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Abstracts of Presented Papers From the Parapsychological Association 57th Annual Convention, Concord, California, USA, August 14–17, 2014

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Edited by Stanley Krippner, Adam Rock, Julie Beischel, Harris Friedman, and Cheryl Fracasso
Christine Simmonds-Moore
We would like to thank the following persons for their work in translating abstracts for this issue of the Journal:
Etzel Cardeña (Spanish), Renaud Evrard (French) and Eberhard Bauer (German).
INVESTIGATION OF TEMPORAL LOOPS AND RETROCAUSAL CONDITIONING VIA USAGE OF PRESTIMULUS ELECTROCORTICAL SIGNALS

Stephen Leslie Baumgart

It is possible to derive scenarios of retrocausal signaling using standard quantum mechanics. Though a technological realization has not yet been developed, there is extensive evidence of retrocausal communication from experiments utilizing human subjects. Retrocausal signaling brings with it the question of temporal loops, whose physical nature as well their influence on human behavior and perception of the world remains unclear. If, as experiments suggest, unconscious precognition, or presentiment, is a ubiquitous element of human experience, then the question arises as to whether presentiment can be learned. I hypothesize that a presentiment response can be conditioned in human subjects.

I propose that an EEG brain-computer interface (BCI) provides opportunities to test retrocausal signaling and temporal loops. In an experiment reported by D. Radin et al., the prestimulus electrocortical activity of experienced meditators was found to differ significantly in response to light flashes and auditory tones. By mapping the light flash and auditory tone to a binary target and by evaluating recorded prestimulus EEG potentials in real-time, one should be able to predict the state of a future random target, allowing above-chance retrocausal communication.

Moreover, one can trigger the target stimulus based on this prediction, either by triggering the stimulus appropriate to the potential (based on the post-stimulus potential) or its opposite. In my experimental setup, there are three possible operation modes, “prediction,” in which a random binary target is predicted, “feedback,” in which the stimulus with a poststimulus potential symmetric to the prestimulus potential is triggered, and “bilk,” in which the stimulus with a poststimulus potential antisymmetric to the prestimulus potential is triggered.

A human participant can use the feedback of a BCI to train brain activity to exhibit certain patterns. I am now undertaking an experiment to test whether a retrocausal BCI in which a stimulus is triggered by the appropriate symmetric prestimulus response (the “feedback” condition) can condition or affect the response patterns in anticipation to random stimuli (the “prediction” condition). The hit rates for predicting binary targets over runs of prediction mode trials are reported as the primary result. Runs of only prediction mode trials are used as the control. Runs which alternate feedback and prediction mode trials are used for the experimental condition to test whether exposure to feedback trials affects prediction mode trials.

A pilot experiment showed a statistically significant enhancement of the prediction mode hit rate relative to the null hypothesis when prediction mode trials were alternated with feedback mode trials whereas the control runs were consistent with the null hypothesis. It is not yet clear whether this is a learned effect or a kind of resonance-like effect and whether the effect will be robust in future formal experimentation on multiple participants. During exploratory testing an intriguing possible effect was observed on running bilk mode trials on postdecline hit rates; again, more research is needed.

Theoretical and Applied Neurocausality Laboratory
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A SELF-STUDY OF THE ROLE OF MOOD ON OSTENSIBLE PK

Cherylee M. Black & James C. Carpenter

Positive mental states have traditionally been linked to more positive results in tests of PK. Unfortunately, spontaneous PK occurrences do not always engender positive mental states. This long term self-study was used to chart the emotional journey as poltergeist phenomenon gave way to controlled attempts to elicit ostensible PK.

Data from PANAS mood scores and documented attempts to produce ostensible PK in the form of a pin-wheel spinning inside a sealed jar (referred to as “canned PK” or CPK) were used to demonstrate the relationship between measures of emotion and successfully generating CPK. The first successful attempt to produce CPK did not occur until the fourth month of the study period. After that initial success, CPK became robust enough within a few weeks’ time to allow for more formalized measurements of increased skill levels attained over time. Positive affect, joviality, self-assurance, and surprise were all associated with positive results in achieving CPK both in the entire study period as well as during the period limited to only after the initial CPK event took place. A negative correlation with accomplishing CPK was shown with regards to negative affect, fear, and guilt, but only when the entire study period is taken into account.

When simply considering the period after CPK was first demonstrated, no significant correlations between negative emotions and CPK were in evidence. Such negative emotions may play a role in preventing controlled displays of ostensible PK, but once CPK has been previously generated, negative emotions may not be enough of a barrier to prevent subsequent occurrences.

There were substantial differences in mean PANAS scores obtained prior to and after the first occurrence of CPK. Negative affect, fear, guilt and sadness all demonstrated significant decreases after the initial CPK event. Positive affect, self-assurance, attentiveness, and surprise all revealed significant increases after CPK was first observed. It seems likely that constructively dealing with emotional issues and going through the process of personal development may have helped to advance the skill of producing CPK on cue, but that doesn’t preclude the possibility that the practice of quietly sitting down each evening to mindfully attempt CPK may have also had a positive effect on such personal development issues.

In contrast to the process of eliciting the phenomenon of CPK, emotional state does not appear to play a role in regards to facilitating an increase of CPK skill level as demonstrated by the ability to generate it in a more timely fashion. It seems likely that continued practice of CPK over time can help to overcome the limitations of emotional barriers once the initial resistance to performing ostensible PK has been surmounted. The findings of this work suggest that if the resistance to eliciting ostensible PK on demand can be overcome, with continued practice it may be possible to maintain the production of positive results.

Rhine Research Center
Durham, NC, USA

A STUDY OF DMILS ON SMALL ANIMALS AND POSSIBLE SYNCHRONOUS ENVIRONMENTAL EFFECTS

Jeannette E. Briggs

In this research project a single volunteer who made himself known to the Rhine Research Center (RRC) in 2013 will use direct interaction on a living system (DMILS) to put small animals to sleep or into a sleep-like state. When he contacted the RRC, the volunteer claimed he could use a self-taught method of collecting energy in his lower abdomen that he then would direct to the immediate proