasizing the development of the phenomenological school in both parapsychology and the neurosciences. In the context of all possible paranormal experiences, whether subjective or objective, in the empirically based research environment, we should analyze information phenomenologically.

We can easily generalize this methodology to out-of-body experiences and to the olfactory hallucinatory experience. In fact, I have examined olfactory hallucinatory experiences, comparing the consistency of those in temporal lobe disease with those in subjective paranormal experients, and evaluating the overlap. We have also applied this method to our temporal lobe symptomatology research, where the temporal lobe appears to be the source from which subjective paranormal experiences are either derived or are modulated through the brain. One can, moreover, demonstrate this at two different levels: Subjective paranormal experients are highly functioning but have significant possible temporal lobe symptoms; by contrast, temporal lobe epileptics have significantly more subjective paranormal experiences (Neppe, 1983a; Palmer & Neppe, 2003, 2004). We can also apply the lessons learned from déjà vu phenomenological research by examining the correlates of such psychological phenomena as the experimenter effect or the personality effect on psi. We can also apply these to subjective paranormal case analyses, both prospective and retrospective. We can move further to examine how the subjective links up with objective paranormal experiences, as in the experimental research paradigm.

The lessons, therefore, are not restricted to the subjective paranormal experiences in the Neppe work and their application to temporal lobe symptomatology, to olfactory hallucinations, and to the

subjective paranormal olfactory hallucination work (Neppe 1983a, 1985a), as well as the application of phenomenological analyses to any subjective experiences (Neppe, 2009). Just as we apply hallucinations and delusions in analyzing psychiatric diseases such as the psychoses, we can phenomenologically compare this with nonpsychotic experiences, because hallucinations might even have subjective paranormal experience components (Neppe, 1983e).

Moreover, prospective paradigms may allow for a more detailed data set if we apply the lessons that have been learned and prospectively do detailed analyses. We must then apply phenomenological details showing a methodology that allows for data recordings that are standardized and relevant that can be applied for all time. I have suggested the A–Z access classification of such experiences, the so-called SEATTLE, using subtypes of these 26 axes (Neppe, 2009). These can be directed in many ways: For example, in precognizing events, we may use subclassifications as necessary—I have used a classification I call the TICKLES system combined with various metaphorical kinds of systems, what we call the MOLDINGS components (Neppe, 2009).

This déjà vu research is a contribution to all phenomenology because it demonstrates that we should use detailed evaluations of psychological experiences, thereby permitting deeper understandings of the similarities and differences of subjective realities. Like is paired with like, nonlikes are not paired because there are different kinds of nonlikes. Two "likes" might not be completely identical, but they may be identical in certain features, and we have to differentiate those features from other ones. We can apply this to pathological hallucinations and delusions, as well as, possibly, paranormal hallucinations. We can apply it to subjective psychopathology discomfort, to subjective anomalous experiences, or subjective psi experiences, and we can apply it to symptoms of higher brain functioning, including the frontal lobes and also the temporal lobes of the brain. We can even use it as a model for medical history taking. These lessons are very important, and the broader lesson is the motivation of detailed documentation and screening for events. This is equally applicable to parapsychological research and to neuroscience research.

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Acknowledgment

Thank you to Maria Kennihan for her assistance with the preparation of this article and the figures.

Abstracts in Other Languages

French

DEJA-VU: ORIGINES ET PHENOMENOLOGIE : IMPLICATIONS DE QUATRE SOUS-TYPES POUR LA RECHERCHE FUTURE

RESUME : Une analyse des sous-types de déjà-vu est réalisée en suivant la définition opérationnelle universellement acceptée du déjà-vu de Neppas (toute impression subjective inappropriée de familiarité d'un vécu actuel avec un passé indéfini), les 30 circonstances différentes d'« expérience de déjà » et les explications postulées pour le déjà-vu. Neppe a fait l'hypothèse et démontrait quatre sous-types nosologiques et phénoménologiques distincts, représentant 4 populations distinctes motivant 4 sortes de déjà-vu étiologiquement distinctes : l'expérience paranormale subjective (SPE) de déjà-vu (chez ceux qui vivent des SPE), le déjà-vu associatif (chez ceux qui n'ont pas de SPE et sont apparemment « normaux », mais aussi lors d'une dysfonction non-épileptique du lobe temporal et chez les patients ayant une épilepsie non associée au lobe temporal), le déjà-vu psychotique (chez les schizophrènes) et le déjà-vu chez les patients atteints d'épilepsie du lobe temporal (TLE). L'approche employée sert de modèle pour des analyses phénoménologiques pertinentes en neuroscience, psychologie, psychopathologie et parapsychologie. Elle permet un enregistrement

standardisé et détaillé, tout en requérant le développement de futurs questionnaires appropriés pour assurer l'homogénéité phénoménologique dans la recherche future et les méta-analyses. Le déjà-vu de SPE a des implications pour les conceptions de la précognition, de la réincarnation et de la rêverie.

Spanish

ORÍGENES DEL "DÉJÀ VU" Y FENOMENOLOGÍA: IMPLICANCIAS DE LOS CUATRO SUBTIPOS PARA LA INVESTIGACIÓN FUTURA

RESUMEN: Un análisis de los subtipos de " déjà vu", fue realizado, de acuerdo con la definición operacional, universalmente aceptada de Neppe, de las experiencias de "déjà vu" (cualquier impresión subjetiva de inapropiada familiaridad, de una experiencia presente, con un pasado indefinido), de 30 diferentes circunstancias, asociadas a la "experiencia déjà" y 50 explicaciones propuestas, para el fenómeno del déjà vu. Neppe hipotetizó y posteriormente demostró 4 subtipos nosológicamente distintos, representando 4 poblaciones diferentes que motivan 4 tipos de déjà vu, etiológicamente distintos: Déjà vu, en Experiencias Paranormales Subjetivas (EPS); " déjà vu" asociativo (en personas "normales", o no experimentadores de experiencias paranormales subjetivas. También en pacientes sin trastornos asociados a epilepsia al lóbulo temporal); déjà vu psicótico (en esquizofrenicos) y déjà vu, en pacientes con Trastorno al Lóbulo Temporal (TLT). El abordaje usado sirve como un modelo para análisis fenomenológicamente relevantes en neurociencia, psicología, psicopatologia y parapsicología. Esto permite recuentos estandarizados y relevantes, también requiere el desarrollo de nuevos cuestionarios apropiados que aseguren homogeneidad fenomenológica en posteriores investigaciones y meta-análisis. El Deja vu en EPS tiene implicaciones para la precognicion, reencarnación y los sueños.

German

DÉJÀ VU: URSPRÜNGE UND PHÄNOMENOLOGIE: IMPLIKATIONEN DER VIER UNTERGRUPPEN FÜR ZUKÜNFTIGE FORSCHUNG

ZUSAMMENFASSUNG: Déjà vu-Untergruppen werden analysiert in Übereinstimmung mit Neppes allgemein akzeptierter operationaler déjà vu-Definition (jeder subjektiv unangemessene Eindruck der Vertrautheit einer gegenwärtigen Erfahrung mit einer undefinierten Vergangenheit), 30 unterschiedlichen Ausprägungen für „déjà-Erfahrung" und 50 postulierten Erklärungen für déjà vu. Neppe vermutete und wies dann 4 phänomenologisch unterschiedliche nosologische Untergruppen nach, die 4 verschiedene, selbständige Populationen repräsentieren, die 4

ätiologisch getrennte déjà vu-Arten hervorbringen: subjektive paranormale Erfahrung (SPE) von déjà vu (bei SPE-Berichterstattern), assoziative déjà vu (bei scheinbar „Normalen“ ohne subjektive paranormale Erfahrung sowie bei Patienten mit nichtepileptischer Temporallappendysfunktion und Epilepsiepatienten ohne Temporallappenbeteiligung), psychotischem déjà vu (bei Schizophrenen) und temporallappenepileptischem (TLE)-déjà vu bei TLE-Patienten. Der Zugang dient als Modell für phänomenologisch bedeutsame Analysen innerhalb von Neurowissenschaft, Psychologie, Psychopathologie und Parapsychologie. Dies ermöglicht standardisierte, einschlägige Aufzeichnungen und erfordert die Entwicklung weiterer geeigneter Fragebögen zur Sicherstellung der phänomenologischen Homogenität zukünftiger Forschung und Metaanalysen. Déjà vu bei SPE hat Implikationen für Präkognition, Reinkarnation und Träumen.

DO SOME OF US HABITUATE TO FUTURE EMOTIONAL EVENTS?

By Adrian Parker and Björn Sjödén

ABSTRACT: From an evolutionary perspective, it may be advantageous not only to unconsciously react to emotionally threatening stimuli but also to habituate to these if they should prove harmless. A major purpose of the study was to test for the occurrence of this precognitive affective habituation at a subliminal level using emotionally loaded pictures. The design chosen here enabled us to evaluate whether or not participants habituated to emotionally loaded pictures and to see if they reacted selectively to just those target pictures that would later be repeatedly exposed, thus becoming potentially less threatening. It was further hypothesized that both the subliminal and the precognitive effects would relate to individual measures of emotional reactivity and transliminality. Fifty participants took part in the two successive computer steered procedures in order to respectively evaluate these aspects. A significant habituation effect was found for the negatively loaded targets. The overall findings failed to show a significant discrimination between those pictures than would be re-presented and those that were not. However, by selecting out the 34 individuals who showed affective habituation, a post hoc significant effect of precognitive habitation was found.

Keywords: precognition, psi, subliminal, affective habituation, emotional reactivity

The last decades of research with the introduction of the concepts of the emotional unconscious and the cognitive unconscious (Kihlstrom, 1987; Kihlstrom, Mulvaney, Tobias, & Tobis, 2000) have raised important questions concerning the limits of unconscious processes.

The evidence that emotional reactions to threat can occur without conscious recognition has been favourably reviewed by Arne Öhman (1999). Intuitive hunches and gut feelings have been shown to have a decisive role in reacting to threat (Katkin, Wiens, & Öhman, 2001; Öhman, 2000). The priority given to rapidly executed unconscious processes over those of conscious analytical ones is crucial for survival. As Öhman (1999, p. 338) vividly expressed it, "Predators strike hard and fast, and therefore time has always been a primary consideration for the fear system." It makes, then, evolutionary sense that exposure effects have been found to be stronger when the stimuli are exposed *subliminally,* that is, too quickly for conscious recognition, than when the participants are aware that they are being exposed to the stimuli (Dijksterhuis & Smith, 2002; Zajonc, 2001).

Turning now to studies relating more to the psychosocial context of emotion, two well-established psychological phenomena are *the mere exposure effect* and *affective habituation.* The mere exposure effect concerns how individuals come to be positively inclined toward stimuli they are frequently exposed to (Bornstein, 1989). Affective habituation refers to

the decreasing arousal with repeated exposure to an emotionally loaded stimulus (Dijksterhuis & Smith, 2002). These two effects are not so contradictory as they seem because the mere exposure effect requires the original stimulus to be of *neutral* value, whereas affective habituation occurs with *relatively strong* (negative or positive) value. The value or "valency" then gradually diminishes in strength with increased frequency of exposure. Despite the above theory, there is an almost complete absence of subliminal habituation studies carried out using *strongly* affective stimuli (Bem, 2003). Accordingly, a major aim of the present study was to rectify this shortcoming by testing whether or not affective habituation occurs when emotionally loaded pictures are subliminally presented.

The second issue concerns the temporal limits of this unconscious processing. The series of studies known as presensing or simply *presentiment* studies found that participants showed an unconscious physiological reaction immediately prior to the actual exposure of emotionally loaded pictures, and that this reaction was not shown with neutral ones (Bierman, 2000; Bierman & Radin, 1997; Bierman & Scholte, 2002). Although these results have been reviewed and discussed in relation to possible computational biases (Dalkvist, Westerlund, & Bierman, 2002), the findings have been further replicated with audio stimuli (Spottiswoode & May, 2003), and it would seem that there is as yet no simple conventional explanation for the presentiment effect.

Using these reports and findings as a starting point, Bem (2003) designed a computer-based procedure for testing an effect that is conceptually similar to presentiment, which he called *precognitive habituation*. Because an important feature of the work reported here concerns emotional targets, we use the term precognitive *affective* habituation. Like presentiment, precognitive affective habituation refers to the time-reversed influence of the stimuli before they are exposed, and, like presentiment, the effect has been found to be linked more specifically to negative than to positive stimuli. Nevertheless, the situation becomes a little more complicated because all forms of affective habituation are limited by the fact that individuals will seek to maintain an optimal level of arousal by virtue of their sensitivity to the negative stimuli.

Nevertheless, what emerges as a common denominator among presentiment, precognitive affective habituation, and subliminal affective habituation is that all of these processes may reflect a sensitivity or preference for reacting to *negatively loaded stimuli*. In the case of presentiment, the sensitivity is directed towards negatively loaded stimuli in the immediately impending future event, whereas in the case of precognitive habituation the sensitivity is toward negatively loaded stimuli, which the individual soon becomes repeatedly exposed to. It is this latter paradigm that was tested in Bem's studies.

The procedure that Bem used is an elegantly simple one: Participants were exposed to two pictures differing in content but with

similar negative or positive emotional valency, and the task was to click on the preferred member of the pair. Directly afterward, one of the pictures, designated as the "target," was randomly selected and then repeatedly flashed on the screen. The participant was next presented with two new pictures and the procedure repeated in a series of trials. The question was: Would the earlier preference for a picture be influenced backward in time by these subsequent exposures? If the participant selected the picture that was subsequently flashed, this counted as a "hit." For all trials, both in terms of the individual and the group, a simple hit rate could then be calculated (with the chance hit rate as 50%).

As was previously mentioned, it was thought necessary to take into account the need for maintaining an optimum level of arousal. Accordingly, it was predicted that the participants would prefer the subsequent target more often in trials in which *negative pictures* were presented (and less often in trials with positive pictures). These results have a high consistency of confirmation[1] at laboratories in several countries (Bem, 2004; Savva, Child, & Smith, 2004).

The Present Study

In designing the present study, attention was given to the fact that while the repeated (mere) exposure and affective habituation effects can be considered as well established, subliminal effects using the affective habituation procedure appear not to have been researched—at least not with naturalistic material. Hence we identify the effect studied here as *subliminal affective inhibition.*

An area of practical concern was whether to include positive (so-called erotic) pictures as naturalistic material in the design. In the previous studies by Bem (2003), the results were predicted to be less successful with positively loaded pictures than with negative pictures. There were also practical and ethical concerns about exposing participants to strongly loaded or valenced material in the form of explicit erotic pictures, as it could not be taken for granted that such pictures would be consistently perceived as positive by the participants. Because of this consideration and the fact that a previous replication had succeeded without the use of erotic pictures (Savva et al., 2004), we decided to use only negative and neutral pictures.

A further consideration in designing the study is related to the potential effect of individual differences on the results. To assess the participant's sensitivity to negatively arousing visual material, Bem (2003) constructed a simple, two-item personality measure that he called the *Emotional Reactivity Scale.*

As predicted by the precognitive affective inhibition hypothesis, scores on this scale correlated positively and significantly with the hit rate

[1] Overall, the target was selected in 52.6% of all negative trials ($p < .01$) and 48.0% of all positive trials ($p = .03$; Bem, 2003)

on negative pictures (but not on neutral pictures). Rather strikingly, the analysis showed that it was only the emotionally reactive participants who scored above chance on the precognitive habituation trials. Nevertheless, as *emotional reactivity* is not an established personality measure, there seemed to be some grounds for using a more general and theoretically grounded personality factor that might predict success in the precognitive affective habituation trials. The *Transliminality Scale* (Lange, Thalbourne, Houran, & Storm, 2000) was chosen because the concept of transliminality relates to the hypothesized tendency for psychological material to cross thresholds into or out of consciousness. Scores on the Transliminality Scale have been found to correlate with measures of creativity, magical ideation, absorption, fantasy proneness, and mystical experience (Lange et al., 2000). There is also some evidence that belief in psi is a reliable predictor of actual psi performance (see Parker & Brusewitz, 2004), and for this reason two items relating to belief were added to the transliminality scale.

Hypotheses

On the basis of the above considerations, three hypotheses were developed:

(1) The repeated subliminal exposure to extremely negative pictures will lead to the subliminal affective habituation of the individual's conscious preferences for these pictures. This will be shown as the ratings of previously subliminally exposed pictures being significantly less extreme than those of new pictures of the same kind. The effect is predicted to be absent for neutral pictures.
(2) Participants will differentially and significantly prefer the pictures that moments later are designated as targets. This precognitive affective habituation effect will occur significantly more often with negatively loaded pictures than with neutral, low-affect pictures.
(3) The above hit rate is predicted to be significantly higher for participants scoring high than those scoring low on scales designed to measure transliminality and emotional reactivity.

Method

Design

To test the above hypotheses, two studies were carried out independently in the form of computer-presented tasks, although in practice this occurred with the same participants in the same experimental session. The

same-subjects design also enabled further analyses in terms of order effects to study how affective habituation might relate to precognitive habituation. Because there was a concern that the previous exposure of negative images might influence the precognitive affective habituation procedure, the actual order in which the two computer programs were run was not counterbalanced. In order to maintain the novelty and integrity of the procedure, the precognitive affective habituation condition was always run first.

Study 1. This experiment was a replication attempt of the original precognitive habituation study reported by Bem (2003). The independent variable was the valency (negative or neutral) of the pairs of pictures exposed, and the dependent variable was the marked preference for one of the two pictures, measured as a "hit rate" in relation to the target exposed.

Study 2. This experiment tested the affective habituation hypothesis using subliminally exposed pictures of the same type (negative or neutral) used in Study 1. The design was based on that of Dijksterhuis and Smith (2002), in which study the independent variables were designated as *the valency of the picture* (negative or neutral) and *the type of exposure* (previously presented or newly presented). The dependent variable was the *picture preference* in terms of perceived valency of the pictures, as rated on an ordinal scale ranging from *extremely negative* to *extremely positive.*

Ethical Concerns

Research on subliminal perception can be ethically problematic because it involves some degree of manipulation that bypasses the participant's volitional control. Due to the extreme negative valency of some pictures used in the study, strong negative—and to some degree unconsciously elicited—affective reactions were to be expected and needed for the purpose of evaluating the hypothesized effects. In order to meet the ethical recommendations prescribed by the American Psychological Association (2002), the participants were therefore explicitly warned, both in the recruiting announcement and verbally in the laboratory, of the potentially strong negative content of some stimuli in the study. Participants were also told that they could withdraw from the study at any time without penalty. Afterward, they were fully debriefed and informed how they could later obtain a copy of the final report.

Participants

The preset goal was to recruit a minimum of 50 participants. In effect, 51 participants were recruited from announcements on notice boards at the university, by an appeal for volunteers to a first-year psychology class, and via personal contacts. Most of the participants belonged to the student population. One participant withdrew from the experimental session prematurely due to the unpleasant content of some of the pictures

used. Data from this participant were not used in the analysis. Thus, the preset goal was exactly met and data were obtained from 50 participants (27 women and 23 men, aged 19–63 years).

Instruments

Questionnaires. The main questionnaire used was Thalbourne's *Transliminality Scale: Form B* (Houran, Thalbourne, & Lange, 2003). The Transliminality Scale has been presented in several forms and versions (see e.g., Houran et al., 2003; Lange et al., 2000; Thalbourne, 2004). The latest form, the *Revised Transliminality Scale* (RTS) was used, which consists of all 29 items rated "true" or "false." Only 17 items are actually scored, the rest being filler items. The RTS has a documented test-retest reliability of .82 (Houran et al., 2003). Examples of scored items are, "At the present time, I am very good at make-believe and imagining" and "I think I really know what some people mean when they talk about mystical experiences."

Two additional questions related to *emotional reactivity* were derived from Bem (2003). The two questions were: "In general, how intense are your emotional reactions to movies, videos, or photographs that are violent, scary, or gruesome?" and "In general, to what degree are you aware of, attuned to, or in touch with your emotional reactions to images that are violent, scary, or gruesome?"

Responses could range from 1 (*not at all intensely aware*) to 5 (*very intensely aware*). As instructed by Bem (2003), anybody who scored above the midpoint (i.e., 4 or 5) on both scales was defined as "emotionally reactive"; all others were defined as "emotionally nonreactive." Besides this dichotomous grouping, a mean score on the emotional reactivity items (i.e., 1–5) was used for the correlational analysis.

A further two questions were added that related to ESP: "Do you believe that ESP exists?" and "Have you had any experiences that you believe were ESP?" These items were also measured on a five-point scale, ranging from *definitely no* to *definitely yes*. We label the scores on these questions as *apparent ESP-proneness*. The full questionnaire was translated into Swedish and the translation was checked by backward translation into English by a native speaker.

Pictures. Both the picture set provided with the original PH Program and the pictures used in Study 2 were selected from the International Affective Picture System (IAPS; Lang & Greenwald, 1993). The IAPS database contains 820 digitized photographs, rated for valency and arousal on scales from 1 (negative/low) to 9 (positive/high). Twenty-four pictures were selected for Study 2 after excluding the pictures already used in the PH condition. One set of 12 pictures constituted the extremely negative stimuli and consisted of those pictures from the IAPS with the lowest average valency scores ($M = 1.7$, $SD = 0.1$). These pictures typically depicted such material as explicit physical injuries and mutilated bodies. Another set

of 12 pictures made up the neutral stimuli and consisted of those pictures with valency scores closest to 5 (the mid-point of the scale; $M = 5.0$, $SD = 0.0$). These pictures typically depicted everyday objects such as a lampshade or a fan. To balance out possible differences in perceived valency among the selected pictures, the 12 neutral pictures and the 12 negative pictures were divided into two sets of six neutral and six negative pictures. One of the two picture sets was then randomly assigned to be subliminally exposed to every participant.

Computer equipment and software. Two equivalent desktop computer sets were used in the study. The systems met the technical specifications stated in the instruction manual for the original precognitive habituation studies by Bem (2003).

For both monitors, the screen refresh frequency was 60 Hz. This meant that visual stimuli could be theoretically exposed for, at a minimum, 16.7 ms (i.e., 1/60 s, or the exposure time of one screen refresh frame). The exposure time was minimized by using the techniques of forward and backward masking with plain visual stimuli (in this case colored patterns). These masking stimuli were displayed immediately before and after the exposure of a picture, ensuring that no image from the intended stimulus remained on the retina for longer than 16.7 ms. Although this exposure time enables most participants to occasionally identify the pictures, experience from previous studies has shown that participants are in fact most often wrong when asked what they have seen. Moreover, practical experience suggested that this exposure was sufficiently short to produce a subliminal effect (Bem, 2003; Robert Morris, personal communication, April 14, 2004).[2]

In *Study 1,* the affective precognitive inhibition study, the original precognitive inhibition program software was used. The program options were set on the experimenter's opening screen (which was not displayed to the participant), to exclude erotic pictures and to provide the "cool down period": an initial 5-min relaxation period during which the program displayed a starry sky on the screen and played the sound of ocean waves from an audio file.

In *Study 2,* the subliminal affective habituation study, the software used was Inquisit version 2.0 (a Windows-based program for producing rapid picture presentation on the computer screen). The accuracy of the exposure times using this software has been validated in independent studies using a photocell and the computer program FASTLOG to measure the actual exposure times Inquisit can produce.[3] The experiment was programmed in the Inquisit command language by the second author and presented to the participants in Swedish.

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[2] Other studies of subliminal perception, according to a recent Swedish dissertation on the subject, have used exposure times of up to 100 ms using this technique (Birgegård, 2004).

[3] For the specific report, see http://users.ugent.be/~adeclerc/inquisit/. Further details and technical specifications about the software can be found on the manufacturer's web page, www.millisecond.com.

Procedure

The initial questionnaire was sent out by email, to be completed by the participants before the study, either at home or as soon as they arrived at the laboratory.

The experimental sessions with individual testing were carried out during a 2-week period at the Department of Psychology at Gothenburg University. Instructions were provided in English in the computer program designed by Bem. In addition, participants were briefly introduced to the program in Swedish and instructed to always follow their "gut feelings" when making any choices concerning the presented stimuli. Beyond these points, the experimenter (in all cases the second author) was present only to start the second computer program, and returned only after the participant had completed the precognitive affective inhibition program. All the other instructions were given via the computer.

The experimental session began with a 5-min relaxation period prior to Study 1, the affective precognitive habituation study. Following the relaxation period, the participant marked the preference for one of two pictures displayed next to each other on the screen by clicking on it with the mouse. The pairs of pictures depicted were either negatively valenced or else neutral in content, as previously described. Immediately following the participant's choice, one of the pictures was randomly selected by the program and then repeatedly exposed subliminally: in total 12 times. For each 16.7 ms exposure, the selected picture was displayed randomly on either the left side or the right side of the screen. After these repeated exposures, two new pictures were presented, with the procedure being repeated for a total of 48 trials. The program took 15–20 min to complete. (This included short pauses for eye resting periods made after 1/3 and 2/3 of the trials.)

Following the display of the closing message in Study 1, the experimenter started the second program and briefly introduced its task. At this point, participants were also asked if they had experienced any technical difficulties; no one reported any.

Study 2, the subliminal affective habituation study, followed directly from Study 1 and contained two phases. First, participants were given six repeated subliminal exposures to the 12 pictures from one of the two picture sets. This occurred while they consciously focused on carrying out a simple categorization task by pressing the "A" key if a dark grey square appeared on screen or the "L" key if a light yellow square appeared. The participant was thereby advised to keep a left-hand finger on the "A" key and a right-hand finger on the "L" key. The subliminal exposures occurred before every colored square was displayed by first flashing a masking stimulus for nine screen-refresh frames (0.15 s), then a picture (neutral or negative) from the selected set for one frame (16.7 ms), followed by another masking stimulus for one frame (16.7 ms). The colored square was thereafter displayed until the participant responded

by pressing either the "A" key or the "L" key. Every picture in the set was exposed six times in random order, for a total of 72 trials. Next, participants were required to judge the valency of the whole set of 24 pictures, presented one at a time in random order, by clicking with the mouse on a 21-point scale ranging from -10 (extremely negative) to +10 (extremely positive). These pictures consisted of the set of six neutral pictures and six negative pictures that had been previously exposed, as well as the other set of six neutral and six negative pictures that had not been exposed. The whole program took 5–6 min to complete.

After the completion of both programs, participants were debriefed about the hypotheses of the experiment. An awareness check was also administered by asking the participants if they had been able to identify the pictures that flashed by. Most participants said that occurred at least occasionally; however, they were not confident what had been displayed or why. The participants were subsequently thanked and dismissed, and they were asked not to share the information with other potential participants.

Statistical Analyses

To reduce any ambiguity and to keep to the replication requirements, the planned statistical analyses for the affective inhibition study used the same methods that Bem (2003) had used, namely one-sample *t* tests of obtained hit rates against the chance hit rate of 50% and independent samples *t* tests to compare the results between emotionally reactive and nonreactive participants. Additionally, standard Pearson correlations were computed between hit rates and the selected personality measures.

In Study 2, the data were analyzed by a repeated measures analysis of variance (ANOVA), where the calculated means for the valency judgments of the pictures (previously exposed and not previously exposed) constituted the within-participants variables, and the picture set selected (Set 1 vs. Set 2) constituted the between-participants factor. This was done with the judgments of negative pictures and neutral pictures, respectively, as the dependent variable.

A further analysis used a difference score, calculated for both types of pictures with regard to the difference in the valency judgment scores between the results with the previously exposed (used) set of subliminal pictures and the new set of pictures. These *difference scores* were also analyzed in relation to the selected personality measures.

Results

We present the results in the actual order in which the studies were carried out (rather than the hypothesis order). The alpha level was preset to 95% ($p = .05$) and all tests were two-tailed.

Study 1

The overall hit rate on negative pictures was 51.0%, $t(49) = 0.51$, $p = .61$, and on neutral pictures, 50.3%, $t(49) = 0.17$, $p = 0.87$. These are very slight differences from the chance hit rate of 50% and failed to provide support for the affective precognitive habituation hypothesis.

Emotionally reactive participants ($n = 20$) achieved a slightly higher hit rate on negative pictures, 52.9%, compared to 49.7% for emotionally unreactive participants ($n = 30$), but the difference is not significant, $t(48) = -0.80$, $p = .43$). Moreover, emotionally reactive participants alone did not score significantly better than chance, $t(19) = 1.16$, $p = .26$. For the neutral picture trials, this difference became slightly less: Emotionally reactive participants obtained 50.8% neutral hits, compared to 50.0% for emotionally unreactive participants. The ESP prone participants ($n = 14$) obtained a hit rate on negative picture trials of 53.0%, compared to 50.2% for ESP nonprone participants ($n = 36$). On neutral picture trials, the hit rates for ESP prone and ESP nonprone participants were 50.6% and 50.2%, respectively.

There was an absence of a significant correlation between the transliminality scores and the hit rates on negative trials, $r = .03$, df 49, $p = .87$.

Study 2

There was a significant main effect of the previous exposure on the scores on negative pictures as the dependent variable, $F(1, 48) = 8.20$, $p < .01$. This effect was in the predicted direction, in that the previously exposed pictures were judged as less negative ($M = -7.08$, $SD = 1.63$) than pictures that had not been previously exposed ($M = -7.68$, $SD = 1.67$).

Using the scores on neutral pictures as the dependent variable, the effect was nonsignificant. Although neutral pictures that were not previously exposed were judged as slightly more positive ($M = 1.80$, $SD = 1.77$) than pictures that were previously exposed ($M = 1.62$, $SD = 1.85$), the difference was not significant. The results are summarized in Table 1.

Table 1
Evaluation of Means (Standard deviations) for Neutral and Extreme Negative Pictures as a Function of Previous Subliminal Exposure

	Exposed	Not exposed
Neutral pictures	1.62 (1.85)	1.80 (1.77)
Negative pictures	-7.08 (1.63)	-7.68 (1.67)

Note. -10 = extremely negative, 10 = extremely positive.

The Relation of the Psi Scores to Emotional Reactivity

For trials with negatively loaded pictures, the correlation between mean scores on emotional reactivity and difference scores between the exposed and unexposed negative pictures just reached significance, $r(49) = .28$, $p = .05$. For trials with neutral pictures, the correlation between transliminality and the difference scores between exposed and unexposed pictures was nonsignificant.

The Relation of Psi Scores to Transliminality

For trials with negatively loaded pictures, there was a nearly significant positive correlation between transliminality and the difference between pictures previously exposed and not previously exposed, $r(49) = .27$, $p = .06$. For trials with neutral pictures, the correlations between transliminality and the difference scores between exposed and nonexposed pictures, were both nonsignificant. There was also a small but significant correlation between transliminality and emotional reactivity, $r(49) = .31$, $p = .03$.

Post Hoc Analyses

In reviewing the preliminary findings, a colleague, Dick Bierman, commented that it would have been preferable for the two studies to have been carried out in the reverse order. This design would then have enabled us to select out the individuals who showed a sensitivity to affective habituation for the study of precognitive affective habituation. If precognitive habituation is a special case of subliminal affective habituation, then it would be most easily detected in specially selected individuals. Such selected individuals would then be expected to obtain higher scores on the subliminally exposed negatively loaded pictures, which they were previously exposed to, than on the previously unexposed ones. Accordingly, we selected out the 34 individuals who showed the affective habituation effect and compared their hit-rate scores on the precognitive habitation target pictures with the remaining 16 individuals who did fulfil the selection criterion. The average hit rate for the 34 affectively habituated individuals was 54.2%, whereas for the 16 nonhabituated individuals it was 44.3%. A t test of this difference was significant, $t(48) = -2.50$, $p = .016$, two-tailed.

In order to examine our third hypothesis**,** that concerning the influence of emotional reactivity and transliminality on scoring, a multiple regression was performed on the full data using transliminality, habituation, and emotionality as predictor variables for the dependent variable of the scores on negatively loaded targets. In practice, the usefulness of this analysis was limited by the fact that subliminal affective habituation is a dichotomous variable (either it occurred for participants in Study 2 or it

did not), and emotionality scores were based on just two questions. The model that emerged reached marginal significance, $F(3, 46) = 2.44$, $p = .077$. Only one variable, habituation, gave a significant positive loading for its beta coefficient $t(49) = 2.38$, $p = .021$.

Discussion

The major finding that subliminal affective inhibition occurs, and that it occurs with emotionally loaded pictures having a negative valency (rather than neutral content), confirms what many hunters know: You approach an animal slowly and in successive stages so that even if it does not appear outwardly to see you, you give it time to get used to the impending threat. In humans, we may feel a bodily uneasiness due to the continued presence of threat, but if no attack ensues, we can eventually become used to it without reflecting further on it.

Although the post hoc findings can be seen as supporting Bem's work, it is of course unfortunate that the order of the study confounded the clarity of the conclusion here. Had we not been concerned with the importance of maintaining the novelty of the pictures for the affective habituation study, this study would have been run afterward, thereby allowing us to select the participants with an emotional responsiveness to subliminal affective habituation. Indeed Bem's overall significant results indicate that it was only the emotionally reactive individuals who achieved a psi effect on the negatively loaded targets. Bem (2003) writes, "These results also imply researchers seeking to replicate the precognitive habituation effect can save time and effort by screening out nonreactive individuals ahead of time" (p. 11).

Although the effect of emotional reactivity failed itself to reach significance, it did correlate significantly with the scores predicting thereby whether the individual showed affective habituation or not. It should be mentioned that in one sense, the results here and in Bem's work are counterintuitive; it might easily be expected that emotionally sensitive individuals would be less likely to habituate, but it may instead be the case that it is an overreactivity that habituates.

Emotional reactivity to the target pictures was a rather crude measure based on a self-reported response to two questions: one concerning the intensity of the emotional reactions and one concerning how much attention is given to these reactions. Emotional reactivity may be closely related to the broader concept of *affect intensity*, on which there has been considerable work (see Larsen, 2009, for a review). Affect intensity concerns the degree to which the individual shows general and personal reactions to emotional stimuli and seems to be a variable with good reliability and construct validity. Daily moods, measured by experiential sampling methods, correlate well with the results from a questionnaire, the *Affect Intensity Measure*, and it is possible that such scores would provide a more

precise way of selecting participants. Ethical considerations would naturally demand a careful selection of pictures to eliminate the more gruesome pictures in the IAPS.

The present results indicate that we need to pay closer attention to individual differences in studying unconscious reactions to stimuli. The use of measures of emotional reactions and affect intensity show promise in this respect. In view of this fact, we consider our results to justify some attention to these variables and encourage further work in this regard.

How do these findings relate to the bigger picture? We began with a discussion of the limits of cognitive-emotive consciousness. There is a body of evidence (reviewed in Parker, 2003) suggesting that paranormal experiences relate to unconscious processes that generally gain their expression as a form of intuition, or in the content of spontaneous altered states of consciousness. Various attempts have been made to relate these experiences to subliminal perception, fantasy proneness, and dissociation (see, e.g., Irwin, 1990, 1994) and the development of the Transliminality Scale is an attempt to find a common ground. Interestingly, the variable ESP-proneness did appear in our post hoc analysis to interact with affective habituation as a predictor of a precognitive effect.

More than a hundred years has passed since William James wrote, "The subliminal region being thus established as an actuality, the next question is as to its farther limits, where it exists. My subliminal, for instance, has my ordinary consciousness for one of its environments, but has it additional environments on the remote side?" (James, 1903, p. 24). It has taken a century, but we now have the means, methods, and perhaps the motivation to answer James's question.

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Acknowledgment

The authors wish to thank the BIAL Foundation, Portugal, for supporting this work.

Abstracts in Other Languages

French

EST-CE QUE CERTAINS D'ENTRE NOUS S'HABITUENT A DE FUTURS EVENEMENTS EMOTIONNELS ?

RESUME : Selon une perspective évolutionniste, il pourrait être avantageux non seulement de réagir inconsciemment à des stimuli menaçants émotionnels mais aussi de s'habituer à eux s'ils se révèlent être inoffensifs. Un des principaux buts de l'étude suivante est de tester l'occurrence de l'habituation affective précognitive à un niveau subliminal en utilisant des images émotionnellement chargées. Le dispositif choisi ici nous a permis d'évaluer si les participants s'habituaient aux images émotionnellement chargées et de voir s'ils réagissaient sélectivement seulement à ces images cibles dont la présentation serait répétée plus tard, ce qui les rendrait potentiellement moins menaçante. Il fut ensuite fait l'hypothèse que tant les effets subliminaux et précognitifs seraient reliés aux mesures individuelles de la réactivité émotionnelle et de la transliminalité. Cinquante participants prirent part à deux procédures informatisées successives afin d'évaluer respectivement ces deux aspects. Un effet significatif d'habituation fut trouvé pour les cibles chargées négativement. L'ensemble des résultats ne parvint pas à montrer une discrimination significative entre les images qui seraient re-présentées et celles qui ne le seraient pas. Toutefois, en sélectionnant en post hoc 34 des individus qui montraient une habituation affective, un effet significatif d'habituation précognitive fut découvert.

Spanish

¿ALGUNOS DE NOSOTROS NOS HABITUAMOS A EVENTOS EMOCIONALES FUTUROS?

RESUMEN: Desde una perspectiva evolucionista, podría ser ventajoso reaccionar, no solo inconscientemente a estímulos emocionales amenazantes, si no que también, habituarse a estos estímulos, si ellos prueban que son no dañinos. Un propósito mayor del estudio fue poner a prueba la habituación afectiva precognitiva, a un nivel subliminal, usando fotos cargadas de contenido emocional. El diseño elegido nos permitió evaluar si los participantes se habituaron o no a las fotos con carga emocional e identificar si ellos reaccionaron selectivamente solo con aquellas fotos

objetivo, que posteriormente iban a ser repetidamente expuestas, lo que las haría potencialmente menos amenazantes. Fue posteriormente hipotetizado que los efectos subliminales y precognitivos se relacionarían con mediciones individuales de reactividad emocional y transliminaridad. Cincuenta participantes tomaron parte en dos procedimientos desarrollados computacionalmente, en orden de evaluar respectivamente estos aspectos. Un efecto de habituación significativa fue encontrado para las fotos objetivo cargadas emocionalmente con contenidos negativos. Una mirada general de los hallazgos falla en mostrar una capacidad de discriminación significativa, entre aquellas fotos que podrían ser re-presentadas y aquellas que no. Sin embargo, al seleccionar dentro del grupo de los 34 individuos que mostraron habituación afectiva, un efecto post hoc significativo de habituación precognitva fue encontrado.

German

GEWÖHNEN SICH MANCHE VON UNS
AN ZUKÜNFTIGE EMOTIONALE EREIGNISSE?

ZUSAMMENFASSUNG: Aus evolutionärer Perspektive könnte es von Vorteil sein, nicht nur unbewußt auf emotional bedrohliche Reize zu reagieren, sondern sich auch daran zu gewöhnen, sollten sie sich als harmlos herausstellen. Ein Hauptziel der Studie bestand darin, das Vorliegen einer solchen präkognitiven affektiven Habituation auf subliminaler Ebene unter Verwendung emotional aufgeladener Reize zu überprüfen. Die dafür gewählte Versuchsplanung hat zum Ziel, herauszufinden, ob sich die Versuchsteilnehmer an emotionale Bilder gewöhnen (oder auch nicht), und um festzustellen, ob sie selektiv gerade auf solche Zielbilder reagierten, die ihnen später mehrfach gezeigt und dadurch möglicherweise weniger bedrohlich wirken würden. Es wurde weiterhin angenommen, dass sich sowohl die subliminalen wie die präkognitiven Effekte auf individuell unterschiedliche Maße von emotionaler Reaktivität und Transliminalität beziehen würden. Fünfzig Teilnehmer nahmen an zwei aufeinanderfolgenden computergesteuerten Versuchen teil, um diese Aspekte in dieser Abfolge zu überprüfen. Ein signifikanter Habituationseffekt zeigte sich bei den negativ geladenen Zielbildern. Bei der Gesamtauswertung ergab sich kein signifikanter Unterschied zwischen denjenigen Bildern, die wiederholt präsentiert wurden, und solchen, bei denen dies nicht der Fall war. Bei getrennter Auswertung derjenigen 34 Personen, die die affektive Habituation zeigten, konnte post hoc ein signifikanter Effekt der präkognitiven Habituation nachgewiesen werden.

PARANORMAL BELIEF, SCHIZOTYPY, AND TRANSLIMINALITY

By Neil Dagnall, Gary Munley, Andrew Parker, and Ken Drinkwater

ABSTRACT: The current study investigated the relationship between paranormal belief and cognitive-perceptual personality measures. Participants completed a questionnaire battery containing a paranormal belief measure, the Schizotypal Personality Questionnaire (SPQ-B), and the Revised Transliminality Scale (RTS). Scores on the SPQ-B and RTS were found to be positively correlated with overall paranormal belief. Differences in level of paranormal belief for participants scoring high and low on each cognitive-perceptual measure were assessed. Participants above the median demonstrated higher levels of endorsement across all paranormal belief subscales (hauntings, aliens, superstition, other life, religion, PK, ESP, astrology, and witchcraft) than those scoring below the median. Partial correlation and hierarchical regression revealed the majority of the variance was explained by the cognitive-perceptual factor of the SPQ-B. In addition to this, within the regression model, the RTS was found to explain additional variance to that accounted for by the cognitive-perceptual factor of the SPQ-B.

Keywords: paranormal belief, transliminality, schizotypy

Schizotypy is a multifactorial psychological construct, which describes a continuum of personality characteristics and experiences related to psychosis, and in particular schizophrenia (Goulding, 2004). Three models have commonly been used to define schizotypy (Claridge, 1997; McCreery & Claridge, 2002). These view schizotypy as: (a) a disease, a milder form of schizophrenia (Meehl, 1962; Rado, 1953); (b) a personality dimension (Eysenck, 1960), psychoticism being the upper end of the normality-psychosis continuum (Goulding, 2004); and (c) both a healthy variation and a predisposition to psychosis, compromise model (Claridge, 1997). The latter two models suggest that level of schizotypy may influence cognitive-perceptual experiences within the general population and thus contribute to the formation and maintenance of paranormal belief.

This notion is supported by Irwin (2009), who postulates that clinically oriented variables, such as schizotypy, correlate with paranormal belief because they intrinsically entail reality testing deficits. It has been previously proposed that reality testing deficits per se may be fundamentally involved in the formation of paranormal beliefs (Alcock, 1981, 1995; Goode, 2000; Irwin, 2004; Vyse, 1997; Zusne & Jones, 1982). Consistent with this view, Irwin (2009) argues that reality-testing deficits bias individuals towards intuitive-experiential interpretations of anomalous events. Such interpretations lack analytical-rational processing and are likely to facilitate the generation of nonconventional "paranormal" explanations.

Once advanced, paranormal hypotheses are then maintained because subsequent evidence is not subjected to critical evaluation. Thus according to Irwin (2009), paranormal beliefs are formed and maintained because individuals fail to rigorously test self-generated explanations of the world (Irwin, 2004).

Studies examining the structure of schizotypy have consistently identified three underlying factors: aberrant perceptions and beliefs in other worlds (the positive symptoms of psychosis, i.e., hallucinations and delusions); cognitive failures (thought blocking and attentional difficulties) together with social anxiety; and introvertive anhedonia (inability to experience pleasure and social withdrawal; Goulding, 2004). These factors are reflected by the three domains of the Schizotypal Personality Questionnaire (SPQ; Raine, 1991): cognitive-perceptual, disorganised, and interpersonal. The SPQ was designed to reflect the major groups of schizophrenia symptoms (i.e., positive, negative and disorganised) (Andreasen, Arndt, Alliger, Miller, & Flaum, 1995; Compton, Goulding, Bakeman, & McClure-Tone, 2009). It is worth noting that there has been considerable recent debate regarding the factorial structure of schizotypy, which has resulted in some authors proposing the existence of a fourth (paranoid) factor (Compton et al., 2009; Stefanis, et al., 2004). Given the controversial status of this additional factor, the current paper will concentrate upon the traditional three-factor model of schizotypy.

Pertinently, schizotypal personality disorder has been found to be associated with cognitive and perceptual distortions, including odd beliefs or magical thinking (Goulding & Parker, 2001). Magical thinking in this context is defined as the belief in forms of causation which by conventional standards are considered to be invalid (Eckblad & Chapman, 1983). Thalbourne (2009) further explicates that magical ideation is a "belief, quasi-belief, or semi-serious entertainment of the possibility that events which, according to the casual concepts of this culture, cannot have a causal relation with each other, might somehow nevertheless do so" (Eckbald & Chapman, 1983, p. 215). Collectively, these findings explain the commonly reported positive correlation between schizotypy and paranormal belief (Genovese, 2005; Goulding, 2004, 2005; Wolfradt, Oubaid, Straube, Bischoff, & Mischo, 1999). Irwin (2009) reports the strength of correlation between schizotypy and paranormal belief to be about .6 or less (e.g., Thalbourne, 1985; Thalbourne & Delin, 1994).

Hergovich, Schott, and Arendasy (2008) expanded upon previous research when they explored the relationship between paranormal belief and schizotypy among adolescents. Hergovich et al. (2008) found that schizotypy was a predictor of some Revised Paranormal Belief (R-PBS; Lange, Irwin, & Houran, 2000a; Tobacyk, 1988; Tobacyk, 2004; Tobacyk & Milford, 1983) subscales (i.e., precognition, psi, witchcraft and spiritualism). Their findings also support the notion that paranormal belief is related to the positive symptoms of schizotypy (Genovese, 2005; Hergovich & Arendasy,

2007; Wolfradt et al., 1999); a stronger relationship was observed between paranormal belief and the cognitive-perceptual component of schizotypy than with the interpersonal and disorganised factors. While of great interest, the extent to which these findings can be generalised to adults is limited by the use of an adolescent sample. There is evidence that the factorial structure underlying paranormal belief in adolescents differs from that of adults (Wolfradt & Straube, 1998).

Work with schizotypy has been paralleled within the personality and paranormal literature by use of the perceptual-personality construct of transliminality (Thalbourne & Houran, 2000; Thalbourne & Maltby, 2008). Transliminality has been defined as hypersensitivity to psychological material (Thalbourne & Maltby, 2008), "a hypothesised tendency for psychological material to cross (*trans*) thresholds (*limines*) into or out of consciousness" (Thalbourne & Houran, 2000, p. 853; see also Houran & Thalbourne, 2003; Storm & Thalbourne, 1998–99; Thalbourne, 1999; Thalbourne, Keogh, & Witt, 2005). Thalbourne (1998) posits that high scores on paranormal belief measures correlate with scores on psychopathology measures because of this "leaky" mental threshold. Similarly, Hartmann (1991) adopted the notion of mental boundaries to explain the relative ease with which psychological material moves between different states of consciousness (Soffer-Dudek & Shahar, 2009). As Rabeyron and Watt (2010) point out, studies have frequently reported there to be a link between "thinner" mental boundaries and paranormal experiences (Houran, Thalbourne, & Hartmann, 2003; Kennedy, 2005; Palmer & Braud, 2002; Richards, 1996).

While a unitary construct, transliminality possesses seven underlying psychological variables: mystical experience, magical ideation, fantasy proneness, absorption, manic experience, dream interpretation, and hyperesthesia (Thalbourne, Crawley, & Houran, 2003). The construct of transliminality arose from a factor analysis of several variables related to paranormal belief and experiences and hence paranormal belief/experience is a core constituent of transliminality (Thalbourne & Houran, 2000). This explains why strong positive correlations have been reported between the Transliminality Scale and paranormal belief/experience, mystical experiences and magical thinking (Thalbourne, Bartemucci, Delin, Fox & Nofi, 1997; Thalbourne & Houran, 2000).

Thalbourne and Houran (2000) administered the Mental Experience Inventory (Kumar & Pekala, 1992) to respondents in Australia and the United States and found strong positive correlations between paranormal belief (subscales measuring belief in psi-related and unusual events, paranormal unusual experiences, and paranormal experience) and transliminality; no differences were found between the two national samples, and correlations ranged from .59 to .82. Similarly, Houran and Thalbourne (2001a), using measures derived from Kumar and Pekala (2001; Pekala, Kumar, & Marcano, 1995), found that the Revised

Transliminality Scale positively correlated with the encounters subscale (alleged encounters with beings and entities such as angels, the dead, and UFOs), .61; the poltergeist subscale (general phenomena associated with hauntings and poltergeists), .51; and seeing a ghost (a single item on apparitions), .38.

In addition to these findings, transliminality has been reported to be highly positively correlated with temporal lobe lability (Thalbourne et al., 2003). Furthermore, temporal lobe lability has been demonstrated to be associated with mystical experiences, paranormal beliefs, and psychic experiences (Persinger & Makarec, 1987; Persinger & Valliant, 1985). Indeed, Persinger (1995) reports having induced paranormal experiences by applying fluctuating magnetic fields across the temporal lobes of participants.

While schizotypy and transliminality have been psychometrically evaluated, validated, and established as independent constructs, it is clear that they share considerable common variance. Consequently, the present study sought to determine the extent to which each construct explained unique variance in a measure of paranormal belief. Particularly, the current study expanded upon previous research in several ways. Firstly, schizotypy and transliminality were considered in combination. While it was predicted that these constructs would be positively correlated with paranormal belief and each other, it is unclear which construct best predicts paranormal belief. The approach adopted in the present study was similar to that used by Thalbourne and Maltby (2008), who examined the relationship between transliminality (Houran, Thalbourne, & Lange, 2003; Lange, Thalbourne, Houran, & Storm, 2000; Thalbourne, 1998) and three correlated measures: Hartmann's Boundary Questionnaire (the Sumbound measure; Hartmann, 1991; Houran, Thalbourne, & Hartmann, 2003); the unusual experiences scale of the O-LIFE (Claridge, 1997; Mason, Claridge & Jackson, 1995; Mason, Claridge, & Williams, 1997); and a measure of temporal lobe lability (Persinger, 1984).

Thalbourne and Maltby (2008) found that transliminality and the three measures could be reduced to a single factor. Boundary thinness (Boundary Questionnaire; Sumbound) was the best measure of the underlying factor, however, transliminality was considered to be the most representative variable because: it had been Rasch-scaled, it was the shortest of the four measures, and transliminality had previously been found to be significantly related to performance on psychophysical threshold tasks using visual and vibro-tactile stimuli. Similarly, the current study intended to determine which of the inter-related measures (schizotypy and transliminality) best explained variance in a measure of paranormal belief.

Secondly, the present study examined whether paranormal belief was more highly correlated with the cognitive-perceptual of schizotypy than the interpersonal and disorganised factors (Genovese, 2005; Hergovich

et al., 2008; Wolfradt et al. 1999). Irwin and Green (1998) using the SPQ found that the cognitive-perceptual factor of schizotypy was related to belief in precognition and spiritualism, especially in females. Also, Houran, Irwin, and Lange (2001) reported the cognitive-perceptual factor predicted scores on the New Age factor of the R-PBS (Irwin, 2009; Lange et al, 2000a). Irwin (2009) suggests this is because the cognitive-perceptual factor of schizotypy, which parallels schizophrenia such as delusional symptoms in the normal population, is associated with intuitive-experiential reasoning. Such reasoning is likely to produce beliefs which are not based on reliable, objective evidence, and which are not subjected to critical analysis. In line with previous research it was predicted that the cognitive-perceptual factor would explain the majority of variance shared between schizotypy and paranormal belief.

Interestingly, Irwin and Green (1998) also found differences with regard to the disorganisation and interpersonal factors of the SPQ. Scores on the disorganisation scale were associated with endorsement of beliefs in extraordinary life forms and witchcraft, and disavowing belief in precognition and traditional religious views in men. People with schizotypal interpersonal deficits were found to be relatively inclined to embrace spiritualist beliefs but to disbelieve in psi and witchcraft. On the basis of these findings, Irwin and Green (1998) concluded that the relationship between paranormal beliefs and schizotypy was a complex one; beliefs vary across the three factors of schizotypy and each factor makes a positive and negative contribution to beliefs. Similarly, the relationship between schizotypy and subjective evaluation of paranormal experiences has been found to be complex. Schofield and Claridge (2007) found that highly cognitively organised participants reported positive schizotypy/pleasant experiences, whereas cognitively disorganised participants expressed negative schizotypy/distressing experiences.

Finally, a few studies have extended research beyond conventional measures of paranormal belief; the R-PBS (Lange et al., 2000a; Tobacyk, 1988; Tobacyk, 2004; Tobacyk & Milford, 1983) and the Australian Sheep–Goat Scale (Thalbourne, 1995; Thalbourne & Delin, 1993). The current study employed a broader measure of the paranormal, including subscales measuring other life and alien visitations alongside standard paranormal belief subscales (superstition, ESP, PK, etc.) (Dagnall, Munley, Parker, & Drinkwater, in press). Subdividing belief in this way enabled the current study to examine Hergovich et al.'s (2008) finding that only certain aspects of paranormal belief (i.e., belief in precognition, psi, witchcraft, and spiritualism) are predicted by schizotypy.

This approach is based upon a more comprehensive definition of paranormality than that enshrined within the R-PBS. The Paranormal Belief Scale (Tobayck, 1988) was based upon Broad's (1949/1978) definition of paranormality, which delineates paranormal phenomena as those that, if genuine, would violate the basic limiting principles of science.

This definition was later criticised by Lawrence (1995), who proposed a more exact definition, which focused upon hypothesized processes that are in principle physically impossible or outside the realm of human capabilities, as presently conceived by conventional scientists (Irwin, 1993). Considering this conceptual debate, the current study adopted a definition of paranormal consistent with the working definition proposed by Irwin: "a proposition that has not been empirically attested to the satisfaction of the scientific establishment but is generated within the nonscientific community and extensively endorsed by people, who might normally be expected by their society to be capable of rational thought and reality testing" (Irwin, 2009, p. 16-17).

Hergovich et al. (2008) argued that paranormal belief can be divided into components of paranormal belief that are strongly associated with schizotypy and those which are not. Using subscale measurements alongside a measure of overall paranormal belief enabled the present study to determine whether respondents high in transliminality demonstrate a similar pattern of paranormal endorsement to that found with schizotypy. In addition to this, the present study tested whether the findings of Hergovich et al. (2008) generalised beyond adolescents to a broader adult sample.

Method

Respondents

The participant pool comprised 320 respondents. Eighty-five respondents were males (27%) and 235 (73%) female. The ages ranged between 17 and 60 years with a mean age of 26.45 ($SD = 9.86$). Respondents were recruited from the psychology program at the Manchester Metropolitan University and by research students using snowball sampling, which involved asking people to participate and encouraging contacts to take part in a study concerned with measuring a "variety of different types of beliefs."

Materials

A booklet containing four questionnaire measures was presented: paranormal belief, the Schizotypal Personality Questionnaire, Version B (SPQ-B; Raine & Benishay, 1995), and the Revised Transliminality Scale (RTS; Lange et al., 2000). Presentation order was counter-balanced across booklets.

The paranormal belief measure was a composite measure derived from principal component analysis of several existing measures: Revised Paranormal Belief Scale (R-PBS; Lange et al., 2000a; Tobacyk, 1988; Tobacyk, 2004; Tobacyk & Milford, 1983); Australian Sheep-Goat Scale (Thalbourne, 1995; Thalbourne & Delin, 1993); Paranormal Short

Inventory (PSI; Randall, 1997); Manchester Metropolitan University Scale of Paranormal Belief (Foster, 2001); Superstition Scale (Wiseman & Watt, 2004); Poltergeists and Hauntings Scale (Kumar & Pekala, 2001); and Extraterrestrial Life and UFO-related Beliefs (Chequers, Joseph, & Diduca, 1997; Dagnall et al., 2009).

Overall there were 58 items corresponding to nine belief clusters: hauntings and communication with the dead (8 items), internal reliability .96 (Hauntings); existence of life on other planets (6 items), .91 (Other Life); superstition (7 items), .90 (Superstition); religious belief and belief in the after life (6 items), .91 (Religion); extra-terrestrial visitations (8 items), .95 (Aliens); extrasensory perception (7 items), .89 (ESP); psychokinesis (6 items), .93 (PK); astrology (7 items), .91(Astrology); and witchcraft (3 items), .84 (Witchcraft). All items were responded to on a 5-point scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*) and were randomly ordered so that they appeared in a single self-report questionnaire.

Schizotypy was assessed using the SPQ-B (Raine & Benishay, 1995). The SPQ-B is an easy-to-administer, 22-item instrument derived from the 74-item SPQ; it was used in the current study because of time constraints. The SPQ-B correlates highly with the longer version of the SPQ and is a widely used research tool (Bailey & Swallow, 2004). The SPQ-B includes items from each of the three subscales of the SPQ: cognitive-perceptual, (8 items); interpersonal (8 items); and disorganized (6 items). The SPQ-B consists of questions or statements that are responded to with "yes" or "no" answers. All yes responses are totalled to produce an overall score ranging from 0 to 22; higher scores indicate higher levels of self-reported schizotypy. Internal consistency reliability, test–retest reliability, and criterion validity of the SPQ-B have been demonstrated to be good (Raine & Benishay, 1995). Raine and Benishay (1995) found the internal reliabilities of the three subscales ranged from .72 to .80, with a mean of .76. Similarly, Axelrod, Grilo, Sansilow, and McGlashan (2001) observed reliabilities ranging from .74 to .76.

The RTS is a Rasch (1960/1980) scaled version of Thalbourne's (1998) original 29-item, true/false scale (Lange et al., 2000). While all 29 items are administered, 12 items from the original scale are excluded from scoring due to age and gender biases (see Houran et al., 2003). The Rasch reliability of this scale is .82, which translates to a KR-20 reliability coefficient of .85. Additionally, the scale possesses good test–retest reliability, .82 (Houran et al., 2003). The RTS has established construct validity, and it has been found to correlate with experimental measures and experiential and attitudinal phenomena (Crawley, French, & Yesson, 2002; Thalbourne, 2000b). The RTS produces scores at an interval-level of measurement. The Rasch scaled score has a mean of 25 ($SD = 5$); scores above the Rasch mean indicate high levels of trait transliminality compared to scores below the Rasch mean.

Procedure

All respondents were approached individually or in small groups and were asked to complete the questionnaire. A statement was attached to the front of each questionnaire outlining the topic and content. A brief sheet was also included which indicated that participation was voluntary and that responses would remain anonymous. Participants were asked to ensure that they responded to each questionnaire item as openly and spontaneously as possible. They were also asked to ensure that they responded to all the presented items.

Results

Reliability Analysis and Descriptive Statistics

The SPQ-B overall was found to have good internal reliability (George & Mallery, 2003); Cronbach's α = .85. The subscales of the SPQ-B were also found to have acceptable to good reliability; cognitive-perceptual (α = .67), interpersonal (α = .80), and disorganised (α = .72). The RTS also demonstrated acceptable internal reliability (α = .79). See Table 1 for summary statistics.

Table 1
Summary Statistics for the SPQ-B, PDI, and RTS

	M	*SD*	α
SPQ-B	8.28	5.08	.85
Cognitive-perceptual	3.33	2.13	.67
Interpersonal	3.09	2.46	.80
Disorganised	1.86	1.77	.72
RTS	21.45	4.15	.79

Principal Component Analysis

To ensure that the nine-factor solution was applicable to the current sample, a principal component analysis (PCA) was undertaken. Prior to the PCA, the suitability of the paranormal belief scale data was assessed. Inspection of the correlation matrix revealed the presence of coefficients .3 and above. The Kaiser-Meyer-Oklin value was .94, exceeding the recommended value of .6 (Kaiser, 1970, 1974). Bartlett's Test of Sphericity was significant, supporting the factorability of the correlation matrix, $\chi^2(1653) = 17454.70$, $p < .001$. A cut-off point of .50 for factor loadings was adopted, that is, only those items scoring higher than this threshold were retained for further analyses (Comrey & Lee, 1992). The PCA accounted for 71.12% of the total variance (see Table 2). All

emergent factors had eigenvalues exceeding 1[1] (Kaiser, 1960) as indicated by a step change in the scree slope (Cattell, 1966), demonstrated good levels of internal consistency, and were conceptually distinct (see Table 3).

Table 2
Factor Loadings for the Total Paranormal Belief Scale and the Variance Explained by Each Factor

	Eigenvalue	Variance Explained	Cumulative Variance
Hauntings	20.96	36.1%	36.1%
Alien	6.62	11.4%	45.5%
Superstition	3.59	6.2%	53.7%
Other life	3.14	5.4%	59.1%
Religion	2.32	4.0%	63.1%
PK	1.86	3.2%	66.3%
ESP	1.68	2.9%	69.2%
Astrology	1.49	2.6%	71.8%
Witchcraft	1.31	2.3%	74.1%

The overall Paranormal Belief Scale and interscale correlations are contained in Table 4. Paranormal belief overall was found to have excellent internal reliability, α = .97. The paranormal belief subscales/scales were also found to have excellent internal reliability; Hauntings α = .96, Alien α = .94, Superstition α = .91, Other life α = .92, Religion α = .91, PK α = .95, ESP α = .90, Astrology α = .93, and Witchcraft, α = .88. See Table 3 for summary statistics.

Table 3
Summary Statistics for the Paranormal Belief Scale and Subscales

	M	*SD*	α
Overall	2.86	0.65	.97
Hauntings	3.04	1.07	.96
Alien	2.41	0.78	.94
Superstition	2.70	0.97	.91
Other life	3.66	0.80	.92
Religion	3.11	1.01	.91
PK	2.53	0.94	.95
ESP	2.99	0.95	.90
Astrology	2.40	0.86	.93
Witchcraft	2.89	1.11	.88

[1] The most frequently used method for identifying factors is the eigenvalue-greater-than-1 rule (Henson & Roberts, 2006; Thompson & Daniel, 1996).

Interparanormal belief scale correlations are presented in Table 4.

Table 4
Paranormal Belief Scale and Subscale Correlations

	1	2	3	4	5	6	7	8	9	10
1. Overall										
2. Hauntings	.88*									
3. Aliens	.60*	.49*								
4. Superstition	.53*	.41*	.13**							
5. Other life	.33*	.20*	.41*	-.06						
6. Religion	.63*	.54*	.14**	.29*	.03					
7. PK	.80*	.67*	.57*	.24*	.27*	.37*				
8. ESP	.81*	.69*	.40*	.38*	.19*	.44*	.62*			
9. Astrology	.79*	.69*	.36*	.56*	.07	.43*	.58*	.67*		
10. Witchcraft	.74*	.61*	.37*	.23*	.13**	.47*	.59*	.54*	.48*	

* $p < .001$. ** $p < .01$. Significance levels are one-tailed.

Correlational Analysis: Schizotypy, Delusional Ideation, and Transliminality

Correlations between the paranormal belief scale, SPQ-B, and RTS were explored. Scores on the paranormal belief scale were positively correlated with SPQ-B, $r(318) = .26$, $p < .001$; and RTS, $r(318) = .42$, $p < .001$. Significant positive correlations were also observed between the SPQ-B and RTS, $r(318) = .52$, $p < .001$.

The relationship between the paranormal belief scale and the SPQ-B subscales (cognitive-perceptual, interpersonal, and disorganised) was examined further. Significant positive correlations were observed between the paranormal belief scale and the cognitive-perceptual factor, $r(318) = .47$, $p < .001$; and the paranormal belief scale and the disorganised factor, $r(318) = .11$, $p = .026$. The correlation between the paranormal belief scale and the interpersonal factor was not found to be significant, $r(318) = .04$.

This pattern of results suggests that scores on the cognitive-perceptual measures of the SPQ-B are more strongly associated with scores on the paranormal belief scale. Indeed, partial correlations between the paranormal belief scale interpersonal and disorganised factors, controlling for the cognitive-perceptual factor, produced small significant negative correlations: paranormal belief scale and interpersonal, $r(317) = -.17$, $p = .001$, and paranormal belief scale and disorganised, $r(317) = -.12$, $p = .017$. For this reason, the interpersonal and disorganised factors were omitted from subsequent analyses.

Tests of Difference

Three one-way between-groups multivariate analyses of variance (MANOVAs) were performed for above and below the median scores on cognitive-perceptual, PDI, and RTS to investigate differences in paranormal belief. Nine dependent variables were used: Hauntings, Alien, Superstition, Other life, Religion, PK, ESP, Astrology and Witchcraft.

For below versus above scores on the cognitive-perceptual factor, a significant difference was observed on the combined dependent variables, $F(9, 310) = 7.93$, $p < .001$; Wilks' Lambda = .81; $\eta_p^2 = 0.19$.[2] Respondents above the median on the cognitive-perceptual factor scored higher on the paranormal belief scale ($M = 3.14$, $SD = 0.56$) than those below the median ($M = 2.63$, $SD = 0.63$). Differences on each of the dependent variables were found to be significant (see Table 5).

Table 5
The Cognitive-Perceptual Factor and Scores on Each Paranormal Belief Scale and Subscale

	Cognitive-perceptual median split							
	Below		Above					
	M	*SD*	*M*	*SD*				
Factor	($n = 177$)		($n = 143$)		*F*	*df*	*p*	η_p^2
Hauntings	2.72	1.02	3.44	0.99	40.49	1,318	< .001	0.11
Alien	2.26	0.72	2.60	0.82	15.27	1,318	< .001	0.05
Superstition	2.49	0.91	2.96	0.98	19.13	1,318	< .001	0.06
Other life	3.52	0.78	3.83	0.79	12.59	1,318	< .001	0.04
Religion	2.87	1.02	3.41	0.91	24.39	1,318	< .001	0.07
PK	2.32	0.93	2.80	0.89	21.95	1,318	< .001	0.07
ESP	2.67	0.92	3.39	0.82	52.74	1,318	< .001	0.14
Astrology	2.16	0.81	2.69	0.84	33.33	1,318	< .001	0.10
Witchcraft	2.64	1.10	3.19	1.04	19.96	1,318	< .001	0.06

For below versus above scores on the RTS, a significant difference was observed on the combined dependent variables, $F(9, 310) = 5.62$, $p < .001$; Wilks' Lambda = .86; $\eta_p^2 = 0.14$. Respondents above the median on the RTS scored higher on the paranormal belief scale ($M = 3.07$, $SD = 0.61$) than those below the median ($M = 2.66$, $SD = 0.62$). Differences on Hauntings, Alien, Other, life PK, ESP, Astrology, and Witchcraft were found to be significant. With

[2] Cohen (1988) suggested that partial η_p^2 effects should be interpreted using the following rule of thumb: values between .01 and .06 reflect a small effect size, values within the .06–.13 range a medium effect size, and a value of .14 or higher indicates a large effect.

Bonferroni adjustment for multiple comparisons, differences on Superstition and Religion were not found to reach significance (see Table 6).

Table 6
RTS Factor and Scores on Each Paranormal Belief Subscale

	Transliminality median split							
	Below		Above					
Factor	*M* (*n* = 164)	*SD*	*M* (*n* = 156)	*SD*	*F*	*df*	*p*	η_p^2
Hauntings	2.74	1.02	3.36	1.03	28.62	1,318	< .001	0.08
Alien	2.28	0.71	2.54	0.83	9.14	1,318	= .003	0.03
Superstition	2.58	0.91	2.83	1.00	5.29	1,318	= .022	0.02
Other life	3.50	0.79	3.82	0.77	13.68	1,318	< .001	0.04
Religion	2.97	1.03	3.27	0.96	7.40	1,318	= .007	0.02
PK	2.33	0.89	2.74	0.94	16.35	1,318	< .001	0.05
ESP	2.70	0.88	3.30	0.92	34.84	1,318	< .001	0.10
Astrology	2.16	0.80	2.65	0.85	28.19	1,318	< .001	0.08
Witchcraft	2.64	1.03	3.15	1.13	17.36	1,318	< .001	0.05

Regression

A hierarchical regression with the predictor variables (cognitive-perceptual and RTS) entered in order of zero-order correlation with paranormal belief scale was performed. Multicollinearity was assessed using the variance inflation factor (VIF). Multicollinearity is generally considered to be severe if the VIF is greater than 5 (Yang, 2007). In the current study the VIF for all variables considered within the model was within the recommended tolerance (maximum observed value was 2.16).

A significant model was observed at each step of the regressions: Step 1, when the cognitive-perceptual factor was entered, $F(1, 318) = 94.08$, $p < .001$; and Step 2, when RTS was added to the cognitive-perceptual factor, $F(2, 317) = 59.04$, $p < .001$. A significant R^2 change was observed in Step 2 when RTS was added to the cognitive-perceptual factor, $F_{change}(1, 317) = 18.75$, $p < .001$. There was a 4.1% increase in adjusted R^2, which improved from 22.6% to 26.7%. Partial correlation between scores on the paranormal belief scale and RTS, controlling for the cognitive-perceptual factor, found a significant correlation, $r(317) = .19$, $p < .001$; $d = .39$. It is clear from this pattern of results that scores on the paranormal belief scale are best predicted by the cognitive-perceptual factor of SPQ-B, and that RTS scores contribute additional significant variance to the model (see Table 7).

Table 7
Hierarchical Regression Predicting Paranormal Belief

	B_1	B (*SE*)	B_2	R^2	*t*	*p*	R^2	F_{Change}	*p*
Step 1									
Cognitive-perceptual	.15	.02	.48	.23	9.70	< .001			
Step 2									
Cognitive-perceptual	.10	.02	.32	.27	5.28	< .001			
RTS	.04	.01	.26		4.33	< .001	.04	18.75	<.001

Bivariate Correlations

Finally, a series of correlations were conducted between the cognitive-perceptual factor and each of the paranormal belief subscales/scales identified in the Hergovich et al. (2008) study; ESP, witchcraft and hauntings with superstition and religion (see Table 8).

Table 8
Cognitive-Perceptual Factor and Paranormal Belief Subscale Correlations

	1	2	3	4	5	6
1. Cognitive-perceptual						
2. Hauntings	.40*					
3. ESP	.48*	.69*				
4. Witchcraft	.27*	.61*	.54*			
5. Superstition	.30*	.41*	.38*	.23*		
6. Religion	.27*	.54*	.44*	.47*	.29*	

* $p < .001$. Significance levels are one-tailed.

The strength of relationship between subscales previously found to be strongly associated with schizotypy (ESP, Witchcraft, and Hauntings) was compared with subscales found to be less strongly associated (superstition and religion). ESP was found to be more strongly correlated with cognitive-perceptual scores than superstition ($z_{diff} = 3.24$, $p < .001$) and religion ($z_{diff} = 3.95$, $p < .001$). Similarly, haunting beliefs were found to be more strongly

correlated with cognitive-perceptual scores than superstition (z_{diff} = 1.79, p = .037) and religion (z_{diff} = 2.61, p = .0045). Witchcraft was not found to correlate more strongly with cognitive-perceptual scores than superstition (z_{diff} = 0.46) and religion (z_{diff} = 0.00).

Schizotypy, transliminality and paranormal belief. The expected positive correlation between schizotypy and transliminality was observed. In addition, schizotypy was found to be positively correlated with paranormal belief. Examination of the SPQ-B subscale correlations together with partial correlation established that the cognitive-perceptual factor was more strongly associated with paranormal belief than the interpersonal and disorganised factors. Similarly, transliminality was found to be positively correlated with paranormal belief.

Considering scores on each of the paranormal subscales, a similar pattern of endorsement was found for participants above and below the median on the cognitive-perceptual SPQ-B factor and the RTS. Finally, hierarchical regression revealed that scores on the paranormal belief scale were best predicted by the cognitive-perceptual factor, and that the RTS predicts additional variance.

Paranormal subscale and the cognitive-perceptual factor of schizotypy. As predicted, both ESP and haunting beliefs were found to be more strongly correlated with cognitive-perceptual scores than superstition and religion. However, the predicted difference between witchcraft and superstition and religion was not observed.

Discussion

The current study found that paranormal belief was most strongly correlated with the cognitive-perceptual factor of the SPQ-B. This factor is comprised of items tapping into atypical cognitions and perceptions (i.e., ideas of reference, odd beliefs/magical thinking, unusual perceptual experiences and paranoid ideation). As predicted, the cognitive-perceptual factor was also positively correlated with transliminality. This relationship is explained by the fact that both constructs share considerable common variance; for example, magical ideation is included within both scales. Despite this overlap, the RTS accounted for additional variance in the paranormal belief scale. This may be because transliminality is a broad construct containing several underlying psychological domains: (fleeting) hypomanic or manic experience, mystical experience, absorption, hyperesthesia, positive attitude towards dream interpretation, magical ideation, and fantasy-proneness (Thalbourne et al., 2003; Thalbourne & Houran, 2005).

Interestingly, the cognitive-perceptual factor in combination with the RTS accounted for only 27% of the variance in the paranormal measure. This indicates that other variables must play an important role in the formation, development, and maintenance of paranormal beliefs. One explanation for this finding is provided by Irwin (2004, 2009), who

posits that clinically oriented variables correlate with paranormal belief because they intrinsically contain items tapping into reality testing deficits. Several previous studies have reported that reality testing deficits may be fundamentally involved in the formation of paranormal beliefs (Alcock, 1981, 1995; Goode, 2000; Irwin, 2004; Vyse, 1997; Zusne & Jones, 1982). Thus it is not schizotypy and transliminality per se that are related to the formation, development, and maintenance of paranormal beliefs but the reality testing deficits inherent within both measures.

Comparing participants above and below the median on each of the paranormal belief subscales revealed important findings. Firstly, significant differences were observed on each of the nine subscales (Hauntings, Alien, Superstition, Other life, Religion, PK, ESP, Astrology, and Witchcraft). Secondly, the pattern of results for the cognitive-perceptual factor and transliminality were similar, although marginally higher effect sizes were observed for the cognitive-perceptual factor. The observed effect sizes overall were typically within the small range (partial eta-squared between .01 and .06; Cohen, 1988). Differences on the Hauntings, ESP, and Astrology scales produced effect sizes within the medium range (partial eta-squared between .06–.13; Cohen, 1988). Finally, similar large effect sizes were found for overall paranormal belief (partial eta-squared .14 or higher; Cohen, 1988); participants above the median on the cognitive-perceptual factor and transliminality were more accepting of paranormal beliefs.

The differential endorsement rate of paranormal subscales provides some support for Hergovich et al. (2008). Using a sample of adolescents, Hergovich et al. (2008) found that schizotypy was a predictor of R-PBS subscales measuring precognition, psi, witchcraft, and spiritualism, whereas subscales measuring belief in traditional religious contents, superstitious thoughts, and belief in extraordinary life forms were better predicted by paranormal belief scores. Hergovich et al.'s (2008) specific findings were difficult to evaluate in the context of the current study because a different measure of paranormal belief was employed. The closest correspondence was found comparing ESP, Witchcraft, and Hauntings with Superstition and Religion. Comparing the correlation strengths of these subscales revealed that ESP and Hauntings were more strongly correlated with the cognitive-perceptual factor than Superstition and Religion. However, Witchcraft did not differ from Superstition and Religion; all three subscales were similarly correlated with the cognitive-perceptual factor. This difference may be explained by the fact that Hergovich et al. (2008) used a sample of adolescents, whereas the current study used an older, more heterogeneous sample. There is evidence to suggest that the underlying beliefs of adolescents differ from those of adults (Hergovich et al., 2008).

The results of the current study support previous research demonstrating a strong relationship between the cognitive-perceptual factor of schizotypy and paranormal belief (Genovese, 2005; Hergovich & Arendasy,

2007; Hergovich et al., 2008; Wolfradt et al., 1999). The disorganised factor was found to be only weakly correlated with paranormal belief, while the interpersonal factor failed to produce a significant correlation. In line with Hergovich et al. (2008), the current results suggest that the disorganised and interpersonal factors do not directly contribute to the formation and maintenance of paranormal beliefs. As in other similar studies, it is evident that paranormal belief is related to positive schizotypy (Brugger & Graves, 1997; Hergovich et al., 2008; Mohr, Graves, Gianotti, Pizzagalli, & Brugger, 2001). Future studies may wish to explore this relationship further by exploring differences between positive symptom clusters (ideas of reference, odd beliefs/magical thinking, unusual perceptual experiences, and paranoid ideation).

Overall, the schizotypy findings require cautious interpretation because both the disorganised and interpersonal factors have been found to influence the evaluation of paranormal experiences. For example, Schofield & Claridge (2007) reported that highly disorganised participants showed a negative schizotypy/distressing experiences relationship, while cognitively organised participants demonstrated a positive/pleasant experiences relationship. Thus, while the disorganised and the interpersonal factors may not be directly involved with the development of paranormal beliefs, they appear to play an important role in the pathologisation of anomaly proneness. For this reason, future studies may wish to consider the interaction between scores on the cognitive-perceptual, disorganisation, and interpersonal factors. Particularly high scores on the cognitive-perceptual factor could be subdivided on the basis of high versus low disorganisation scores. This may provide useful insights into the complex relationship between schizotypy and paranormal beliefs (Irwin & Green, 1998).

Finally, it is worth noting that several self-report measures have been developed to measure schizotypy in nonclinical individuals (Chapman, Chapman, & Kwapil, 1995; Mason et al., 1997b). While these measures focus upon the schizophrenia spectrum and schizophrenia, there is considerable variation in item content (Mason & Claridge, 2006). The SPQ-B (Raine & Benishay, 1995) used in the present study has a broad remit and was designed to represent the DSM symptoms of Schizotypal Personality Disorder. This measure has the advantage of being quick to administer and provides a valid and reliable measure of overall and subscale scores (cognitive-perceptual, interpersonal, and disorganised); however, the measure is not capable of providing reliable and valid indices of the individual features of schizotypal personality disorder (Raine & Benishay, 1995). Consequently, subsequent studies may wish to use a more comprehensive measure.

Particularly important in this context is the distinction between scales based on the full (e.g., O-LIFE; Mason et al., 1995) and quasi-dimensional (e.g., Chapman scales; Chapman et al., 1995) approaches to schizotypy. The fully dimensional model describes schizotypy on a personality

continuum, with traits representing a healthy variation and a predisposition to psychosis (Claridge, 1997). By contrast, the quasi-dimensional or disease model views schizotypy as a milder form of schizophrenia (Meehl, 1962). Therefore, future studies examining the relationship between schizotypy and paranormal belief need to consider carefully the impact of scale choice on the generality of their research findings.

The current research findings are important for a number of reasons. Firstly, they contribute to the burgeoning literature examining the relationship between cognitive-perceptual factors and paranormal belief. Secondly, they offer further insights into the role of schizotypal ideation (Pizzagalli et. al, 2000). Finally, the paper explores the relationship between schizotypy and transliminality. This is important because although the SPQ-B and RTS were developed and originated separately, in the context of paranormal belief they appear to overlap and complement each other.

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Abstracts in Other Languages

French

CROYANCE PARANORMALE, SCHIZOTYPIE ET TRANSLIMINALITE

RESUME : La présente étude étudie la relation entre la croyance paranormale et des mesures de la personnalité cognitivo-perceptuelle. Les participants ont complété une batterie de questionnaires dont une mesure de la croyance paranormale, le Questionnaire de la Personnalité Schizotypique (SPQ-B), et l'Echelle Révisée de la Transliminalité (RTS). Les scores sur la SPQ-B et la RTS furent positivement corrélés avec la croyance paranormale. Des différences dans les niveaux de croyance paranormale furent détectées entre les participants qui avaient des scores élevés ou faibles à une mesure cognitivo-perceptuelle. Les participants au-dessus de la médiane montraient des niveaux plus élevés d'acception de toutes les sous-échelles de la croyance paranormale (hantises, extraterrestres, superstition, autres vies, religion, PK, ESP, astrologie et sorcellerie) que ceux ayant des scores en-dessous de la médiane. Une corrélation partielle et une régression hiérarchique révèlent que la majorité de la variance s'expliquait par le facteur cognitivo-perceptuel de la SPQ-B. En plus de cela, au sein du modèle de régression, la RTS expliquait la variance additionnelle à celle dont rendait compte le facteur cognitivo-perceptuel de la SPQ-B.

Spanish

CREENCIA EN LO PARANORMAL,
ESQUIZOTIPIA Y TRANSLIMINARARIDAD

RESUMEN: El siguiente estudio, investigo la relación entre creencia en lo paranormal y medidas de personalidad cognitivo-perceptuales. Los participantes completaron una batería de cuestionarios que contenía una medida de creencia en lo paranormal, el Cuestionario de Personalidad Esquizotípica (CPE), y la Escala de Transliminaridad Revisada (ETR). Puntajes del CEP y ETR fueron encontrados que correlacionaban positivamente con los índices generales de creencia en lo paranormal. Diferencias en el nivel de creencia en lo paranormal, para los participantes con puntajes altos y bajos en cada medida cognitivo-perceptual, fueron realizadas. Participantes sobre la media demostraron altos niveles de aprobación a través de todas las subescalas de creencia en lo paranormal (apariciones, alienígenas, supertición, otras vidas, religión, PK, PES, astrología y brujería), con respecto a los que puntuaron bajo la media. Análisis de correlaciones parciales y regresión jerárquica, revelaron que la mayoría de la varianza era explicada por el factor cognitivo-perceptual del CPE. Además de esto, dentro del modelo de regresión lineal, se encontró que el ETR explicó un nivel de varianza adicional a la correspondiente al nivel de varianza identificado al factor cognitivo perceptual del CPE.

German

PARANORMALE GLAUBENSEINSTELLUNG,
SCHIZOTYPIE UND TRANSLIMINALITÄT

ZUSAMMENFASSUNG: Die vorliegende Studie untersucht den Zusammenhang zwischen paranormaler Glaubenseinstellung und kognitiv-perzeptuellen Persönlichkeitsmaßen. Die Teilnehmer füllten eine Fragebogenbatterie bestehend aus einem Maß für paranormale Glaubenseinstellung, dem Schizotypal Personality Questionnaire (SPQ-B) und der Revised Transliminality Scale (RTS) aus. Die Punktwerte auf dem SPQ-B und der RTS korrelierten positiv mit der paranormalen Glaubenseinstellung insgesamt. Unterschiede in der Ausprägung der paranormalen Einstellung bei solchen Teilnehmern, die hoch bzw. niedrig bei den jeweiligen kognitiv-perzeptuellen Maßen abschnitten, wurden überprüft. Die über dem Median liegenden Teilnehmer zeigten höhere Zustimmungswerte auf den Subskalen für paranormale Glaubenseinstellung (Spukhäuser, Aliens, Aberglaube, außerirdisches Leben, Religion, PK, ASW, Astrologie und Hexerei) als die unter dem Median liegenden. Mittels partieller Korrelation und hierarchischer Korrelation ließ sich zeigen, dass der Hauptanteil der Varianz durch den

kognitiv-perzeptuellen Faktor, gemessen durch den SPQ-B, aufgeklärt wurde. Zusätzlich zeigte sich innerhalb des Regressionmodells, daß die RTS zusätzliche Varianz aufklärte neben der durch den kognitiv-perzeptuellen Faktor des SPQ-B.

REANALYSES OF GROUP TELEPATHY DATA WITH A FOCUS ON VARIABILITY

By Jan Dalkvist*, William Montgomery**, Henry Montgomery* and Joakim Westerlund*

ABSTRACT: Reanalyses of data from experiments on telepathic communication of emotions, as evoked by slide pictures, between groups of senders and groups of receivers are reported. In the present study, variability in performance rather than level of performance was in focus. Fits between variability in distributions of hits expected by chance and variability in empirical distributions were explored. The expected distributions were derived by means of the hypergeometric distribution, which provides the number of successes in a sequence of *n* draws from a finite population without replacement. Session level analyses showed that the variability in hit-rate was smaller than that expected by chance, particularly when the session groups who started as senders and those who started as receivers were analyzed separately and when the geomagnetic activity was low. Monte Carlo analyses indicated that these results could not be explained by stacking effects. Individual level analyses did not show any effects. In a second part of the study, the variability of responses to the individual target pictures was explored. The variability differed significantly among the pictures. Simulation showed that this effect was not attributable to stacking effects. Two predictions to be tested in an ongoing replication experiment are presented.

Keywords: telepathy, emotions, variability, hypergeometric distribution, simulation

The vast majority of ESP experiments have been performed and analyzed at the individual level. That is, data have been collected for each participant individually, and the unit of analysis has been the participant, even though the results in general have been summarized at the group level.

One reason why group experiments on ESP are relatively rare is probably the old and widespread opinion that group testing is inferior to individual testing in producing positive results (see, e.g., Rhine, 1947/1971). In line with this negative evaluation, several later studies have failed to produce any positive results (e.g., Haight, Weiner, & Morrison, 1978; Milton & Wiseman, 1999). Positive results have also been reported, however (Barker, Messer, & Drucker, 1975; Carpenter, 1988; Dalkvist & Westerlund, 1998), but attempts to replicate some of these results have failed (Carpenter, 1991; Westerlund & Dalkvist, 2004).

In any case, it would be premature to abandon group testing at this point in time. One reason is that, thus far, too few well-controlled group studies using different designs and types of ESP tasks have been tested to permit any definite assessment of the merits and drawbacks of group testing. For example, most studies have been concerned with clairvoyance or precognition and not with telepathy. Besides the above-mentioned studies by two of us (JD and JW), we know of only one group telepathy study (Auriol

et al., 2004). This long-term experimental series failed to demonstrate any deviation from chance expectation with respect to performance level, but performance variations among experiments that deviated significantly from chance expectation were found.

Another reason for continuing to use group testing is that this method is much less time-consuming than individual testing is. Thus, as long as we are not certain that group testing, in contrast to individual testing, will fail to uncover any ESP phenomena, group testing should be used for purely practical reasons. A further, less obvious, reason for not abandoning group testing in ESP research is that ESP may be critically dependent on social factors, such as the psychological atmosphere in a group of senders or receivers in a telepathy experiment.

Unfortunately, when running group experiments, one is faced with a big statistical problem, called "stacking," which probably has made many researchers refrain from doing group experiments. The problem is this: Due to the possible occurrence of dependency among participants' responses in group testing (e.g., due to the occurrence of a common response bias, such as a tendency on the part of the respondents to give one type of response at the beginning of a run and another type at the end of it), the statistical assumption of independent measures runs the risk of being violated. In general, the stacking effect acts to inflate the results by effectively reducing "*n*" in any (conventional) statistical test due to the occurrence of positive correlations among participants' responses caused by stacking (for example, when all participants invariably respond in exactly the same way, the effective *n* is reduced to one).

There are several ways of overcoming the stacking problem, however. One is by statistically correcting the data for the effects (Thoules & Brier, 1970), although this method is in general extremely laborious or uncertain, depending on the specific technique being used.

Another solution is to let the whole group of participants who have been subjected to the same experimental treatment be the measurement object in a statistical analysis, and not the individual participant, the rationale behind this method being, of course, that correlations among responses within groups become irrelevant by this procedure and that no stacking effect can occur among different groups because of the lack of communication among them. There are drawbacks to this method, however. One is practical. The method requires a considerable amount of data, that is, a large number of different groups. Another drawback is that the method is not generally applicable. It works well for analyses concerned with means or some other measure of the central tendency. However, it cannot be used reliably for analyses concerned with variability rather than the central tendency. This is because the response variation within groups, for statistical reasons, is reflected by the variation among corresponding group means (or central values of some other type). Thus, when considering, for example, the standard deviation of the means of a

performance measure for different groups of participants all of whom have been treated in the same way, we cannot tell to what extent it has been affected by within-session correlations among participants' responses that have been caused by stacking (in general, the variation will increase rather than decrease due to the occurrence of positive response correlations within the groups).

Still another possibility, which is free from any theoretical shortcoming, is to resort to a statistical simulation technique, a so-called Monte Carlo method (Dalkvist & Westerlund, 1998), where empirical data are compared to corresponding simulated data generated according to the null hypothesis using some appropriate random sampling technique and the set of empirical responses at hand. This method may, in effect, be useful as a complement to ordinary statistical methods, for example to check all significant results but omit all nonsignificant ones. By using such a selective strategy, the often time-consuming and technically demanding work required in doing simulations may be considerably reduced.

Since the spring of 1993, a series of group telepathy studies has been performed at the Department of Psychology, Stockholm University, with one of us (JD) as initiator. Based on the idea that strong emotional messages—for instance, signals of danger—may, for evolutionary reasons, be easier to transmit telepathically than are more neutral messages (Moss & Gingerelli, 1968), the studies have all been concerned with transmission of emotions as evoked by slide pictures.

As a first part of the present series of studies, five individual studies, which mainly served to generate a set of hypotheses (Dalkvist & Westerlund, 1998), were performed. These hypotheses were then tested in a comprehensive replication study (Westerlund & Dalkvist, 2004). The outcome of this study was clearly negative. However, a new finding, concerned with the order of sending and receiving telepathic messages, was obtained. To elucidate this finding, a reanalysis of previous data was carried out, leading to additional new hypotheses (Dalkvist & Westerlund, 2006). Still more new hypotheses were suggested by another reanalysis, and will be presented in the present paper. These hypotheses are concerned with variability in performance rather than with mean performance.

So far in the present project, only means have been used as a summary measure of performance. It should be borne in mind, however, that the mean (or any other measure of central tendency) describes only one particular aspect of the underlying distribution of measurements—its overall level. Another important aspect is the variability, as indicated, for example, by the standard deviation. Although a measure of the central tendency of a distribution of measurements and a corresponding measure of variability are not quite independent of each other—either mathematically or empirically (the standard deviation is, for example, most often positively related to the mean)—a measure of variability

may provide useful information over and above that provided by the central measure, as demonstrated, for example, by recent research on performance as related to aging (e.g., MacDonald, Nyberg, & Bäckman, 2006) and ADHD (Söderlund, Sikström, & Smart, 2007). Nevertheless, the specific information provided by the standard deviation (or some other measure of variability) is often neglected.

In some parapsychological contexts, variability in performance rather than the level of performance has been in focus. For example, in research on the decline effect, Carpenter found a decreasing run score variance, meaning that run scores started out either high or low at the beginning of the session but approached chance as the session progressed (e.g., Carpenter, 1966, 1968, 1969; Carpenter & Carpenter, 1967).

Another context in which the concept of variability has been considered is meta-analysis when tests are made to see whether different data sets in a large database are more heterogeneous (have greater variability) than expected by chance. If so, based on the assumption that deviating data sets tend to be less reliable than nondeviating ones due to systematic errors, deviating data sets are often discarded to make the database more homogeneous and therefore (it is assumed) more reliable (e.g., Honorton & Ferrari, 1989).

This procedure may be questioned, however. The argument is that greater heterogeneity than expected by chance may reflect real (i.e., parapsychological) effects rather than systematic errors, meaning that reducing the heterogeneity by discarding deviating data amounts to eliminating—or at least reducing—the very effects under study. There may, for example, be a bidirectional effect involved: While one part of the distribution may contain real hits, the opposite part may contain data resulting from psi-missing, that is, a reversed response pattern turning hits into misses in a systematic manner. For example, the finding of greater variation in hit-rate than expected by chance in ganzfeld data (e.g., Storm & Ertel, 2001) may be taken as evidence of a bidirectional effect, involving both hits and misses not expected by chance alone. Thus, rather than interpreting greater variability than expected by chance as a sign of errors, it can preferably be seen as suggestive of real effects, at least initially.

However, not only *greater* variability than expected by chance but also *lesser* variability than expected by chance may be taken as suggestive of a genuine effect. Such reduced variability may be expected to occur, for example, in very successful studies, where participants consistently perform at a high level. Conversely, reduced variability may also be expected to occur in studies where psi-missing occurs consistently. In either case, reduced variability is paired with a deviating central measure—a high one in a successful study and a low one when psi-missing predominates.

The main purpose of the present study was to reanalyze data from the above-mentioned group telepathy studies performed by two of us (JD and JW)—but now looking at variability.

Two different types of analysis were performed. One involved comparison between the empirical interindividual variability in hit-rate and the corresponding expected theoretical variability. The other type of analysis concerned the question of whether the stimulus targets differed from each other in interindividual response variability.

Before considering the present study, we will give a brief overview of the previous studies.

Previous Studies

The Original Studies

A total of 337 participants, 222 females and 115 males, with a mean age of 27 years, took part in the five original studies (Dalkvist & Westerlund, 1998). Most of the participants were undergraduate psychology students at Stockholm University, who chose to participate in the study as part of course requirements.

The studies comprised 24 single experiments in all, the number of experiments per study varying from two to nine. The mean number of participants per experiment was approximately 14.

As stimuli, 30 slide pictures were used, 15 with positive motifs (such as nature pictures and pictures of happy people) and 15 with negative ones (such as pictures of traffic accidents and starving children).

When the participants arrived at the laboratory, they were randomly divided into two groups, one sender group and one receiver group. The senders and the receivers were sequestered in two soundproof rooms, with one room in between. The two experimental rooms were connected by a signal device: a lamp in the receiver room that could be turned on and off from the sender room. There were two experimenters in the sender room and two in the receiver room.

The slides were presented in random orders, a new order for each group of senders. The senders' only task was to look at the pictures and to "hold on to" the feelings evoked by the respective pictures as long as they were being shown. The receivers were instructed to guess whether a given picture was positive or negative (they were informed about the number of slides, but not that the number of positive and negative pictures was the same). One of the experimenters in the receiver room watched the signal lamp and reported to the receivers when a new picture was being shown to the senders. Each picture was shown for 20 seconds, with an interstimulus interval of about half a second.

When all 30 pictures had been shown, the participants changed rooms, and those who had served as senders now served as receivers and vice versa.

Hit-rate, defined as number of correct responses or proportion of correct responses (when stimulus data were analyzed), was invariably used

as the dependent variable in the data analyses. Hit-rate was analyzed as a function of various personal and other factors.

The Replication Study

On the basis of the results of the five above studies, a number of predictions were formulated and tested in the replication study (Westerlund & Dalkvist, 2004). These predictions were all based on statistically significant (or, in one case, marginally significant) results obtained when data from the five studies were combined.

The new study was an exact replication of the latest of the five original studies, except that two additional minor control measures were adopted.

The replication study comprised 432 females and 173 males, 605 participants in all, with a mean age of 27 years. As before, the large majority of the participants were undergraduate psychology students at the Department of Psychology at Stockholm University, who chose to participate in the study as part of course requirements.

A total of eight predictions were tested. None of them was borne out (Westerlund & Dalkvist, 2004), which strongly argued against the possibility that some psi-phenomenon had been at work.

In spite of this failure, in a post hoc analysis, two physical moderator variables were entered: (a) local sidereal time (LST), an astronomical time and space measure, which is indirectly related to the magnitude of cosmic radiation that reaches the earth, and (b) disturbances in the global geomagnetic field, as measured by the *ap*-index. For a large number of different studies, performed in the northern hemisphere, Spottiswoode (1997) found both of these measures to be systematically related to the effect size of the studies. Our failures to replicate the previous positive results could not be explained in terms of differences in LST or *ap*-index, however.

A Follow-up Analysis

Although none of the eight predictions were born out, a significant unexpected result was obtained. In the original studies, a significant interaction effect was obtained between gender and receiver order, with an average hit-rate above expectation for the males when they started as receivers and an average hit-rate above expectation for the females when they started as senders. This interaction effect was not replicated in the follow-up study. Instead, a significant main effect of sender/receiver order was obtained, with a significant negative deviation from mean chance expectation for participants who started as receivers and a nearly significant positive deviation for those who started as senders.

Admittedly, this result was not predicted to occur, and as many as eight different predictions were tested in the study, meaning that the result did not reach significance when correction was made for number of tests. Nevertheless, inspired by earlier reports of effects of sender/receiver order in ganzfeld research (Haraldsson, 1980, 1985), we decided to carry out a follow-up analysis of the present sender/receiver order effect, based both on the original data set (with the first three studies, which were less well controlled, removed) and on data from the replication study (Dalkvist & Westerlund, 2006). In the following, we will collectively refer to data from the last two studies in the initial series of studies as the "old data" and data from the replication study as the "new data."

The analyses were not carried out at the individual level, as before, but at the group level. Specifically, each session (one of the two parts of a single experiment) was used as the unit of analysis, and session means were used as input in the statistical analyses, mainly to avoid the stacking effect, as discussed in the introduction.

Was the sender/receiver order effect obtained in the new study a real effect or was it attributable to sampling errors? In an attempt to answer that question, a systematic series of data analyses were carried out.

An initial finding was that the discrepancy between the old and the new data sets apparently could be explained in terms of geomagnetic fluctuations, which were much larger in the old study than in the new one. Moreover, the *ap*-index not only seemed to explain the difference in the sender/receiver order effect *between* the old and the new study, but also the sender/receiver order effect *within* each of the two data sets. Thus, independent of data set, the sender/receiver order effect turned out to be negatively related to the *ap*-index, although not significantly so in the case of the old data set.

As indicated by a step-wise multiple regression analysis on the whole data set, one additional variable was significantly related to the sender/receiver order effect, namely, a response style variable: number of negative guesses, which in contrast to the *ap*-index was positively related to the sender/receiver order effect. This relationship was weaker than that for the *ap*-index, however.

The Present Study

Interindividual Analyses

Data. Exactly the same data as those used in the above-mentioned study on the sender/receiver order effect (Dalkvist & Westerlund, 2006) were analyzed. Calculations were made for the old and the new study separately, as well as for both studies combined. The number of participants, sessions, and experiments over the old, the new, and the total data set are given in Table 1.

Table 1
Number of Participants, Sessions and Experiments
Over Three Data Sets

	Data		
Units	Old[a]	New	Total
Participants	240	605	845
Sessions	34	90	124[b]
Experiments	17	47	64

[a]The last two out of five studies.
[b]Four sessions were discarded because of technical failures.

In between-subjects analyses, the data were also divided into two subsets based on the activity of the geomagnetic field (GMF), one with high activity, as measured by the *ap*-index, and the other with low activity. The subset with high GMF activity included all experiments with a value above the median value plus a sample of 50% of the experiments falling exactly at that value. The subset with low GMF activity included all experiments with a value below the median value plus the remaining 50% of experiments falling exactly at that value.

Empirical Versus Theoretical Interindividual Variability in Hit-rate

General method. For each data set, analyses were carried out both at the individual level (for comparison purposes) and at the session level. To follow up the sender/receiver order effect, mentioned above, separate analyses were carried out for participants starting as senders and for participants starting as receivers.

Also, to eliminate errors associated with particular experiments, and thereby increasing the power of the analyses, in addition to analyzing session means, analyses were also conducted using the corresponding residuals around the means of the experiments (deviations of session means from the mean of any single experiment).

The general strategy was to compare empirical distributions of hit-rate with corresponding theoretical distributions, which assume that only random factors were at work, to see whether the two distributions differed from each other in variability. Comparisons between empirical and theoretical distributions were made using *F* tests. Because the expected distributions of hits would be obtained from an infinite number of respondents, the size of the data set expected by chance was assumed to be infinite; accordingly, an extremely high corresponding *df* value (10^6) was used in the *F* tests.

A theoretical distribution could not be constructed in a straightforward way, however, due to the procedure used in randomizing the stimuli, that is, sampling *without* replacement. If sampling *with* replacement had been used, the theoretical distribution would have been possible to obtain directly using the binominal theorem (although this method would have yielded less sensitive data, because the distribution of positive and negative pictures would then generally not have been optimal for discriminating between positive and negative stimuli, that is, containing the same number of positive and negative stimuli). However, as sampling without replacement was used, the empirical distribution became dependent on the number of positive and negative answers of each individual respondent. This problem was overcome by applying an appropriate algorithm, based on the so-called hypergeometric distribution, which can be used as a substitute for the binominal distribution when samples are drawn without replacement. However, because the participants' responses were assumed to be uncorrelated, possible stacking effects were not incorporated into the model. This problem was addressed by checking all positive results using simulations.

A computer program was written, in Java, to create the present type of distribution.

Theoretical distributions for individual data. The hypergeometric distribution is a discrete probability distribution that provides the number of successes in a sequence of n draws from a finite population without replacement. A typical example is the following: There is a shipment of N objects in which D are defective. The hypergeometric distribution gives the probability p that in a sample of n distinctive objects drawn from the shipment exactly k objects will be defective (Wikipedia, 2005).

The formula for the hypergeometric distribution may be written as follows:

$$p = \frac{D!(N-D)!n!(N-n)!}{N!k!(D-k)!(n-k)!(N-D-n+k)!} \qquad (1)$$

In the present case, the population is the 30 stimulus pictures (N), 15 of which are positive (D) and 15 negative (N-D). The parameter n is the number of responses in the less frequent response category (positive or negative responses). For example, if a participant has given 17 positive and 13 negative responses, n is equal to 13. The parameter k is the number of hits among the n responses in the less frequent response category. For example, if n = 13 and k = 7, there are 7 hits among a total of 13 minority responses. Insertion of these values into Equation 1 yields a p value of 0.27 of getting exactly 7 hits among 13 responses, all of which are negative (or positive if the positive responses are in minority).

Now, once n and k are known, the number of hits in the total set of N responses can be calculated. To show how by means of an example, let us assume again that $n = 13$ and $k = 7$. Let us further assume, as before, that the minority responses are negative. The 7 hits in response to negative stimuli then imply that the remaining 8 negative stimuli (15-7) are to be found among the 17 cases where the participant gave a positive response. The responses to these 8 negative stimuli will then be misses, whereas the remaining 9 positive responses will be hits. Thus, the total number of hits will be 7 + 9 = 16. More generally, the total number of hits (H) can be calculated from the following formula:

$$H = k + (N - n) - (D - k) \qquad (2)$$

Thus, by using the above two formulas, given the number of positive and negative responses, the expected probability for each possible number of hits can be calculated. Specifically, this is done by calculating p and H for each possible value of k for a given n. In the extreme case of $n = 0$, k is equal to 0, giving a hit-rate of $H = 15$ with a chance probability of 1, meaning that the only possible hit-rate is 15. The largest number of possible values of k and the largest number of different hit-rates is obtained when $n = 15$, giving a maximal hit-rate of $H = 30$, with a chance probability of $6.45 * 10^{9}$. In general, the possible number of different hit-rates, and hence the variability in hit-rate, decreases progressively as n decreases.

In constructing a probability distribution for a group of participants at the individual level, as a first step, a specific hit-rate distribution was constructed for each participant separately based on his or her number of negative/positive responses. Each such distribution gives a probability between 0 and 1 for each possible hit-rate to occur, with a total sum of 1. All individual distributions were then merged by summing the individual probabilities for each possible hit-rate. The distribution thus obtained was taken to be the expected probability distribution of hit-rates for the whole group.

Theoretical distributions for group data. In analyzing data at the group level, calculations were made to test whether the empirically obtained distribution of mean number of hits for groups of participants (for example, the groups that started as senders or the groups that started as receivers in each experiment) differed in variability from the corresponding distribution of mean hit-rates that would be expected if only random factors were at work.

In principle, such a theoretical probability distribution could have been obtained by combining every individual distribution in the group with all other distributions in the group to form an "average" distribution for the whole group. However, such a direct method would have required a great amount of computer time, because all possible hit-rates for one individual would have to be combined with all possible hit-rates for all other individuals.

To overcome this problem, a more effective method was developed. Theoretical probability distributions were thus constructed by using a procedure in which Equations 1 and 2 were applied recursively for a successively larger number of participants in a given group until a distribution of mean hit-rates for the whole group had been computed. As will be shown later, the procedure satisfies the necessary condition of giving the same results independent of the order in which data from the participants are entered into the calculations.

To calculate the expected probability distribution of mean number of hits for two arbitrarily chosen participants in the group, the probability distributions $p(H_1)$ and $p(H_2)$ across all numbers of hits H for participants 1 and 2, respectively, were calculated using Equations 1 and 2, as described above. A combined probability distribution for the two participants was then obtained by calculating the product $p(H_1)^* p(H_2)$ for all possible pairs of $p(H_1)$ and $p(H_2)$ for which both p values were greater than zero. Finally, a probability distribution across all different means of hit-rates H_1 and H_2 that could be formed was computed.

This computational procedure is illustrated in the following simple example involving only four hit possibilities, resulting from the low values of n. The hit probabilities greater than zero are assumed to be 0.24 for 13 hits, 0.52 for 15 hits, and 0.24 for 17 hits in Distribution 1 (would be true in the present case if $n = 2$), and .50 for 14 hits, and .50 for 16 hits in Distribution 2 (would be true in the present case if $n = 1$). The combined probability distribution for all pairs of hits in all six possible pairwise combinations of $p(H_1)$ and $p(H_2)$ will then be as follows:

.121 (13, 14)
.121 (13, 16)
.259 (15, 14)
.259 (15, 16)
.121 (17, 14)
.121 (17, 16)

and the corresponding probability distribution for the *means* of all 6 pairs of hits

.121 (13.5)
.121 (14.5)
.259 (14.5)
.259 (15.5)
.121 (15.5)
.121 (16.5)

(For example, the combined probability for the combination of 13 hits and 14 hits is 0.50 * 0.24 = 0.12, which is also the probability of the *mean* of 13 hits and 14 hits, that is, 13.5 hits.)

Finally, the probability distribution as a function of mean hit-rate is calculated by computing the sum of probabilities across all cases with the same mean hit-rate:

.121 (13.5)
.380 (14.5)
.380 (15.5)
.121 (16.5)

(For example, the probability for the mean hit-rate 14.5 is 0.121 + 0.259 = 0.380.)

To calculate the results for a subgroup (or whole group) that includes an additional third participant, the probability distribution of mean number of hits that already had been calculated for two of the participants was combined with the distribution calculated for the third participant using Equations 1 and 2, as before. When calculating the mean hit-rates for the new group of three participants, the distribution of mean hits for two participants was multiplied by two, because this distribution is based on twice as many participants as in the distribution for the single participant. Thus, mean hits were calculated according to the equation

$$M(H_1,H_2,H_3) = ((M(H_1, H_2)*2) + H_3)/3 \qquad (3)$$

where M is the arithmetic mean.

The probability for each $M(H_1,H_2,H_3)$ was calculated by combining the probabilities for the two distributions being used in the same way as when two single participants were combined. That is, we first calculated $p(H_1,H_2)*p(H_3)$ for all possible pairs of $p(H_1,H_2)$ and $p(H_3)$ for which p was greater than zero and then a probability distribution across all different means of hits that could be formed.

For groups with still higher numbers of participants, the procedure described above was applied recursively for each additional number of participants in the group. Generally, the mean hit-rates for a group of n participants was calculated according to the formula

$$M(H_1, \ldots, H_n) = ((M(H_1,\ldots,H_{n-1})*n\text{-}1) + H_n)/n \qquad (4)$$

and the probability associated with each $M(H_1,\ldots,H_n)$ by combining the probabilities for the distributions of H_1, $\ldots,H_{n-1}$ and H_n, respectively, as described above.

That the right-hand side of Equation 4 is indeed equal to $M(H_1, \ldots,H_n)$ follows from the fact that it can be reduced to $(H_1+\ldots+H_n)/n$, that is, the general expression for $M(H_1,\ldots,H_n)$. Obviously, this will be true independent of the order in which the participants are selected in the recursive procedure being used. Thus, Equation 4 will produce all possible $M(H_1,\ldots,H_n)$ for any order in which the participants are selected.

The final distribution was constructed in two steps: First, each theoretical distribution's probabilities were multiplied by the number of participants in the corresponding group of participants, to give a weight to each distribution in accordance with the number of participants. Second, all distributions were added together to form a total expected distribution.

As mentioned before, tests were also performed using residuals around the experiment mean instead of session means, to eliminate variation in hit-rate among experiments. Thus, an algorithm was also written to generate all possible results of residuals with respect to experiments (the differences of the results of the two session groups from the average results of the experiments) and calculating the probabilities of these results. The expected distribution of residuals obtained by chance was calculated in the following way: The probability of each pair of group results was calculated by multiplying the probabilities of the group results. The result of each pair was then added to the frequency of the residual result, which was equal to the average of the group results.

Analyses and results. The results of the comparisons between observed and expected variability for the old study are shown in Table 2. As can be seen from this table, there was no indication of there being any difference in variability between the empirical and the corresponding theoretical distributions. Thus, most of the *F* ratios obtained were close to one, and there was only one significant *F* ratio: that for the individual respondents starting as receivers, who showed a lower observed variation in hit-rate than was expected by chance.

Table 2
Comparisons Between Observed and Expected Variability, Old Data

Source of variation	*F*	Observed *SD*	Expected *SD*	*df*	*p*[a]
Individuals, total set	1.002	2.75	2.75	239	.960
Individuals, first receivers	1.320	2.39	2.75	122	.041
Individuals, first senders	1.260	3.10	2.76	116	.074
Means of sessions, total set	1.110	1.02	1.08	33	.634
Means of sessions, first receivers	1.490	0.88	1.08	16	.080
Means of sessions, first senders	1.110	1.17	1.11	16	.780
Residuals of sessions, total set	1.130	0.72	0.76	16	.824
Residuals of sessions, first receivers	1.140	0.72	0.77	16	.806
Residuals of sessions, first senders	1.140	0.72	0.77	16	.806

[a]Two-tailed

The results of the comparisons between observed and expected variability for the data from the new study are shown in Table 3. As can be seen from this table, there was a tendency for the empirical variability to be smaller than the theoretical variability—but only at the session level. At that level, two of the three *F* ratios obtained using ordinary means were significant: that for the total set of sessions and that for the sessions with participants starting as senders. The strongest results, however, were obtained for the residuals of the two sender/receiver order sessions. Taken separately, the results for the residuals of the sessions with participants starting as receivers and the sessions with participants starting as senders were highly significant. But the results for the residuals were also significant for all sessions taken together, although less strongly so. At the individual level, however, all of the three *F* ratios were close to zero and nonsignificant.

Table 3
Comparisons between Observed and Expected Variability, New Data

Source of variation	*F*	Observed *SD*	Expected *SD*	*df*	*p*[a]
Individuals, total set	1.05	2.68	2.75	604	.386
Individuals, first receivers	1.05	2.68	2.75	324	.575
Individuals, first senders	1.08	2.65	2.76	279	.368
Means of sessions, total set	1.43	0.95	1.13	89	.029
Means of sessions, first receivers	1.25	0.99	1.10	46	.344
Means of sessions, first senders	2.12	0.80	1.17	42	.003
Residuals of sessions, total set	1.76	0.60	0.80	42	.021
Residuals of sessions, first receivers	2.28	0.53	0.80	42	.001
Residuals of sessions, first senders	2.28	0.53	0.80	42	.001

[a]Two-tailed

The results of the comparisons between observed and expected variability over both data sets are shown in Table 4. The results shown in this table exhibit about the same pattern as the results for the data obtained in the new study. At the session level, however, the present *p* values are larger than those for the new study for all but one *F* ratio (between groups, first receiver), reflecting the high *p* values obtained at the session level in the old study.

Table 4
Comparisons Between Observed and Expected Variability, Both Data Sets

Source of variation	*F*	Observed *SD*	Expected *SD*	*df*	*p*[a]
Individuals, total set	1.03	2.70	2.75	843	.497
Individuals, first receivers	1.11	2.60	2.74	447	.132
Individuals, first senders	1.03	2.80	2.76	395	.674
Means of sessions, total set	1.30	0.97	1.11	123	.051
Means of sessions, first receivers	1.29	0.96	1.09	63	.191
Means of sessions, first senders	1.42	0.96	1.14	59	.084
Residuals of sessions, total set	1.54	0.63	0.79	59	.034
Residuals of sessions, first receivers	1.69	0.61	0.79	59	.011
Residuals of sessions, first senders	1.69	0.61	0.79	59	.011

[a]Two-tailed

In order to test whether the positive results obtained when comparing observed and theoretically expected variability at the session level were due to a stacking effect (or faulty calculations), a Monte Carlo method was used. Specifically, standard deviations were repeatedly recalculated for 100 simulated studies, obtained by substituting each original stimulus order in each study for a new randomized stimulus order. If stacking effects did occur they would become incorporated into the simulated data through the participants' responses (but not into the theoretical data, because the participants' responses were assumed not to be correlated in the present theoretical model). Hence, *p* values that were unaffected by stacking effects could be obtained by inserting empirical standard deviations into corresponding sampling distributions of simulated standard deviations.

Table 5 shows the *p* values obtained in this way for the whole data set and for the new one. As can be seen by comparing Table 5 with Tables 3 and 4, there was a very good agreement between the *p* values obtained by simulation and the corresponding *p* values obtained by comparing observed and theoretically expected variability. Thus, the possibility that the significant results obtained by comparing observed and theoretical variability at the session level were due to stacking effects could be refuted.

Table 5
P Values for Empirical Between-Session Standard Deviations of Mean Hit-Rates for the New and the Total Data Set Obtained by Monte Carlo Analyses Using 100 Simulated Studies

Source of variation	Data	
	Total	New
Means of sessions, total set	.03	.03
Means of sessions, first receivers	.05	.12
Means of sessions, first senders	.08	< .01
Residuals of sessions, total set	.01	< .01
Residuals of sessions, first receivers	.01	< .01
Residuals of sessions, first senders	.01	< .01

The results of the comparisons between observed and expected variability for the data associated with low activity of the earth's magnetic field are shown in Table 6. These results are very similar to those obtained in the new study. At the session level, the observed variation is thus smaller than expected by chance, particularly for the residuals of the two separate sessions, but not at the individual level.

Table 6
Comparisons Between Observed and Expected Variability, Low Geomagnetic Activity

Source of variation	*F*	Observed *SD*	Expected *SD*	*df*	*p*[a]
Individuals, total set	1.02	2.72	2.75	406	0.770
Individuals, first receivers	1.09	2.64	2.76	221	0.372
Individuals, first senders	1.02	2.77	2.75	184	0.908
Means of sessions, total set	1.50	0.91	1.12	61	0.048
Means of sessions, first receivers	1.42	0.94	1.12	32	0.215
Means of sessions, first senders	2.53	0.72	1.14	28	0.003
Residuals of sessions, total set	1.62	0.62	0.79	28	0.112
Residuals of sessions, first receivers	2.72	0.48	0.80	28	0.002
Residuals of sessions first senders	2.72	0.48	0.80	28	0.002

[a]Two-tailed

The results of the comparisons between observed and expected variability for the data associated with high activity of the earth's magnetic field are displayed in Table 7. There is a marked difference between these results and those for the data associated with low geomagnetic activity, just considered. There is still a tendency for the observed variation in hit-rate to be smaller than expected by chance, but this tendency is weaker than the tendency in the data for low geomagnetic activity, as indicated, for instance, by the lack of any significant *F* ratio.

Table 7
Comparisons Between Observed and Expected Variability, High Geomagnetic Activity

Source of variation	*F*	Observed *SD*	Expected *SD*	*df*	*p*[a]
Individuals, total set	1.05	2.69	2.75	436	0.511
Individuals, first receivers	1.14	2.56	2.74	224	0.173
Individuals, first senders	1.04	2.82	2.76	211	0.672
Means of sessions, total set	1.16	1.03	1.11	61	0.460
Means of sessions, first receivers	1.26	0.97	1.08	30	0.439
Means of sessions, first senders	1.08	1.11	1.15	30	0.822
Residuals of sessions, total set	1.48	0.65	0.79	30	0.183
Residuals of sessions, first receivers	1.47	0.65	0.79	30	0.185
Residuals of sessions, first senders	1.47	0.65	0.79	30	0.185

[a]Two-tailed

Using the same 100 simulated studies as above, Monte Carlo simulations were also performed to test the possibility that the difference between the low and the high geomagnetic activity results were due to stacking effects. As can be seen from Table 8, this possibility could be refuted.

Did the between-session standard deviations for the simulated studies differ from the corresponding theoretical standard deviation? To get an answer to that question, one-sample *t* tests were conducted as indicated in Table 9. As can be seen from this table, neither the means nor the residuals in any of the two data sets exhibited a significant difference. Thus, if there was any stacking effect, this effect was not large enough to affect the variability at the session level to a discernible extent.

Table 8

P Values for Empirical Between-Session Standard Deviations of Mean Hit-rates for Sessions With Low and High Geomagnetic Activity, Respectively, Obtained by Monte Carlo Analyses Using 100 Simulated Studies

Source of variation	Geometric Activity	
	Low	High
Means of groups, total set	.02	.17
Means of groups, first receivers	.10	.21
Means of groups, first senders	< .01	.34
Residuals of groups, total set	.04	.13
Residuals of groups, first receivers	<0.01	0.13
Residuals of groups, first senders	<0.01	0.13

Table 9

One Sample *T* Test of the Difference Between the Theoretical Between-Session Standard Deviation and the Mean of Between-Session Standard Deviations for 100 Simulated Studies

	New Data			
	SD (theoretical) - Mean *SD* (simulation)	*t*	*df*	*p*[a]
M	1.13 - 1.13 = 0.00	-0.05	99	.964
Residuals	0.80 - 0.79 = 0.01	-1.18	99	.239
	Both Data Sets			
	SD (theoretical) - Mean *SD* (simulation)	*t*	*df*	*p*[a]
M	1.11 - 1.12 = -0.01	0.98	99	.330
Residuals	0.79 - 0.78 = 0.01	-1.23	99	.222

[a]Two-tailed

Why did the session level analyses predominantly yield significantly smaller-than-expected variability in hit-rate while the individual level analyses did not? One possible explanation is that the smaller-than-expected between-session variation was compensated for by larger-than-expected within-session variation. Comparisons between theoretical and empirical within-session distributions contradicted this explanation, however.

Another possible explanation assumes that sessions differed with respect to their internal variability in hit-rate such that larger session groups had greater internal variability than did smaller session groups. In that case, the individual level variability would be larger than the session level

variability, because individuals in larger session groups would get lower weights than individuals in smaller session groups when the variation among session means was calculated. According to this explanation, there should be a positive correlation between a measure of the within-session variability in hit-rate and the size of the session group for the new and the total data set, but not for the old one, where no clear-cut difference between the individual level analyses and the session level analyses was obtained. As can be seen from Table 10, this prediction was borne out: Significant positive correlations were obtained between the standard deviation of the hit-rate scores within sessions and the number of participants in the session group.

TABLE 10
PEARSON CORRELATIONS BETWEEN NUMBER OF RECEIVERS AND WITHIN-SESSION STANDARD DEVIATIONS FOR THE OLD, NEW, AND TOTAL DATA SETS

Data set	*r*	*df*	*p*[a]
Old	-.10	32	.564
New	.33	88	.002
Total	.21	122	.021

[a]Two-tailed

To test whether the significant correlations between number of receivers and within-session standard deviation shown in Table 10 were genuine or attributable to some kind of stacking effect, the two correlations were repeatedly recalculated using, again, each of the 100 simulated data sets. For the new data set, 5 out of the 100 correlations were found to be larger than the empirical correlation ($r = .33$). That is, according to the present Monte Carlo simulations, the empirical correlation for the new data set was marginally significant. For the whole data set, however, as many as 12 of the simulated correlations turned out to be larger than the empirical correlation ($r = .21$), which thus did not reach significance. These findings indicate that an artifact, probably a stacking effect that was positively related to the number of participants in the session, did occur. Nevertheless, the almost significant simulated correlation for the new data set gives some support to the interpretation that sessions differed with respect to their internal variability in hit-rate such that larger session groups had greater internal variability than smaller ones.

Stimulus Target Analyses

General method. Thus far, we have focused on variability in overall performance, that is, general hit-rate, to see whether and how this variability differed from theoretical expectations under the assumption that only random factors were at work. In the present part of the study, we have

instead focused on variability in responses to the individual target pictures, to investigate whether and how the variability differed among them.

Interindividual response variability was measured at the session level. The receivers' responses were coded binarily. Specifically, a guess that a picture presented to the senders was positive was coded as "0" and a guess that the picture was negative as "1." For each target picture and session, the variability of responses was taken to be the standard deviation of type of guess (positive or negative). This measure takes on its highest value (= .50) when the numbers of positive and negative guesses are equal and its lowest value (= 0) when all guesses are either positive or negative.

According to the null hypothesis, the 30 target pictures do not differ with respect to variability. Using the above measure of response variation, this hypothesis can be tested statistically, using analysis of variance (ANOVA). (If the response variability had been measured on whole data sets instead of subsets, no such test could have been made, due to the lack of any error estimate.)

Analyses and results. To test whether the 30 stimulus pictures could be discriminated from each other in terms of the mean within-session standard deviation of positive and negative guesses, a one-way repeated measures ANOVA was performed with pictures as the independent variable and the standard deviation of the within-session guesses as the dependent variable for the old, the new, and the total data set. As can be seen from Table 11, a significant picture effect was obtained for the new data set and a nearly significant picture effect for the total data set, but no effect at all for the old one.

Table 11
Results From a One-way Repeated Measures ANOVA With Pictures as the Independent Variable and the Standard Deviation of Within-session Guesses as the Dependent Variables for the Old, New, and Total Data Sets

Study	*F*	*df*	*p*[a]
Old	1.01	29, 957	.449
New	1.59	29, 2581	.024
Total	1.37	29, 3567	.087

[a]Two-tailed

It is of some interest to note that tests corresponding to those above using ordinary session means instead of within-session standard deviations did not show any positive results at all.

Table 12 shows the results of simulating the above ANOVA analysis using one single simulated study. As can be seen, there was not even a tendency for the stimulus pictures to differ in any of the three data sets, thus

negating the possibility that the positive results shown in Table 11 were due to a stacking effect. (Given the absence of any effects at all in the simulated study, it was not worthwhile, we thought, to spend all the necessary time and effort to perform a full Monte Carlo analysis.)

TABLE 12
RESULTS FROM A SIMULATED ONE-WAY REPEATED MEASURES ANOVA WITH PICTURES AS THE INDEPENDENT VARIABLE AND THE STANDARD DEVIATION OF WITHIN-SESSION GUESSES AS THE DEPENDENT VARIABLES FOR THE OLD, NEW, AND TOTAL DATA SETS

Study	*F*	*df*	*p*[a]
Old	0.97	29, 957	0.512
New	0.63	29, 2581	0.937
Total	0.69	29, 3567	0.895

[a]Two-tailed

DISCUSSION

Empirical Versus Theoretical Interindividual Variability

In comparing empirical and theoretical interindividual variability in hit-rate, the strongest results were obtained when analyzing the residuals for the sessions with respect to the mean hit-rate of the corresponding experiment. Thus, using these residuals, for both the new and the total data set, the observed variability in performance was found to be significantly smaller than expected by chance in all three analyses, with a very low *p* value for the two separate sets of groups in the new data set. Similar results were obtained in the session level analyses for the new and the total data set using ordinary means, although some tests did not reach significance in that case. Monte Carlo simulations of the positive results indicated that these results could not be explained by the occurrence of stacking effects. Taken together, the findings suggest that the present analyses have revealed something interesting.

The present findings are related to previous results, showing a mean performance difference between groups of subjects starting as senders and groups of subjects starting as receivers (Dalkvist & Westerlund, 2006). The fact that the variability tended to be smaller than expected by chance in the present session level analyses sheds some light on this finding. Thus, the smaller-than-expected variability observed in the session level analyses indicates that the mean difference in hit-rate between the two sets of sessions is associated not with an increase in the variability among all groups, as might have been expected, but rather with *decreased* variability within each

of the two sets of sessions, reflecting the occurrence of coherence within each set of sessions.

The fact that residuals gave more strongly significant results than did the original session means reflects the fact that the mean hit-rates of the two sender/receiver order session groups were positively correlated across experiments in the new and the total data set. In the previous study just mentioned (Dalkvist & Westerlund, 2006), this correlation was utilized by using a paired samples *t* test instead of an ordinary *t* test to increase the power in comparing the two sender/receiver orders. In the present study, the correlation between the hit-rates of the two sender/receiver order session groups was instead utilized by eliminating the variation in performance among experiments using residual analysis. In principle, the two methods to decrease the error variance are analogous.

In contrast to the new and the total data set, the old data set did not yield any significant results at the session level. Again, this is in agreement with the preceding study (Dalkvist & Westerlund, 2006), which did not show any mean performance difference between the session groups starting as senders and the session groups starting as receivers in the old data set. As mentioned before, in that study, the difference between the old and the new data set could be related to variation in the geomagnetic activity (as was true in the present study as well), the old data being associated with greater geomagnetic variation than the new data. In accordance with that finding, only the data set associated with a low level of geomagnetic activity showed any significant results in the present study. This dependence of the results on the geomagnetic activity in both studies lends some credibility to the results.

In contrast to the session level analyses, analyses at the individual level did not generally show any significant results. This difference is puzzling, because the greater random variation at the individual level should, theoretically, be compensated for by a larger number of degrees of freedom and thereby yield equally powerful tests as those at the session level. This paradox could apparently be resolved, however, based on the fact that the within-session variability increased with the size of the session group in the new and the total data sets, even though this correlation apparently was partly due to a stacking effect. Given the positive correlation between the intrasession variability and group size for the new and the total data set, it follows logically that, for these two data sets, the between-session variability will be smaller than the within-session variability, because smaller weights will be assigned to individuals in larger groups than to individuals in smaller ones when session means are compared.

The above results obtained by comparing observed and theoretical variability are sufficiently strong to justify continued research. Specifically, in an ongoing replication study, we will test the following prediction:

> *Prediction 1.* When analyzed at the session level, the data will show smaller between-session variability in hit-rate

> than expected by chance for sessions with the same sender/receiver order, at least when the session means are replaced with the residuals calculated around the experiment mean.

This prediction will be tested using the same methods as those used in the present study.

Before turning to the target picture analyses, a comment should be made on the method used in the above analyses. Contrary to what many people believe, the methodological standards in parapsychology are in some respects higher than in other, comparable fields, for example, in using blind and double-blind protocols more often than is common in these fields (Sheldrake, 1999). Moreover, parapsychology has also contributed to the development of new methods. One example is the finding of a new statistical bias occurring when averages of responses affected by expectancies in some types of experiments are calculated (Dalkvist, Westerlund, & Bierman, 2002; Wackermann, 2002) and the suggestion of methods to avoid this bias (Dalkvist & Westerlund, 2006). The present method of constructing theoretical distributions based on the hypergeometrical distribution when sampling without replacement is used constitutes another example. When the method is used in group studies, however, one must make sure in some way that the results are not affected by stacking effects, as the model underlying the method assumes that the participants' responses are statistically independent.

In group studies, the present method of constructing theoretical distributions based on the hypergeometrical distribution should ideally be combined with a simulation method. Such a method guarantees that a statistically significant result is not caused by stacking, and can therefore, in contrast to the hypergeometrical method, also be used alone without any test of the stacking effect. However, simulations alone do not show whether or not a stacking effect does occur, and do not give any information about such an effect if it really does occur. But by comparing the two methods, it is possible both to establish and to characterize a stacking effect in terms of its strength and other properties of interest (for example, whether participants' responses tend to be positively or negatively correlated).

Of particular interest would be to investigate whether, and to what extent, the stacking effect is caused by response bias, as conventionally assumed, or reflects some genuine parapsychological effects arising within the group of receivers. This issue could perhaps be addressed by comparing receivers who are isolated from each other in time or space with receivers working in the same room at the same time using both simulation and the hypergeometrical method.

Interindividual Response Variability as Related to Target Pictures

Positive results were also obtained when response variability was related to target pictures using one-way repeated ANOVA, and the possibility that this finding was caused by a stacking effect was effectively ruled out by comparison with a simulated study. Accordingly, the following prediction was made for testing in the ongoing replication experiment:

> *Prediction 2.* A repeated measures ANOVA will show the 30 stimulus pictures to differ with respect to within-session variability in responses, as indicated by the mean standard deviation of the within-session type of response (positive or negative guess).

General Considerations

Taken together, from a strict empirical perspective, the results presented here are quite impressive. Had they been obtained in a mainstream study, one would surely expect at least some of them to be replicable. There is also some theoretical support for the present findings, however, namely from Carpenter´s (2004a, 2004b) recent first sight model, according to which psi phenomena emanate from deep unconscious processes. Most notably, consistent with this model, the differences in performance between participants starting as senders and participants starting as receivers, resulting in relatively low variability between session groups with the same sender/receiver order, might basically be an effect of priming, such that participants starting as senders were subliminally affected by seeing the pictures. This idea must be clarified and tested, however (it could, for example, be tested by relating senders' reported degree of emotional involvement in the pictures to their hit-rate). Nevertheless, considering the notorious difficulty of replicating positive results in parapsychology, we are far from certain that the present results will be replicable. However, even if the results turn out not to be replicable, we must still explain how and why the current findings were obtained.

As discussed in the introduction, exploring measures of variability may be very informative in suggesting the occurrence of specific underlying processes. However, before we know which of the above results, if any, are replicable, we will not attempt to interpret any of our findings in terms of such processes.

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Acknowledgment

We would like to acknowledge the financial support of the John Björkhem Memorial Foundation.

Abstracts in Other Languages

French

RE-ANALYSES DES DONNEES DE TELEPATHIE
EN GROUPE AVEC UN FOCUS SUR LA VARIABILITE

RESUME : L'article présente des ré-analyses des données provenant d'expérimentations sur la communication télépathique des émotions, évoquées par des images sur des diapositives, entre des groupes d'émetteurs et des groupes de receveurs. Dans la présente étude, la variabilité dans la performance est plus centrale que le niveau de performance. Elle explore les accords entre la variabilité des distributions des succès tels qu'attendus par la chance seule et la variabilité des distributions empiriques. Les distributions attendues furent dérivées des moyennes de la distribution hypergéométrique, qui donne le nombre de succès dans une séquence de n tirages pour une population finie sans remplacement. Des analyses au niveau de la session montrent que la variabilité dans le taux de réussite était plus petite que celle attendue par la chance seule, en particulier lorsque les sessions des groupes débutant comme émetteurs et de ceux débutant comme receveurs étaient analysées séparément, et que l'activité géomagnétique était basse. Les analyses de type Monte Carlo indiquent que ces résultats ne peuvent pas être expliqués par des effets d'empilement. Les analyses au niveau individuel ne montrent aucun effet. Dans une seconde partie de l'étude, la variabilité des réponses aux images cibles individuelles est explorée. La variabilité diffère significativement entre les images. La simulation montre que cet effet n'était pas attribuable à des effets d'empilement. Deux prédictions à tester lors d'une expérimentation de réplication en cours sont présentées.

Spanish

REANÁLISIS DE DATOS DE TELEPATÍA GRUPAL CON UN FOCO EN LA VARIABILIDAD

RESUMEN: Reanálisis de los datos provenientes de experimentos relacionados con comunicación telepática de emociones, evocadas por

fotos en diapositivas, entre grupos de emisores y grupos de receptores fue reportado. En el presente estudio, la variabilidad en el desempeño mas que el nivel de desempeño, fue el foco de estudio. La concordancia entre variabilidad en distribuciones de aciertos esperados por azar, y la variabilidad en las distribuciones empíricas, fue explorada. Las distribuciones esperadas fueron derivadas por medio de la distribución hipergeométrica, que provee el numero de éxitos en una secuencia de n intentos a partir de una distribución finita sin reemplazo. Análisis del nivel de la sesión mostró que la variabilidad en el rango de aciertos, fue mas pequeña, que lo esperado por azar, particularmente cuando los grupos de sesiones que comenzaron como emisores y los que comenzaron como receptores, fueron analizados separadamente y cuando la actividad geomagnética era baja. El análisis de Monte Carlo indicó que estos resultados no podrian ser explicados por "stacking effect" {termino que hace referencia a puntajes espuriamente bajos o altos en un test de PES, debidos a una relación fortuita ocurrida entre los sesgos al adivinar de los percipientes y las peculiaridades de la secuencia de los objetivos (nota del traductor)}. Análisis a nivel individual no han mostrado presencia de este efecto. En una segunda parte del estudio, la variabilidad de las respuestas para las imágenes objetivo individuales fue explorada. La variabilidad defirió significativamente en estas fotos. La simulación mostrada en este efecto no fue atribuible a efecto staking. Dos predicciones que serán probadas en un experimento de replicación, en curso, son presentadas.

German

REANALYSEN VON DATEN BEI
GRUPPENTELEPATHIE FOKUSSIERT AUF VARIABILITÄT

ZUSAMMENFASSUNG: Es werden Reanalysen von Daten bei Experimenten zur telepathischen Übermittlung von Emotionen, evoziert durch Diabilder, zwischen Gruppen von Sendern und Gruppen von Empfängern berichtet. In der vorliegenden Studie stand eher die Variabilität der Trefferleistung als das erreichte Leistungsniveau im Mittelpunkt. Übereinstimmungen zwischen der Variabilität in den Trefferverteilungen unter Zufallsbedingungen und der Variabilität in den empirisch gefundenen Verteilungen wurden untersucht. Die erwarteten Verteilungen wurden mittels der hypergeometrischen Verteilung abgeleitet; mit deren Hilfe lässt sich die Anzahl der Treffer in einer Sequenz bei n-Ziehungen ohne Zurücklegen aus einer endlichen Population berechnen. Analysen des Trefferniveaus während der Sitzungen ergaben, dass die Variabilität der Trefferrate geringer ausfiel, als unter Zufall zu erwarten war, besonders wenn die Sitzungen der Gruppen, die als Sender begannen und diejenigen, die als Empfänger begannen, getrennt ausgewertet wurden und die geomagnetische Aktivität gering war. Monte-Carlo-Analysen ergaben, dass diese Resultate nicht durch Stacking-Effekte

erklärt werden konnten. Analysen einzelner Sitzungsverläufe ergaben keinerlei Effekte. Im zweiten Teil der Studie wurde die Variabilität der Reaktionen auf einzelne Zielbilder untersucht. Die Variabilität zwischen den Bildern unterschied sich signifikant. Eine Simulation ergab, dass sich dieser Effekt nicht auf Stacking-Effekte zurückführen ließ. Es werden zwei Vorhersagen gemacht, die in einem laufenden Wiederholungsexperiment überprüft werden.

BOOK REVIEWS

THE END OF MATERIALISM: HOW EVIDENCE OF THE PARANORMAL IS BRINGING SCIENCE AND SPIRIT TOGETHER by Charles T. Tart. Oakland, CA: Noetic Books, Institute of Noetic Sciences; New Harbinger Publications, 2009. Pp. xi + 397. $29.95 (hardback). ISBN 978-1-57224-645-4.

Since the 17th-century scientific revolution, scholars have struggled with the issue of how to reconcile the physicalism and determinism of modern science with human freedom and dignity. René Descartes dealt with the problem by bifurcating the world between extended, dead matter and unextended, living consciousness. According to his version of substance dualism, human beings are "thinking things," with freedom, dignity, and immortality.

After Darwin's theory of evolution placed the development of mind into the context of evolutionary biology, some scholars, such as Henry Sidgwick, Frederick Myers, Edmund Gurney, and William James, founded psychical research, the ancestor of contemporary parapsychology. Although not all of these thinkers were dualists, the brunt of psychical research focused on the possibility of the proving via experience the existence of a nonphysical component of the human being, a component that could survive death. Even J. B. Rhine (1947), who revolutionized parapsychology with an experimental approach, tended toward a substance dualistic interpretation of psi evidence. Charles Tart's book *The End of Materialism* lies squarely within this dualistic tradition.

Like the founders of psychical research, Tart is disturbed by what he considers to be the materialism (or physicalism) and determinism of contemporary science. He makes a sharp contrast between the 19th century Canadian psychiatrist Richard Maurice Bucke's experience of "cosmic consciousness," of the universe as alive, and the worldview of modern science, which accepts a universe of "dead matter." The latter view is exemplified by the near-nihilistic pessimism of Bertrand Russell's "firm foundation of unyielding despair." Tart believes that a recovery of spirituality (as opposed to "organized religion") can halt the slide into the anomie of materialism.

Ironically, it is through science that Tart believes such a recovery can take place. He defends the position that the findings of parapsychology are consistent with the existence of a nonmaterial aspect to human existence, opening the door to a new spirituality informed by the best findings of science. As a prelude to his exploration of parapsychology, Tart discusses an exercise he gives to his students called "The Western Creed." This creed, in the same literary form as religious creeds, has a person

affirm materialism, atheism, the lack of any objective life meaning, the subjectivity of moral values (leading to individual ethical hedonism), no divine retribution for wrongs ("sins"), and no afterlife. Tart is interested in his students' emotional response to the Western Creed—what "gut-level" reaction does a person have to the affirmation of what is, in effect, nihilism?

In ethics, a similar approach appeals to the "yuck factor" of certain actions, such as Leon Kass's (2002) example of the negative emotional reaction U.S. citizens had to the news that dead bodies would be used in auto crash tests. Kass suggests that the "yuck factor" may represent deep moral wisdom. Could the "yuck factor" regarding materialism suggest a deep metaphysical wisdom that materialism may, in fact, be false? While Tart does not go that far, his thought experiment suggests that, to him and many others, materialism seems too "awful" to be true; if it were true, it would be psychologically "unlivable."

To be fair to materialists, there are philosophical materialists who deny that materialism has the implications Tart claims. Canadian philosopher Kai Nielson (1990) has defended an objective "ethics without God." Humanistic thinkers believe that a person can live a truly meaningful life bettering humanity without belief in anything other than the material universe. But Tart's position holds such positions to be inconsistent. If materialism is true, then everything is governed by deterministic scientific law plus chance interactions of dead matter—which is Jacques Monod's (1971) position in his book *Chance and Necessity*. The philosopher George Mavrodes (1998), while not going as far as Tart, argues that the seriousness of moral obligation is heightened by theism.

Tart moves on to discuss ways of knowing and pathologies of knowing and learning. He does not strictly limit all knowing to science (which would be an unscientific claim), and he recognizes that it is pseudoskepticism, rather than true skepticism, that denies even the possibility of psi. These claims are unproblematic. What is problematic is his conception of the "scientific method," which he privileges over other ways of knowing—thus his call for an "evidence-based" (i.e., scientific) spirituality. Tart's version of "the scientific method" is basically Baconian, with observation leading to theory formation, followed by prediction and testing (experimentation), followed by refining our view of reality.

Philosophers of science have mounted so many criticisms against this view of the "scientific method" that I know of no philosopher of science who would take it seriously. All observation is theory-dependent (Hansen, 1958), and there is no such thing as a pure "fact" uninfluenced by theory. Even the statement, "Rover is a dog" requires some low-level theory about what a dog is. In addition, theory-formation is often done in the light of a larger theoretical framework, a "paradigm" that guides scientists in how they should interpret the data (Kuhn, 1996). Tart may have been misled by the nature of parapsychology, because it lacks an overarching paradigm

and would fall into what Kuhn calls "preparadigmatic science." Other philosophers of science offer different models of theory formation and testing, including Popper's (1959) falsificationism; Tart accepts the ability to be falsified as a necessary condition for a successful scientific theory, Lakatos's (1978) "research programmes," and Lauden's (1977) "research traditions." The latter two models (as well as Kuhn's and Popper's later work) all propose sophisticated models of scientific theory formation and change that take into account the scientific community's role in evaluating theories (and to be fair to Tart, the importance he places on "communication" does bring in the larger scientific community). But science itself has its traditions, and while Tart is certainly right in criticizing "scientism," a philosophical interpretation of scientific data that permits only materialistic explanations of reality, science cannot avoid being influenced by larger worldviews and philosophical movements.

It is also not clear that there is one "scientific method." A good case can be made that there are a plurality of methods, depending on the particular field of science. Sometimes methods may clash even within a particular science (theoretical versus experimental physicists, for example). It may also be the case that the methods of science change throughout history (Chalmers, 1999). This does not imply that science is wholly subjective and arbitrary, as Paul Feyerabend (1975) believed, but that greater knowledge of how a field of science works may lead to the development of better methodologies.

It is also not clear that science offers a superior route toward spirituality than religious tradition. Spirituality, contra Tart, is not primarily an individualistic matter. Most often, spiritual experiences occur within the context of a particular community. It is only American pluralism and the divorce of such experiences as NDEs from particular faith communities that would lead one to accept an individualistic view of spirituality. But to define spiritual experiences as primarily individual experiences is not only ahistorical; it privileges the extreme individualism of the United States (an extreme individualism which itself, ironically, is a tradition) over other cultures' interpretations of these experiences.

Tart's alternative to materialism is a version of Cartesian substance dualism, which he defends using evidence from psi. His book contains an excellent summary of the evidence supporting "the big four" (telepathy, clairvoyance, psychokinesis, and precognition), often referring to Tart's own work in these areas. He also summarizes the results of recent remote viewing and psychic healing experiments. Although he discusses his controversial claim that psi ability can be improved through feedback, it is an important issue that is proper to mention in a discussion of psi abilities. Tart's summary is accessible to the general reader and offers those who are not trained in parapsychology a clear, up-to-date account of current research and the strength of the evidence for psi. Tart goes on to summarize the evidence concerning other aspects of psi such as out-of-body experiences

and mediumistic experiences. Here Tart discusses the possibility of survival of death.

The key claims Tart makes in his book have to do with his extrapolations from psi evidence to conclusions about the nature of the human mind. But why should he assume that the options "materialism versus dualism" are the only realistic options? And is his interpretation of "materialism" the only possible interpretation? The mathematician and philosopher Alfred North Whitehead, for example, believed that all reality is made of "actual entities" or "actual occasions" (Whitehead, 1979 [1929]). Every actual occasion has both a mental and physical pole, but these are not separate substances. Such "panpsychism" (a version of which Leibniz also accepted in the late 17th century) is nondualistic but allows for meaning in life, the existence of God, and (in the case of Leibniz) personal immortality.

Some Christian philosophers, such as Nancey Murphy (2006), are materialists when it comes to the mind-body problem. She believes that the human mind can wholly be explained in terms of the human brain. Yet she accepts the existence of God (and is a metaphysical dualist in that sense), but she also believes in the resurrection of the body (God "plugs" the memory patterns of a person on earth who has died into a "new body"). Yet Tart ignores the possibility of bodily resurrection. Why should he assume that the immortality of the soul is the only possible way to experience personal immortality?

Tart admits the possibility of quantum mechanical interpretations of psi experience similar to the approach of Dean Radin (2006). Tart is correct, in my judgment, in noting the dangers of extrapolating too easily from quantum theory to other fields. Although he does not take Stephen Braude's (1996) approach that holds quantum mechanical approaches to be overly reductionistic, Tart's caution is commendable. However, the very possibility of a quantum mechanical approach eliminates a sharp disjunction between the options of materialism and dualism.

Tart argues that telepathy and clairvoyance support the existence of a nonphysical realm because they work in spite of shielding devices that block all electromagnetic signals, they maintain their strength in spite of distance, and they may also be independent of time. Precognition shows, Tart believes, the independence of psi phenomena from time, and psychokinesis shows the ability of the mind to move objects in the physical world.

Although Tart admits to a broad conception of "nonphysical" or "nonmaterial," he does not define what he means by "physical" or "matter." He seems to include energy in the same category as matter (given that it can be converted to energy, and vice versa, this is a reasonable position). But could psi be mediated by some kind of energy not yet detected that could, in a broad sense, be called "physical"? Tart would refer to this view as "promissory materialism," but even if some "material" explanation for psi

is discovered, it still might leave room for the spiritual values Tart desires to defend. A scientist can be as overeager to dismiss a quasi-materialistic explanation for psi as to dismiss a nonphysical explanation. The most reasonable position, in my judgment, is to say that we do not know whether the mechanism for psi is physical in some broad sense or completely nonphysical.

As a philosopher, I am impressed with the scope of Tart's book. Not only does he summarize nicely the evidence for psi phenomena, he also delves into issues in metaphysics, epistemology, the philosophy of mind, the philosophy of science, the philosophy of religion, and religious studies. It is no surprise, then, that the noted religious scholar Huston Smith (with Kendra Smith) wrote the foreword. Due to its flexibility, *The End of Materialism* could be used in a variety of college and university courses, such as an introductory parapsychology course, a course on parapsychology and philosophy of mind, a course on religious experience, and even a course in the philosophy of science. Despite its flaws (and no book is without some flaws) this book is a worthy summary of the thought of one of the most significant parapsychologists of the last 50 years. It is worth owning, at the very least, as the reflections of one of the leading parapsychologists concerning his many years of research in the field. It belongs in the library of every person interested in parapsychology.

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THE PSYCHOLOGY OF PARANORMAL BELIEF: A RESEARCHER'S HANDBOOK by Harvey J. Irwin. Hertfordshire, England: University of Hertfordshire Press, 2009. Pp. vii + 213. $34.95 (paperback). ISBN 978-1-902806-93-8.

Psychologists and other behavioral researchers have been interested for many years in why otherwise ordinary people sometimes hold beliefs about themselves and their worlds that appear to be biased, irrational, or contrary to what most other people believe. Early efforts to explain unusual beliefs, such as Mackay's *Extraordinary Popular Delusions and the Madness of Crowds*, were mostly descriptive in nature, but more recent investigations have involved careful experimental and correlational studies of the situational and dispositional antecedents of atypical beliefs. In *The Psychology of Paranormal Belief: A Researcher's Handbook*, Irwin focuses on one subset of such beliefs—those involving phenomena that are commonly regarded as "paranormal." In this book, Irwin provides a comprehensive and interdisciplinary review of theory and research on the nature and functions of paranormal beliefs, the characteristics of people who believe them, and methodological and measurement issues in this area. Although most of the scholarship in the book is based on work in psychology, it also relies heavily on sociological and anthropological perspectives.

Irwin begins by tackling the thorny conceptual problem of how to define a paranormal belief. The range of phenomena that are typically characterized as "paranormal" is quite broad, encompassing beliefs involving superstitions, extrasensory perception, divination, magic, disembodied spirits, time travel, extraterrestrials, cryptozoological creatures (such as

Bigfoot and chupacabra), and even certain features of organized religions. Given the diversity and complexity of these beliefs, one should not be surprised that no one has yet offered a definition of paranormal belief that easily captures them all. However, the conceptualization that Irwin offers works as well, if not better, than most. Irwin defines a paranormal belief as "a proposition that has not been empirically attested to the satisfaction of the scientific establishment but is generated within the nonscientific community and extensively endorsed by people who might be expected by their society to be capable of rational thought and reality testing" (pp. 16-17). Irwin stresses that paranormal beliefs need not involve assertions that are fundamentally scientifically unacceptable but only those that are not currently verified or accepted by science. For example, nothing in science suggests that cryptozoological or extraterrestrial creatures cannot exist, but because their existence has not been scientifically documented, maintaining that they exist constitutes a paranormal belief.

Irwin's working definition is particularly intriguing because it encompasses not only the typical variety of paranormal beliefs (for example, beliefs in ghosts, psychic phenomena, and astrology) but also widely accepted claims that have not been scientifically confirmed such as the belief that God answers prayers or the belief in a just world. Irwin is correct to suggest that such beliefs should be regarded as paranormal no matter how many people believe them or how accepted they are within a particular culture.

After establishing the domain of the book, Irwin discusses sociocultural influences on both what people regard as paranormal (beliefs that are accepted as self-evident in one culture may be viewed as wildly paranormal in another) and who comes to endorse paranormal beliefs. The determinants of paranormal beliefs are largely the same as those of any belief system, including influences that originate from parents, peers, one's conjugal partner or spouse, educational institutions, social movements, the media, and culture more generally.

In chapter 3, Irwin provides an invaluable service to all researchers who study paranormal beliefs by describing and critiquing the most commonly used self-report measures of paranormal belief systems. For researchers who study paranormal beliefs, this chapter alone is worth the price of the book, providing not only detailed descriptions of the psychometric properties of the measures and reviews of studies that have used them but also the scales themselves (in an appendix). Coming away from this chapter, it is easy to see the advantages and disadvantages of each measure as well as a broad picture of the current state of the field and how the measurement of paranormal beliefs might be improved in the future. No researcher should undertake a study of paranormal beliefs without consulting this chapter.

Having dispensed with conceptual and measurement issues, Irwin devotes four chapters to detailed scholarly analyses of four prevailing hypotheses regarding the antecedents of paranormal beliefs, hypotheses

that focus on social marginality, people's worldviews, cognitive deficits, and psychodynamic functions. The social marginality hypothesis proposes that people who are most inclined to adopt paranormal beliefs tend to be members of disadvantaged or marginalized groups, who gravitate to such beliefs to deal with the sense of low control and privation that they regularly experience. After reviewing the empirical evidence, Irwin concludes that research findings do not support the social marginality hypothesis. The worldview hypothesis, which links paranormal beliefs to a broader worldview involving subjective and esoteric beliefs, fares somewhat better, although Irwin notes that other considerations, such as a sense of being vulnerable to uncontrollable events, may also be involved.

A third perspective, the cognitive deficits hypothesis, suggests that people who adopt paranormal beliefs tend to be illogical, irrational, uncritical, or credulous, if not downright unintelligent. By and large, the data do not support the notion that people believe in paranormal events because they are unable to think carefully and critically about such things. Finally, the psychodynamic functions hypothesis suggests that people adopt paranormal beliefs when those beliefs serve psychological functions for them. Although the hypothesis has little support with respect to adopting paranormal beliefs in general, Irwin suggests that it might apply to certain kinds of paranormal beliefs. Given that many of people's beliefs serve psychological functions for them—warding off anxiety, providing meaning, reducing uncertainty, and so on—it would be surprising if paranormal beliefs did not serve these functions as well.

Irwin's comprehensive review of research with respect to the antecedents of paranormal beliefs organizes the literature in a coherent and thematic way that addresses key questions about the origin of paranormal beliefs. Like all good reviews, it raises at least as many questions as it answers, allowing readers to see lingering questions that, if addressed, would promote our understanding of paranormal belief systems. Furthermore, Irwin provides the interested researcher with the most comprehensive bibliography imaginable with over 800 references.

The book concludes with the author's own efforts to integrate what is known about paranormal beliefs within a causal model. A central feature of this model is the distinction between the presence and the activation of a belief. That is, people may come to hold a particular belief, but that belief may have little effect on their interpretations of or reactions to events until it is activated. According to the model, early experiences with a sense of low control and the resulting desire for mastery are hypothesized to underlie the presence of paranormal beliefs, which are molded by a combination of sociocultural and psychological factors such as those that are discussed in chapter 2. Then, contextual stress arising from a situation that induces an immediate sense of vulnerability or loss of personal control is needed to activate the belief and bring it to the foreground. An activated belief may involve both conscious thoughts and

observable behaviors that are relevant to the belief (such as consulting a psychic, reading about paranormal phenomena, or joining a ghost-hunting group). Such thoughts and actions are then predicted to lower state anxiety.

Irwin's model takes an important step in trying to explain the origin and functions of paranormal beliefs. As he admits, the model may not apply equally to all categories of paranormal beliefs, possibly being most relevant to parapsychological and magical beliefs. (It is more difficult to see this process underlying beliefs regarding cryptozoological creatures, for example.) Indeed, one recurring question in the book is the degree to which various categories of paranormal beliefs operate similarly.

The model also does not easily address instances in which people believe in paranormal phenomena that they would prefer *not* to believe in. There are certainly people who believe in the existence of ghosts, psi, and extraterrestrials who would be happier and less anxious if they didn't, but the model does not appear to account for paranormal beliefs that increase rather than reduce anxiety or a sense of vulnerability. Nor does it easily explain people who come to a paranormal belief via a purely intellectual route when they are convinced by what they view as persuasive evidence. Although the model might not account for all types of paranormal beliefs, it certainly provides an organizing framework for much research in the area, identifies novel relationships among variables, and generates clear, testable predictions regarding the antecedents of such beliefs.

Compared to many other scholars who have focused on unconventional, irrational, or unusual beliefs, Irwin's perspective is both more sympathetic and more even-handed. His approach is sympathetic in that he does not automatically assume that people who believe in things that science does not accept are necessarily deluded, irrational, or psychopathological. It is even-handed in including within his purview paranormal beliefs that are widely accepted, including religious beliefs involving prayer, resurrection, and the existence of angels.

Overall, *The Psychology of Paranormal Belief* makes an important contribution to our understanding of paranormal beliefs and offers insights and direction for the next generation of research on this topic. It should be of interest not only to readers who are interested in paranormal beliefs per se but also to those who are interested in the broader question of how and why people come to adopt the beliefs that they hold.

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THE HANDBOOK OF NEAR-DEATH EXPERIENCES by Janice Miner Holden, Bruce Greyson, and Debbie James (Eds.). Santa Barbara, CA: Praeger Publishers, 2009. Pp. xv + 316. $49.95 (hardcover). ISBN 978-0-313-35864-7.

The Handbook of Near-Death Experiences aims at summarizing the main findings and conclusions of 30 years of research into near-death experiences (NDEs). It is edited and written by leading experts in the field. Kenneth Ring, a prominent NDE expert himself, boldly states in the foreword that this book "is now and is likely to remain for many years the standard reference work for the field" (p. ix). Already a quick glance at the authors list, the table of contents, and the reference list shows that this is no overstatement. The book constitutes an unprecedented overview on 10 different subtopics discussed in the scholarly NDE literature. It fills an important gap among many other volumes on NDEs that all too often present unduly superficial treatises of a subject the complexity of which we have barely begun to map and understand.

The book contains 11 chapters. After the introductory first chapter, which offers a historical overview of the field of NDE studies, each following chapter focuses on a particular subtopic of NDEs and the respective research performed within this field, and addresses open questions worthy of future investigation. Typically, these chapters comprise 20–25 book pages, plus about 50 to 150 literature references for each chapter listed at the end of the book.

In chapter 2, Nancy L. Zingrone and Carlos S. Alvarado present an overview of scholarly inquiry into the contents and circumstances of pleasurable Western adult NDEs, the most familiar subcategory of NDEs that often features out-of-body experiences (OBEs) and feelings of peace, travelling through a tunnel or dark area, or being surrounded with light. This focusing on a specific subcategory of NDEs already reflects the often-underestimated complexity of NDEs. For example, unpleasurable or distressing Western NDEs with different features also exist—treated in chapter 4 of the book. Similarly, non-Western NDEs from different cultures seem to vary in important respects from typical Western NDEs—some of these variations are discussed in chapter 7. After the overview on pleasurable Western NDEs, Zingrone and Alvarado close with recommendations for future research. These include paying more attention to the existing literature on OBEs, investigating claims of veridical perceptions during OBEs, studying features such as how the experiencers perceive themselves during the OBE (e.g., as with or without a body, as a point of light, etc.), and conducting systematic studies on the elements present in the descriptions of the "transcendental" environment.

In chapter 3, Russell Noyes along with Peter Fenwick, Janice Miner Holden, and Sandra Rozan Christian describe the various aftereffects of pleasurable Western NDEs. Because NDEs are profound and emotionally

powerful experiences, they often radically change not only one's previous attitude toward death, but also one's social, religious, and general attitudes toward what is important in life. Not all of these changes are by themselves positive or life-enhancing. And, even if NDErs are affected very positively by their experience, they can still suffer from a deep feeling of alienation from spouses or friends who are not able to adjust to these suddenly altered attitudes. Thus, negative aftereffects of a secondary type might be the consequence for the NDEr and his or her family. For example, several studies found that the divorce rate of NDErs was strongly increased as compared to control groups. With regard to parapsychological issues, it seems that NDErs continue to report paranormal episodes and periodic alterations of their consciousness that were not present before their NDE. This suggests that NDEs render people more suggestible or more accessible to psychic phenomena, or simply more aware of them.

In chapter 4, Nancy Bush provides an overview on the contents and aftereffects of distressing NDEs. Far from what is often assumed, not all NDEs are pleasurable. In fact, there are three different types of distressing NDEs. The first type contains the typical elements also present in pleasurable NDEs. But instead of feeling peaceful and blissful, the experiencer is profoundly frightened and terrified by them. The second type of distressing NDEs involves "a paradoxical sensation of ceasing to exist entirely, or of being condemned to a featureless void for eternity" (p. 71). The third type represents downright hellish experiences featuring threatening demons, hell-like landscapes, falling into dark pits, and so forth. It is unclear why some people have pleasurable NDEs whereas others have distressing NDEs. So far, no causal relations, such as distressing NDEs being experienced predominantly by criminals or by persons with strong feelings of guilt, have been identified.

In chapter 5, the most touching of all the book's chapters, Cherie Sutherland summarizes the research findings regarding NDEs of Western teens and children. The NDEs of very young children are of particular interest for NDE research because infants can be regarded as comparably free of cultural and educational influence. Thus, it is surprising that NDEs of even very young Western children feature the same elements as NDEs of Western adults: apparently veridical OBEs, tunnels, bright lights, beings of light, deceased relatives or friends, but also life reviews. Some also experience distressing NDEs and their respective aftereffects. It is especially surprising that there are several case reports in which young children claim to remember NDEs from life-threatening events that had already happened during the first days or months after birth. One might ask: Why are they able to properly perceive and remember such events at all, given that their eye and brain structures are regarded as not developed well enough to provide proper visual perception and to store such detailed memories at such early ages? These reports challenge current theories on brain development and functioning—if they can be trusted. But for now,

there are no reasons that would justify rejecting these cases simply because they don't fit into current mainstream models of the mind. They obviously deserve the interest of the scientific community and should be looked for in future investigations.

In chapter 6, Janice Miner Holden, Jeffrey Long, and B. Jason MacLurg examine research performed to determine the characteristics of Westerners who have experienced NDEs. They discuss demographic and psychological variables that might influence the likelihood that somebody will experience an NDE, or the quality and depth of the NDE. Among others, these variables include gender, age, ethnicity, education, socioeconomic status, occupation, religious affiliation and religiosity, physical disabilities such as being blind, psychopathological characteristics, or fantasy proneness. In sum, the investigations addressing these variables show that none of them allow for predictions about who will have an NDE and of what quality it will be. In other words, any one of us can have an NDE of unpredictable depth and quality under given circumstances. Thus, the authors conclude:

> We found little evidence to support previously proposed biological, psychological, or sociological explanations as the sole cause(s) of NDEs. . . . Visual experiences in blind experiencers, including those blind from birth, provide strong evidence of the insufficiency of biological, psychological, or social hypotheses, either individually or in combination, to explain NDEs. Converging lines of evidence support the hypothesis that the cause of NDEs involves some component(s) other than or in addition to biological, psychological, or sociological factors alone." (pp. 132–133)

I would have also been interested in learning whether the ingestion of strong pharmaceuticals influences NDEs, and, if yes, in what way. But the authors included no information on this. Still, I doubt that any answer to this question would have an impact on the above-cited conclusions.

In chapter 7, Allan Kellehear provides an overview of the features of non-Western NDEs described in the literature and adds speculations about the origin of these differences. These non-Western NDEs include case reports from different Asian countries and the Pacific Islands, as well as from hunter-gatherer cultures of both Americas, Africa, and Australia. In comparing the different narratives, Kellehear shows that well-known features of Western NDEs such as the life review or the tunnel sensation are *not* universal. Although Kellehear does not explicitly discuss encountering ineffable brilliant lights, as reported from many Western NDEs, it is my impression from reading the literature and Kellehear's chapter that these experiences are also no universal feature. Given such differences, Kellehear

argues that explanatory models for NDEs that are based solely on brain physiological processes cannot account for the whole picture. In his eyes, the reported differences point to sociocultural influences that manifest during NDEs. For example, he attributes the lack of tunnel descriptions in NDEs in certain cultures to different verbalizations, interpretations, and translations of basically the same universal experience, namely travelling through an area of darkness. Because Asians will not be so familiar with real tunnels as Westerners, they won't report so many tunnel sensations in their NDE narratives, but replace them with other descriptions such as crossing a mere darkness.

Although I agree with Kellehear on the major conclusions he draws, he seems to put too much emphasis on possibly divergent interpretations of the allegedly universal NDE feature of travelling through a dark region. First, from the little data available, it seems that travels through dark regions are in general less commonly reported by non-Western NDErs than by Western NDErs—especially as a crucial feature occurring predominantly around the *beginning* of NDEs. Second, some Western descriptions of travels through tunnels (such as being whirled through them at an enormous speed without the contribution of one's own intention) and non-Western dark areas (such as walking through a dark rocky chasm in the mountains on one's own feet at normal walking pace) seem too dissimilar to be regarded as culturally determined verbalizations of the same basic experience. Third, typical Western tunnel experiences include the encounter of a brilliant, empathic, and ineffable light. Typical Western NDEs also include a life review—even in some narratives of very young children without prior knowledge of NDEs and largely devoid of cultural influences. Yet, as mentioned above, these features of Western NDEs seem to be missing in other cultures. But why, then, do NDEs of young children with no or only marginal relevant cultural imprints parallel the NDE patterns of adults from their own culture? Assuming that exposure to sociocultural influence shapes the experience of tunnels, lights, or life reviews, should NDEs of young children not be different from those of adults? Like the authors of another recent cross-cultural examination of NDEs (Belanti, Perera, & Jagadheesan, 2008), Kellehear did not touch upon the crucial enigma of features of children's NDEs and the factors shaping them. From the data currently available, it seems possible that accounting for the cultural differences of NDEs will entail a more complex explanatory model than attributing these differences simply to culturally determined modes of verbalization, interpretation, or the subconscious generation of NDE features. Thus, cross-cultural NDE research represents one of the most fascinating areas of future NDE research. Ideally, such investigations should include collecting NDEs of very young children. This approach would constitute a promising way to address the question of which factors govern the structure and content of NDEs, namely: Which elements are determined by brain physiology, by sociocultural influences, or, perhaps, also by some kind of transcendental causation?

In chapter 8, Farnaz Masumian presents an overview on "World Religions and Near-Death Experiences." This chapter does not in my opinion match the high scientific and scholarly standard present in all of the other chapters. Masumian seemed to be guided by a vision that something in every major religion must somehow match this or that feature of NDEs. While disregarding all incongruencies between traditional lores on the afterlife and the content of NDEs, she constructed numerous alleged correspondences many of which I found too superficial or too decontextualized to be of true significance.

In chapter 9, one of the most fascinating chapters in the book, Janice Miner Holden reviews research performed on one of the most controversial aspects of NDEs: the claim of some NDErs that they had been able to correctly observe what was happening around their unconscious body, or also at some distance. Holden refers to such cases as *apparently nonphysical veridical NDE perception* (AVP). Should cases of AVP be substantiated, they would provide significant evidence that human consciousness can function independently from the brain under certain conditions. There have been two ways of exploring AVP in the past: (a) retrospective studies, i.e., studies in which researchers evaluate AVP reports retrospectively, often long after the NDE has occurred, and (b) prospective studies, that is, studies in which researchers conduct specifically designed investigations with the aim of collecting and documenting AVPs under controlled conditions within a given time frame. In a literature survey of retrospectively published cases of AVP, Holden identified 107 cases. The most impressive case concerns Pam Reynolds, who was artificially rendered into conditions of cardiac arrest and standstill of all brain activity for the purpose of performing a complicated operation under her skull. Nevertheless, she claimed to have observed the scenery from above her body and gave veridical descriptions of incidents that occurred during this operation while she was in conditions of full, deep surgical anesthesia that preceded the standstill of her body functions.

With regard to the few prospective studies that have been done, Holden outlines the difficulties involved in performing such studies, and summarizes that there have so far not been successful documentations of AVPs. Nevertheless, I missed any mention of the findings of a prospective study published by Sartori, Badham, and Fenwick (2006) in this review. The authors reported an AVP and a well-documented incident of unexplained body healing that happened during the NDE of a patient. Moreover, Sam Parnia and collegues have initiated a large prospective research project involving about 25 hospitals in Europe and North America, the AWARE Study (Parnia, 2008). The aim of this study is to examine potential AVP in 1,500 survivors of cardiac arrest. After some preparation time, it was officially announced on November 9, 2008. Regrettably, this book chapter contains no mention of this project—I suspect because of publication time lag.

In my opinion, chapter 10 constitutes the climax of the book. Bruce Greyson, Emily Williams Kelly, and Edward F. Kelly address the currently available explanatory models for NDEs in detail. In doing so, they analyze the extent to which each model accounts for all features of NDEs—long since the most controversially discussed subtopic of NDE research. Among others, the authors review the hypotheses building on expectation, depersonalization, altered blood gas levels (such as hypoxia, anoxia, and hypercarbia), neurochemical hypotheses (such as endorphins and ketamine-like neuroprotective agents), neuroanatomical hypotheses (such as temporal lobe dysfunction), and the transcendental hypothesis, in which it is supposed that the human mind can also function independently from brain physiology. The authors also address multifactorial hypotheses. It is impossible to give an adequate summary of this vital and detailed chapter in this review. The take-away message is: Things are again more complex than often assumed, especially as assumed by most mainstream scientists. The currently available psychological and neurophysiological hypotheses appear to cover at best only parts of the entire phenomenology of NDEs. Interestingly, the authors also describe examples in which advocates of neurophysiological models seem to have misled their readers, sometimes citing allegedly supportive literature incorrectly. I thoroughly recommend that everybody seriously interested in understanding the role of possible neurophysiological triggers or correlates of NDEs read this chapter in depth.

In the final chapter, 11, Ryan D. Foster, Debbie James, and Janice Miner Holden address practical applications of NDE research for health care and educational settings—including medical, psychological and spiritual health care providers working with NDErs, the terminally ill, or the bereaved. This closing chapter left a twofold impression on me. On the one hand, the authors did a superb job in reviewing the relevant literature and highlighting specific subtopics of it. They also included helpful recommendations for those who attend to NDErs. On the other hand, I felt the imbalance between the importance of the subject—death and dying—and the apparent lack of knowledge about NDEs among many providers who care for NDErs, the terminally ill, or the bereaved. It seems that in many cases NDErs and others who are personally confronted with death meet rather unprepared and helpless care providers when it comes down to discussing and integrating the profound experiences they have encountered, be it in the medical, spiritual, or religious setting. Many decades after Raymond Moody (1975) published his seminal book on NDEs, *Life after Life*, many NDErs still fear sharing their experiences out of concern for being ridiculed or rejected. The NDE hype that followed Moody's book has certainly declined. But people still have NDEs and die. It is my hope that the present handbook keeps the public and the scientific interest in NDEs alive and stimulates further research into near-death states.

Conclusions

For those who are personally affected by NDEs and death, but also for scientists who struggle to elucidate the many riddles of the human mind, an appropriate understanding of the factors governing NDEs is important. The authors of *The Handbook of Near-Death Experiences* point out in the various chapters that any intellectually responsible explanatory model for NDEs must address the following crucial questions:

(1) How can complex consciousness, including thinking, sensory perception, and memory, occur under conditions in which current physiological models deem it impossible? As the authors of chapter 10 pointed out, all physiological and psychological hypotheses proposed so far face severe difficulties and are sometimes even inconsistent with the data available.
(2) How can similar and sometimes *identical* experiences occur under conditions of severe brain dysfunction (such as during cardiac arrest) and under conditions of optimal brain functioning (such as during falls and other circumstances only suggestive of an impending death)?
(3) Why do crucial features of NDEs of Western babies and children seem identical to those of NDEs of Western adults—assuming that small children have not yet internalized the respective cultural influence and religious education?
(4) Assuming that brain physiology determines what NDErs experience, why does not everybody experience or remember NDEs, and why can NDEs vary considerably in different individuals? And why do crucial features of NDEs differ in different cultures?
(5) Assuming that brain physiology in combination with psychological factors determine the features of NDErs, why do expectations regarding the afterlife often contrast with what is experienced during NDEs (not only in both Western children and adults, but also in adults in various non-Western cultures)?
(6) Why are there several reports concerning NDEs of babies and very young children, who should not be able to remember and retell NDEs according to the standard models of brain development and physiology?
(7) Why do many NDE-OBErs (and also healthy OBErs) report AVPs concerning the direct surroundings of their motionless body, sometimes also concerning events taking place at distant locations?
(8) Why do blind persons, even if blind from birth, report such AVPs?

At present, much remains to be clarified. No simple answers to these questions are currently available. Nevertheless, there is no doubt that with the publication of this volume, the time of general and often unsupported speculations about the factors triggering and shaping NDEs should be past. I concur with Ring that this book represents the standard reference work on which the discourse about NDEs should be based in the coming years.

From a parapsychological perspective, discussions of several facets of NDEs and related end-of-life experiences were missing in this book. Examples are unexplained body healings during NDEs or near-death states, shared and reciprocal NDEs (including crisis apparitions) or deathbed visions, reports of mists or lights leaving the body of the dying, reports of unexplained music heard at deathbeds, physical death-related phenomena, or possible relations of NDEs to afterlife descriptions given by children who claim to remember previous lives. Such issues represent topics largely underrepresented in the professional literature on NDEs and near-death states. But for the purpose of this handbook, that is, summarizing the scholarly investigations into NDEs, adding these issues to the already puzzling AVPs might well have bewildered the majority of the target group of readers. Still, it should be stressed that NDEs are far from constituting a single oddity occurring in near-deaths states but are part of an intriguing interconnected web of death-releated experiences indicative of paranormal causation. It would take another volume to present and discuss all these relations. Hopefully, such a volume will be compiled one day.

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IMMORTAL LONGINGS: F. W. H. MYERS AND THE VICTORIAN SEARCH FOR LIFE AFTER DEATH by Trevor Hamilton. Exeter, UK: Imprint Academic, 2009. Pp xiv + 359, $39.90 (cloth). ISBN- 9781845401238.

F. W. H. Myers, one of the founders of the Society for Psychical Research (SPR) and author of *Human Personality and Its Survival of Bodily Death*, has recently had somewhat of a revival, pun intended, a few years after the centenary of his death. He was the subject of a profile in a mainstream journal (Kelly & Alvarado, 2005), his work has been reassessed and updated in the light of contemporary psychology (Kelly et al., 2007), and now Trevor Hamilton has produced a carefully researched biography of this complex and original personality.

Myers was a man who evoked strong feelings, by no means all of them positive, and Hamilton acknowledges this from the start by quoting on page 1 two conflicting character summaries. One describes Myers' capacity for sympathy and comradeship, whereas the other refers to "despotism, meanness and all sorts of things lurking in the background ...." While acknowledging Myers' various achievements, Hamilton's book is not a hagiography. Myers is variously described as being arrogant and a snob, with a tendency to judge individuals by their social standing, to appreciate women largely on the grounds of their beauty, and to carefully point out that there was no reason to suppose that his name might indicate "Jewish descent." Although Myers' scholarship shows that he was a free-thinker carving out new territory on the edges of the developing science of his time, it is ironic that, as a person, he was bound in so many ways by the conventions of the class and country in which he was born.

It also becomes clear that Myers' personality had a strain of recklessness, evidenced not only by his night-time swim across the Niagara river but also by his self-conscious sense of superiority in appropriating the lines of other poets for his own work. The latter resulted in accusations of plagiarism and a university scandal. Nonetheless, although Hamilton is not blind to Myers' all-too-human flaws, he is still fairly successful in defending him from the ruthless character assassination of two earlier books by Trevor Hall: *The Strange Case of Edmund Gurney* (Hall, 1964) and *The Strange Story of Ada Goodrich Freer* (Hall, 1980). In them, Myers is cast as the worst kind of Victorian villain, implicated in all manner of shadowy deeds and capable of driving purer souls to suicide. Many of the weaknesses in Myers' character likely stemmed from the sense of entitlement of an academically and athletically gifted man who had fairly reasonable means at his disposal (although he exaggerated them when courting the woman who would eventually become his spouse). In this he differed little from many men of his time. The past is another country; they do things differently there.

To most readers, the particulars of Myers' life are likely to be of less interest than his contribution to psychology and psychical research. Typically the underlying rationale for one's obsessions and life work is to

be found in the darker unresolved areas of one's life. Such is the case with Myers. His life was marked by tragedy at an early age with the death of his father. As a child, the sight of a dead mole provoked a horror at the thought of a death without the possibility of resurrection, a dread that was to haunt him throughout his life, but it was probably the suicide of the love of his life, Annie Marshall, the wife of his cousin, that propelled Myers to investigate scientifically the possibility of survival. First, however, he sought to evaluate the possibility of the independence of the mind from the body as exemplified by ostensible psi abilities such as telepathy. As Alvarado (2009) makes clear, Myers' contribution to the work of the SPR and the range of his topics and inquiries was vast and masterly, covering areas such as hypnosis, dissociation, mediumship, telepathy, PK, and other issues that form the bedrock of modern parapsychology.

Although Myers' original reputation as a poet has not survived the times and he performed indifferently as a school supervisor, the empirical and theoretical contributions by him and a number of his SPR collaborators (Eleanor Sidgwick, Edmund Gurney, and others) have fared much better. With respect to a scientific approach to the study of "spiritual" matters, they did not take a priori positions for or against psi phenomena but followed a number of methodological strategies that remain cornerstones in scientific inquiry: trying to avoid biases, being systematic in the collection of data, investigating the possibility of fraud and non-psi explanations, establishing canons for the evaluation of data, submitting their findings to publication inquiry, debate, and so on. Although at times they fell short (for instance by being particularly uncritical of witness reports from people of the higher socioeconomic classes), they nevertheless established bases that have continued to serve parapsychological research well. For instance, some of the findings from this early work, such as the percentage of people reporting hallucinations and their types, were replicated decades later (Bentall, 2000).

They favored field investigations over experimental research, but they also did careful case-study research with mediums (Gauld, 1968). Some of this research would certainly raise ethical concerns today, such as using lit matches and making incisions on the body of Mrs. Piper, perhaps history's most remarkable mental medium, to test her "trance." It is nonetheless interesting that imperviousness to fire and pain are taken as signs in other cultures of spirit possession (Cardeña, Van Duijl, Weiner, & Terhune, 2009), and of course anaesthesia is a well known phenomenon of hypnosis (Patterson & Jensen, 2003). Scientific fashions change and in the USA the approach of investigators became far more experimental under the influential work of J. B. Rhine (Broughton, 1991), but a strong case can be made that anecdotes, case studies, experiments, and other forms of inquiry all contribute with their particular strengths and weaknesses to elucidate the nature of the elusive and capricious psi phenomena (cf. Pekala & Cardeña, 2000). Reading *Immortal Longings* brings to mind the French saying *le plus*

ca change le plus ca le meme chose (the more things change, the more they remain the same), as some of the battles and arguments concerning the validity of psi are being almost exactly reproduced in our days: Spiritualism followers who blamed the SPR group of arrogance and closed-mindedness and critics who had not even taken the trouble to read the papers they were criticizing have their unworthy successors in our midst.

Immortal Longings is a handsome book and includes beautiful photos of Myers and a number of investigators, mediums, and even a celebrity (Stanley, the explorer, was a surprise to see), but its binding proved rather flimsy. The present reviewers had a copy each, but both copies, carefully handled, detached from the back spine in the same place almost immediately after they were handled. It is reasonable to suggest that this was caused by careless bookbinders rather than poltergeists. Hamilton's biography is competent and well informed, although Myers' personality remains opaque and does not seem to come alive in the way that William James does in Jacques Barzun's book *A Stroll with William James* (Barzun, 2002). Myers had the mixed blessing of being a contemporary of James. He likely benefited from their acquaintanceship and support by James, but the immense presence of the latter overshadows just about everyone who worked on psychology and parapsychology at the time. Nonetheless, they clearly came to us with very different gifts. James was the master phrase-maker and clear thinker that gave us panoramic overviews of where psychology was then and where it might go in the future. In contrast, with his orotund and baroque prose, Myers submitted a systematic grand theory of some of the most fascinating mental phenomena: creativity, dissociation, hypnosis, psi phenomena, and yes, even the possibility of survival. Although it has become commonplace to assert that grand theories in psychology passed their expiration time some time ago, Kelly et al. (2007) make a persuasive case that it may still serve us well to revisit Myers' notion of a subliminal consciousness.

This biography gives us a good introduction to both the man and his work, not least in the manner in which it shows how many thoughts that psychology historians have originally attributed to others (e.g., Jung's notions of an integrative function in the unconscious, and some of James' ideas) were probably inspired by the overlooked work of F. W. H. Myers. For a man whose university days were besmirched by an accusation of plagiarism, it is ironic that his life's work has so often been attributed to others. There is, however, a postscript to provide a double irony. Shortly after his death, and for some time after, automatic writing mediums across the globe began to receive communications purporting to come from the spirit of Myers, building up into an intricate and complex system of cross-correspondence. Was this really Myers, speaking from beyond the grave? It appears that the lingering question over Myers' authorial authenticity pursues him even in the Afterlife.

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HANDBOOK OF INDIAN PSYCHOLOGY. Edited by K. Ramakrishna Rao, Anand C. Paranjpe, and Ajit K. Dalal. New Delhi, India: Cambridge University Press India Pvt. Ltd., Foundation Books, 2008. Pp. xix + 648. $66.00 (hardbound). ISBN 978-81-7596-602-4.[1]

This volume provides 31 chapters by scholars or experts from multiple disciplines, philosophical or religious orientations, and countries. It provides an invaluable resource for those interested in any or all aspects of the very broad topic of traditional Indian philosophical schools of thought that may be deemed to relate to the conceptual and/or applied interests of psychology. A huge strength of this volume is the presence of highly informative and, often, well-developed chapters that represent a variety of views about the underlying nature of reality—a fundamental concern in this volume (even if odd for a psychology text)—and their proposed ramifications for understanding the human mind and its function and for transforming that mind for the better.

The editors and chapter authors of this volume have endeavored to lay a groundwork for and to inspire the development of a psychology that, unconstrained by the metaphysical materialism they deem to guide Western psychology, would be ready to investigate and thereby to learn about and implement in application, concepts derived from ancient Indian scriptural sources and traditional Indian philosophies. They argue that such a psychology, unlike allegedly empiricist and materialistic Western psychology, can address issues of meaning, purpose, and value in life. They suppose that this new psychology should be able to help individuals realize their full potential and to lead genuinely moral and ethical lives, freed from the delusions, pain, fears, and frustrations of those lacking the self-knowledge advocated by Indian scriptures and spiritual teachers.

The development of research is a *sine qua non* for developing a psychology. It was not easy, based on reading this volume, to envision, in

[1] In the case of some of the Sanskrit terms used in this review, a typed character will lack the proper, traditional diacritical marking. These markings on a character signal a change in the pronunciation, imparting a different sound than the same character without such a marking. Due to a lack of suitable fonts on this reviewer's computer, a few of these diacritical markings had to be omitted (or in the case of the term "Vaiśeṣika," substituted) in this review. Most and possibly all of the lapses in diacritical marking in this review refer to the following terms (but there may be a very few other cases of which I have lost track): "Sāmkhya," one of the six orthodox schools (or systems) of Indian philosophy, should be written with a dot (·) directly over the "m." The same diacritical marking also should go directly over the "m" in "samskāras," impressions upon the mind that are created by one's experiences and one's actions and that are deemed to influence one's future experiences and inclinations. The title of one of the six orthodox Indian philosophical schools, written herein as "Vaiśeṣika," instead should be written with a dot (̣) directly under the "s" before "ika." I regret these errors of transcription and hope that any potential confusion related to them will be obviated by this explanatory note.

any clear way, the likely shape of the possible research programs in this new psychology, either in terms of the specific kinds of problems to be investigated or the methodologies that might be useful—except for general endorsement, under the banner of consciousness studies, of introspection without specific suggestions about how to obviate the problems related to that method or about how introspection actually would be used. The frequent derision, in this volume, of Western psychology as too centered on neuroscience and as being (allegedly) philosophically materialistic, made me wonder, reading some of the commentary, whether brain-function research might become a no-no, or something close to such, in this new science. That would be very unfortunate because viewing neuroscience as an enemy of "consciousness" research would seem something short of creative problem analysis. There is plenty of excitement here about the idea of a brave new science, but there are few indications about how it might proceed in order to add meaningfully to—or perhaps even to revolutionize—the scientific understanding of the mind, including of human personality. Had an author well informed about both contemporary cognitive science and traditional Indian philosophy been charged specifically with "seeding the mind" of the reader about potential research problems and investigational approaches for this new science, the result might have been an important contribution.

Chapter Authors and Topics

A full table of contents, with chapter titles, may be found at the following website: http://www.cambridgeindia.org/ShowBookDetails4.asp?ISBN=9788175966024. Actual titles of chapters are not provided below, but, instead, brief indications of the thesis or content of each, along with the chapter numbers and the authors' names.

Introductory chapters (2), which include (Ch. 1), a prologue by K. R. Rao that introduces Indian psychology, along with chapter sketches, and (Ch. 2), wherein S. K. K. Kumar provides a historical perspective on Indian thought and tradition related to psychological issues.

Part I—Systems and Schools

(Ch. 3) P. Jain on Jaina psychology; five chapters on Buddhist psychology, specifically, (Ch. 4) D. J. Kalupahana on early foundations; (Ch. 5) P. D. Premasiri on cognition in early Buddhism; (Ch. 6) W. Waldron on unconscious mind; (Ch. 7) J. Duerlinger on theories of "persons" or nonpersons, given the Buddhist-posited illusory character of self; (Ch. 8) W. L. Mikulas offers a western interpretation of Buddhist psychology; and seven chapters on Vedic (i.e., "Orthodox" or Hindu) traditions, specifically; (Ch. 9) S. Menon examines the *Bhagavad-Gītā* relative to consciousness, meditation, work, and divine love; (Ch. 10) K. R. Rao and A. C. Paranjpe

clearly and carefully explain yoga psychology, both theory and application; (Ch. 11) W. G. Braud describes the ramifications of Patañjali's *Yoga-Sūtras* for psi theory and research, noting potentially relevant parapsychological findings; (Ch. 12) E. Taylor and J. G. Sugg explain yoga psychology's reliance on the metaphysics of Sāmkhya, a dualistic system positing both pure consciousness and inert matter; (Ch. 13) A. C. Paranjpe and K. R. Rao, describe psychology in the Advaita (i.e., monistic) Vedānta teachings; (Ch. 14) V. N. Jha explains the concepts of perception in the Nyāya-Vaiśeṣika system; and (Ch. 15) M. Kapur describes psychological concepts and practices in Āyurvedic medicine, whose roots are in ancient Indian treatises.

Part II—Topics and Themes

(Ch. 16) S. R. Bhatt contrasts Buddhist views of perception with those of non-Buddhist Indian philosophies; (Ch. 17) A. S. Dash delineates Indian (Hindu) views on the origins of motivation, of voluntary and involuntary action, including the *Bhagavad-Gītā* on detachment from the fruits of action; (Ch. 18) A. K. Jha presents the views of the person ("personality") as seen in the six orthodox systems (schools) of Indian philosophy; (Ch. 19) L. Krishnan and V. R. Manoj describe diverse facets of "giving" (or prosocial behavior) from the perspective of an Indian view of values; (Ch. 20) C. P. Bhatta delineates Indian aestheticians' views on prerequisites of being a creative poet; (Ch. 21) D. P. S. Bhawuk presents a model with desire at the root of cognition, emotion, and behavior, based on the *Bhagavad-Gītā*; (Ch. 22) M. Cornelissen argues that an erroneous, inadequate (or dismissive) view of consciousness is a root cause of many ills of humanity and of an (allegedly) impotent psychology; and (Ch. 23) G. A. Mohan, discusses J. Krishnamurti, a spiritual—albeit declaredly nonreligious—teacher originally from India, and explains Krishnamurti's views about obtaining total freedom by escaping the conditioning of the mind.

Part III—Applications and Implications

(Ch. 24) M. Miovic ponders traditional psychotherapy vis-à-vis yogic ideas and practices, with commentary on their inter-relationships, including potential dangers of sādhanā (i.e., yogic spiritual discipline), complementary roles for these two disciplines, and the need for therapist sensitivity to Indian cultural norms; (Ch. 25) D. P. S. Bhawuk, informed by cultural and cross-cultural research, appeals for Indian organizational psychology to stop courting Western ideas and methods and to investigate models developed from traditional cultural sources such as the *Bhagavad-Gītā* or Indian folk proverbs; (Ch. 26) L. S. S. Manickam describes major obstacles seen as impeding the development of an indigenous Indian

psychology, provides suggestions for remediation, and advocates retaining the original meanings of traditional constructs rather than distorting them as sometimes occurs in research and application; (Ch. 27) J. L. Kristeller and K. Rikhye review meditation research in contemporary psychology (largely non-Indian work), emphasizing "mindfulness" research (broadly defined) and the authors' model of meditative effects, viewed from the perspective of meditational development within the individual and its clinical application; includes extensive research-related bibliography; (Ch. 28) H. Motoyama endeavors to explain, in extended, detailed discourse, the evolution of the Buddha's consciousness until *satori* was attained; (Ch. 29) E. Taylor discusses William James' remarks on "pure consciousness" and Samādhi (and mystical states more generally), considering how they might relate both to the philosophically divergent Advaita-Vedānta (monist) and Sāmkhya (dualistic) schools of thought and to James' philosophical ideas and development; (Ch. 30) A. C. Paranjpe provides a concise, but well-developed and clearly stated, description of the unusual course of spiritual development and the clear, nonabstract spiritual teachings of Sri Ramana Maharshi, a well-known spiritual teacher whose instructions for achieving self-knowledge differed from the traditional ones in Advaita-Vedānta, despite his identification with that tradition; and (Ch. 31) Charles Tart explains in detail his proposal for state-specific sciences and discusses problems that can arise in developing and implementing it; he concludes with strong cautions about how respondents' personal predilections and biases can muddle research and threaten the validity of its conclusions. The volume includes a guide to pronunciation and transliteration of the Sanskrit alphabet and a high-quality, extremely useful, glossary of non-English terms with page references (credits for this work appearing in the Preface).

How Well Does This Volume Inform Regarding Traditional Indian Psychology?

This volume merits very high marks on its breadth and variety of coverage of traditional Indian schools of thought and selected scriptural sources, and the level of scholarship contributed by its authors. Understanding such exposition was not always easy, though, for some of the topics expounded were at once highly complex and abstract, requiring steady, determined persistence to pull oneself through some dense material, the more so in the chapters where there was evident a need for copy editing to remediate problems of expression (possibly related to language), punctuation, grammar, and spelling. The difficulty of reading and understanding created by the number, complexity, and/or abstractness of the concepts presented was exacerbated at times by troubling amounts of needless repetition, both within some chapters and between them. There are several chapters that score highly both on the quality of writing and on freedom from bothersome repetition.

Be advised that much—possibly a substantial majority—of the material in this volume can most properly be considered philosophy rather than psychology, even if it be considered "philosophy of mind." Discussion of that kind may have little a priori interest for some psychologists, but reading at least the foundational chapters of this kind would seem very important for those planning research on traditional Indian psychological topics or who would wish to provide psychological services for clients identified with any of these belief systems.

The divergence and number of traditional philosophical perspectives reviewed in these chapters make this a well-balanced and generous volume. In "Yoga Psychology and the Sāmkhya Metaphysic" (Ch. 12) Eugene Taylor and Judith G. Sugg endeavor to redress a perceived historical imbalance of information about yogic philosophy by noting that although most of the information in the West on yoga seems to reflect monistic Vedāntic philosophy (i.e., the Advaita form), Patañjali actually conceptualized yoga, in his foundational treatise, in terms of the Sāmkhya system, a dualistic school. In several other chapters the reader will be apprised of the deep and fundamental philosophical differences between Buddhism, with its pragmatism that eschews metaphysical speculation, and the Hindu orthodox systems, which rely on it, even while across those orthodox systems there are some fundamental differences of metaphysics.

Because of the presence of widely differing philosophical viewpoints in this volume, some readers might have wished for, at its end, a summative, integrative chapter, perhaps by K. R. Rao (senior editor), to pull things together with something of an overarching perspective, possibly one exemplifying Eastern dialectical thinking, rather than Western thinking, which tends to ask, "Which view is right?"

How Well Does This Volume Inform and Foster Research?

In terms of providing useful resources on relevant scientific research, this volume, viewed in cross-chapters perspective, is a bit of a mixed bag. There are several chapters with from good to excellent access to relevant, suitably contemporary, material in refereed journals or scholarly scientific books, but in other chapters, authors' psychology-related references are largely or entirely substantially dated works, and there is, in some chapters, citation of a number of popular sources. Some authors seem to think of the writings of Freud or subsequent psychodynamic writings as the core of psychology.

There are many appeals for research related to traditional Indian (or "indigenous") psychology, for research that breaks away from the much-deprecated (alleged) effort by many Indian psychologists to think and work in the mold of Western psychology. There are complaints that some Indian psychologists use methodology and assumptions inappropriate to the Indian ethos. There also are protests against Western psychology in general, and, in particular, against its alleged metaphysical materialism and

prohibition of introspective data. These circumstances are deemed to have retarded the development of an "indigenous" Indian psychology, which, it is said, must investigate "consciousness."

But how does one find problems that deserve investigation by a renovated Indian psychology, specifically, problems that will advance our understanding of human personality and of our world (i.e., basic research)? That kind of research is needed for the conceptually expansive Indian psychology that is being sought. Finding ideas for strictly applied research—research without the goal of understanding—may seem considerably easier because so much of traditional Indian psychology is technique oriented, regardless of the school of thought. But after finding that a technique is useful or not, it might be even more useful to know why that is the case. Finding problems for basic research relative to Indian traditions is not easy because effective basic (i.e., process-investigating) research is built upon empirical observations and, usually, upon empirically-grounded models and theories, not upon the essentially metaphysical suppositions of traditional sources, suppositions that were not intended by their contributors to guide empirical research. It may be easier, though, to identify problems related to the epistemology of traditional Indian psychology, because there is much prior research on anomalous cognition (e.g., ESP) and many leads to follow (see W. G. Braud's chapter).

A potential research topic, not discussed in this volume, concerns investigating who best succeeds with which kind(s) of spiritual discipline or meditation. Various eminent Indian spiritual teachers (in the Vedic tradition) have acknowledged that many paths lead to the same goal (i.e., to spiritual realization) and recognized that "no shoe fits all feet" (my wording). This refers to what contemporary personality psychologists call person x situation interaction. The success of a given form of spiritual discipline could depend, in part, on the type of person trying to use it. Investigation of this problem might (a) substantially benefit application interests and (b) meaningfully advance understanding. Identifying the kind(s) of persons who succeed best with a given approach (and who do poorly) might provide important clues as to why (or in what manner) a certain technique gains its success. This would combine both experimentation and the study of personality. The investigator might first develop a hypothesis about how a particular discipline creates its known effects, and deduce, from that hypothesis, using information from personality studies, which kind(s) of persons, with which attributes, might most readily profit from it.

It seems unfortunate that this volume lacked one or more extended, well-developed chapter(s) focused specifically on and illustrating how basic-research problems with Indian-psychology relevance might be identified and actively pursued through the process of careful, methodologically suitable, culturally-appropriate research. To "seed the mind's soil," such a chapter might describe some highly Indian-tradition-relevant research problems and discuss how they might be addressed by research. For example, a

research problem could be discerning the basis (bases) of a widespread folk belief that spiritual teachers sometimes take on themselves the "bad karma" or suffering of a disciple, thereby reducing his or her suffering. Two logically-not-incompatible hypotheses might then be investigated: (a) that the folk belief is correct and (b) that these folk beliefs serve to obviate the *angst* created by believing that the spiritual teacher is suffering self-generated "bad karma." Hypothesis-appropriate research methods then would be developed. The much-needed chapter in support of basic research could benefit by discussing in some detail one or a few problem-specific research agendas, going all the way from finding a suitable problem to generating explanatory hypotheses, and, thence, to the development and/or deployment of methods suitable for addressing those problems.

In only a few chapters is there substantial or detailed discussion of empirical research. The empirical research most discussed is meditation work, which is common nowadays, even in the West, where "mindfulness training" is in vogue.

The final part (pp. 207–214) of "Yoga Psychology: Theory and Application" (Ch. 10) by K. Ramakrishna Rao and Anand C. Paranjpe discusses meditation research, including some potential pitfalls in doing or interpreting it. There also is speculation about free will and that meditation might enhance it, as potentially evidenced by psi interactions.

William L. Mikulas in (Ch. 8), "Buddhist Psychology: A Western Interpretation," thoughtfully considers important conceptual ambiguities in some reasonably contemporary mindfulness-meditation research. His highly recommended discussion of this is very germane to planning meditation research (see, e.g., his section, "Confusion and Confounding," pp. 148–149). The discussion could have been even more useful had Mikulas suggested specific strategies for remediating these ambiguities.

Jean L. Kristeller and Kobita Rikhye in "Meditative Traditions and Contemporary Psychology" (Ch. 27) contribute a lengthy, at times repetitive, but highly useful chapter that addresses historical and conceptual issues and summarizes major meditation-research findings, organized according to particular effect-type domains. It also provides an extensive bibliography of meditation research, especially mindfulness work. I do not see this chapter as providing highly specific pointers to potentially productive problems in meditation research. Readers contemplating research in the area or wishing to be able to evaluate it might have profited by some in-depth discussion of major methodological issues, along with examples of methodological difficulties in published work and with suggestions for their remediation. As a supplement to Kristeller and Rikhye's discussion, readers may wish to consult the journal *Emotion*, Volume 10, Number 1, February 2010, for its special 91-page section entitled "Mindfulness Training and Emotion Regulation: Clinical and Neuroscience Perspectives," published by the American Psychological Association and with Special Section Editors Adam K. Anderson, Amishi Jha, and Zindel V. Segal.

An example of developing focused, meaningful, research related to cultural factors affecting behavior in Indian organizations may be found in Dharm P. S. Bhawuk's "Toward an Indian Organizational Psychology" (Ch. 25). His proposal builds on prior cross-cultural research, and conceptual rationales are provided for predicted outcomes.

L. S. S. Manickam's very thoughtful chapter, "Research on Indian Concepts of Psychology: Major Challenges and Perspectives for Future Action," was not intended to focus on the finding of research problems for the new Indian psychology.

Charles Tart's chapter ("Altered States of Consciousness and the Spiritual Traditions: The Proposal for the Creation of State-Specific Sciences") describes and explains his innovative and controversial proposal for studying states of consciousness. Tart is to be commended for his dedication and care in explaining this concept and for his intellectual honesty in recognizing and his candor in describing in this chapter some difficulties that can confront this proposal's actualization as a research tool.

His chapter's final section, "Challenge to a Future Indian Psychology" (pp. 604–606), constitutes a strongly worded, cautionary, mini-essay very relevant to some of the research interests of this volume. It begins with a warning that investigators could be very misled if they assume that observed agreement of reports, across members of a community of belief, about the reality they experienced during an altered state (developed for the purpose of accessing that reality), is valid evidence of their having been in contact with such a reality during that state. This is because these aspirants' meditative experiences, as well as their subsequent recall and reporting of them, may well have been biased by their previous cultural learning, by expectations induced through training, and by social inhibitions against discordant thinking and reporting. Tart emphasizes that these circumstances might strongly bias, even quite unconsciously, one's judgment, evaluation, and attention during meditation, making things seem different than they actually were.

Tart's cautionary note next becomes more generalized. Although he notes some very practical advantages of working in India in pursuit of a fuller understanding of human nature and reality, he voices a strong, general caution: "On the other hand, millennia old traditions, especially as they become implicit assumptions and biases, can severely limit observation, thinking, motivation and action, so they are a major disadvantage" (p. 605).

What may follow from Tart's admonition is that, despite our best efforts, culture and even local-group considerations likely will in some degree shape what we find through our empirical research. Therefore, getting a bigger picture, a perhaps deeper envisioning of reality (and of how to approach it), may require looking more broadly than in just India, the East, the West, or through the window of any single region, religion, or cultural milieu. By casting widely our investigational net through eclectic research,

we eventually may discern, more completely and more validly, both reality and the route to helpful applications. More need be said, though, about the possible consequences of cultural filtering. They may not all be bad. The unique filtering of reality by individual cultures sometimes may highlight information of unique and special value about the potentialities inherent in reality (and about special application-related possibilities). Because each cultural filter shuts out certain things but admits others, it potentially may cast into clearer perspective, unique and valuable information that gets through it. It may make it more salient, more noticeable. The problem arises if we imagine that the special vision of a given culture is all the truth or the only truth. I suspect that the unique spiritual genius of the famous Indian mystic, saint, and spiritual teacher Sri Ramakrishna, derived from his having set out in his personal life to experience non-Hindu religions (i.e., Islam and Christianity), as well as a series of different perspectives from his native Hinduism. I suspect that similar reasoning was behind the genius of the present volume's very eclectic selection of chapters but suggest that a full flowering of the perspectives-expanding psychology it envisions ultimately may be enriched by and even require a still more culturally inclusive perspective than is afforded in this volume.

Parapsychological investigation may play a major role in how the envisioned new science enriches and deepens our understanding of ourselves and of the world. William G. Braud's superb chapter, "Patañjali Yoga and *Siddhis*: Their Relevance to Parapsychological Theory and Research," is wonderfully informative and thoughtful. It adds substantially to this volume's foundational value for anyone interested in psi phenomena as they relate to traditional yogic thinking. This tour de force: (a) summarizes succinctly the philosophical underpinnings of Patañjali's *Yoga-Sūtras*, (b) explains Patañjali's views of the meditational processes and cognitive factors underlying the supposed psi or other remarkable events (called *siddhis* or "attainments") deemed possible through yogic discipline, (c) provides a remarkably broad and useful thumbnail sketch of contemporary parapsychology (with important references), (d) characterizes selected parapsychological theories (or models) and related research findings with potential relevance to Patañjali's discussion of *siddhis*, (e) discusses alternate interpretations of Patañjali's discourse on *siddhis*, and (f) philosophizes about the broader personal implications of such matters. For parapsychologists this chapter may be among the most important in this volume, and it is specific enough, including references to primary sources on research and theory, that it may motivate and help inspire some psi (and other) research related to yogic discipline.

To my surprise, I noticed in that chapter an error regarding my own work, "Reviews and meta-analysis of research findings indicated that hypnosis was conducive to receptive psi functioning (see Braud, 2002; Honorton, 1977; Schechter, 1984; Stanford and Stein, 1993)" (p. 230). That is incorrect, relative to the Stanford and Stein meta-analytic work. We

indicated that, due to several complicating factors revealed by our meta-analysis, no substantive conclusion justifiably could be made about the possible role of hypnosis in facilitating ESP-task performance (Stanford & Stein, 1994; Braud cited a convention proceedings, but our conclusions there were no different than in the cited journal paper).

At least 13 chapters in this volume mention either psi cognition or action (e.g., PK) or use equivalent words. Such events receive mention in the context of their potential emergence—desired or not—at some stage of spiritual practice that includes meditation. Whether the philosophical context of the chapter was Buddhist or Hindu, the discussion acknowledged that extraordinary, nonsensory cognition (or even paranormal action) was likely to emerge as meditation develops. There was mentioned the risk of such events detracting the aspirant from spiritual objectives or even contributing to egoism, which is deemed adverse to spiritual development. These cautions were attributed to Indian spiritual teacher(s) or to published, traditional spiritual resources. The emergence of such events in the course of meditation seemed everywhere regarded merely as a signpost along the road of meditative development, not as a legitimate goal of the spiritual aspirant.

There is, in this volume, a surprising gap in coverage of extant scientific research relevant to psychological ramifications of Indian spiritual traditions. There is neither actual description of nor detailed discussion concerning the extensive reincarnation-hypothesis research conducted by the late Ian Stevenson, an eminent USA psychiatrist. Stevenson, along with colleagues, often from India, investigated, in India and in other countries, ostensible memories of past lifetimes and other putative influences, upon body and/or behavior, sometimes presumed to derive from prior lifetimes (see Tucker, 2008, for a sketch of the major foci of such work by Stevenson and for a bibliography of it). Rao, Braud, and Miovic are to be commended for having mentioned and cited some of this work in their respective chapters. However, this work merited detailed review in a separate chapter because, if rebirth actually occurs—still a big "if," in my view, despite the research—its ramifications could be great for understanding human personality, as is suggested in ancient Indian spiritual treatises.

Aside from putative past-life memories, the pages of this volume contain many references to hypothetical other influences, on the individual, of past lifetimes, via samskāras ("impressions") claimed to carry over from the past and subtly, generally quite unconsciously, but sometimes powerfully, influencing thought, feelings, behavioral dispositions, and even gross biological manifestations. Samskāras are among the kinds of supposed mental conditioning from which the Indian spiritual practices are said to liberate aspirants. So what better ground could there be upon which to begin building a viable new psychology informed by traditional Indian psychology? That a non-Indian living in the USA should have taken the lead in this domain makes Stevenson's work no less an outstanding

contribution to the scientific study of claims centrally relevant to Indian indigenous philosophy.

Extended discussion of Stevenson's findings, methods, and responsible, cautious approach to investigation, along with his admirably tentative suggestions about the most reasonable interpretations in the best cases, could have supplied important guidance to future researchers in an area loaded with potential pitfalls. If the reviewer had criticisms and suggestions for improvement of such work, that would have been good, too. Stevenson's work is replete with empirical findings that could suggest hypotheses, relevant to Indian philosophical traditions, that are worthy of continued investigation.

In conclusion, I regard this volume as an invaluable resource and well worth the considerable effort needed to read its diverse and complex chapters that include many new terms from other languages that may represent entirely new constructs. The thought of psychologists potentially learning and benefiting from these ideas so novel to most of them—from the West and possibly even from India—is an exciting one, especially if that reading should inspire some thoughtful research or research-guided applications along these at once ancient and new frontiers.

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PSYCHOLOGICAL SCIENTIFIC PERSPECTIVES ON OUT-OF-BODY AND NEAR-DEATH EXPERIENCES, edited by Craig D. Murray. New York, NY: Nova Science Publishers, 2009. Pp xiii + 240. $79.00 (hardcover). ISBN 978-1-60741-705-7.

Sir William Lawrence Bragg said, "The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them" (Koestler & Smythies, 1969, p. 182). Out-of-body and near-death experiences (OBEs and NDEs) provide clues to a novel way of understanding consciousness, but as Susan Blackmore notes in this book:

> One of the things that depressed me most in my decades of research is the tendency for people (and the media) to divide theories of OBEs and NDEs into two black-and-white types. On the one hand are the "good" (or "spiritual") theories—OBEs mean the spirit can leave the body, NDEs are a glimpse of life after death. On the other hand are the "bad" (or "boring," or "reductionist") theories—OBEs and NDEs don't exist or are "just hallucinations." (p. 55)

Blackmore states that brain research has changed this situation by linking OBEs to measurable processes in the brain, thereby showing that OBEs are real and that our concept of ourselves may be the true illusion. Although I agree that the research is useful, it has not resolved the debate. People with reductionist views regard these correlations as support for their view that OBEs and NDEs are just a function of our brains' ability to trick us. However, these studies are a sign that academia is lowering its resistance to considering these phenomena as legitimate areas of study.

Another indication is Craig Murray's book, which presents data and perspectives from the fields of medicine, neuropsychology, neuroscience, parapsychology, psychology and sociology. Not surprisingly, the disciplines' different epistemological theories about OBEs and NDEs have led them to interpret the same data differently and/or ignore irreconcilable data when constructing their theories.

The Pam Reynolds case is an excellent example of data that are variously interpreted. The experiment was set up very carefully by Dr. Sabom, because the major argument against the survival of consciousness after death was that none of the NDEs had occurred under controlled laboratory conditions. This case is often presented as incontrovertible proof, because she had an NDE while undergoing an operation for a brain aneurysm. Her EEG was flat-line, which is used as the clinical definition of brain death. She also had her blood drained from her brain, was cooled to 60° F, and her eyes and ears were tightly covered. She experienced a classical NDE, the timing of which was nailed down because she simultaneously experienced the actions and conversations occurring in the operating room.

John Palmer's chapter presents some of the controversy about the Reynolds case. One criticism is that part of the NDE occurred prior to the flat-line EEG. Sabom's rebuttal was that the "principal part of her NDE (seemed to) occur later, when her EEG was flat" (p. 167). Another argument has been that a flat-line EEG doesn't truly represent a brain that is no longer functioning. Since the EEG primarily measures electrical activity at the surface of the brain, it is unclear what is going on electrically at a deeper level of the brain. This is true, but it would be a more valid point if she didn't also have the blood drained from her brain and her temperature lowered to 60° F. Even if she had some minor electrical activity under these conditions, to say that the brain created the NDE seems analogous to saying

that one can cook a meal with only the stove's pilot light on. And even if this were possible, it would still require a revamping of the mainstream view of consciousness and the brain.

Carlos S. Alvarado's chapter reviews the psychological approaches to OBEs since the 19th century. He found that most explanations of OBEs assume that they are either hallucinations or a form of depersonalization. Although there are a small percentage of OBEs in which the experiencer sees remote information that is later verified as accurate (veridical information), most psychological theories discount that aspect of these cases because of an inherent skepticism.

Susan Blackmore's theory explains OBEs in terms of "models of reality." She postulates that we usually "choose" a model of reality based upon sensory information, but we can switch models when there is a lack of sensory input. She believes that we can construct a bird's eye view from memory. This poses the question of whether or not people who have OBEs have better visual-spatial skills. Indeed, Blackmore found that OBErs have greater visual imagery skills than non-OBErs, and Cook and Irwin found that OBErs were better than others at imagining a scene from different viewpoints.

Hypnotic suggestibility can also play a role in OBEs. In their chapter, Devin Terhune and Etzel Cardeña review the advantages and limitations of the use of hypnosis as a means of producing OBEs.

Harvey Irwin theorizes that OBEs are due to a disassociation between the sense of self and the processing of somatic events. He also states that "because dissociation is psi conducive it is possible that the out-of-body imagery could incorporate extra sensory information and thereby feature a degree of veridicality not expected of mere fantasy" (p. 47).

Jane Aspell and Olaf Blanke write about the neuroscientific perspective of OBEs, which have been associated with localized brain damage in people with epilepsy, traumatic injury, strokes, and migraines. The brains of people who have had OBEs were compared with those who have had autoscopic hallucinations, or the perception of seeing one's body in extra-personal space. These hallucinations differ from OBEs in that the people do not feel that their sense of self resides in the extracorporeal body. The brain area repeatedly damaged in OBErs is the temporo-parietal junction, a region associated with integration of sensory information from the body. The information includes proprioception and vestibular input, both of which play a role in our bodies' sense of orientation in space. In contrast, autoscopic hallucinations are correlated with damage to the temporo-occipital and parietal-occipital cortex, areas associated with visual processing, but lacking the vestibular input that could create a sensation that one is floating in space.

The research by Blanke and his colleagues corroborates that an OBE differs from a visual hallucination. It also shows the potential value of creating homogeneous categories for NDEs and OBEs. For example,

research on NDEs often includes people who did not die or even come close to dying, as opposed to just those who have died and were resuscitated. Separating them according to phenomenological differences could aid the legitimization of their study, because it makes it easier to draw inferences from the data.

The book also discusses how OBEs and NDEs can have profound effects on people's lives. Pim van Lommel, a recently retired Dutch cardiologist, observed that people appear to acquire enhanced intuitive feelings, a strong sense of connectedness with others and nature, and sometimes paranormal gifts after an NDE. Craig Murray, David Wild, and Joanne Murray present the personal and social influences on OBEs and how the experiences are later psychologically integrated.

Other chapters show how NDEs are influenced by our cultural expectations. Tachibana found that Japanese people report "almost crossing a Sanzu River to go to the world after the death" as opposed to the Christian experience of approaching the "light," which is associated with "God" (p. 122). Murphy found that "hell and torture" are common themes in Thai NDEs.

In summary, OBEs and NDEs provide windows into the understanding of consciousness that can no longer be ignored. There is sufficient evidence that we need to follow Bragg's directive and explore novel ways to think about: (a) the brain and its relationship to consciousness, and (b) our sense of self and its embodiment. Murray's academic book leaves it to readers to decide for themselves which arguments are the most compelling. The book's main point is not to convince the reader of a particular theory, but rather to convince researchers that anomalous phenomena have much to contribute to our understanding of the full range of human experience.

References

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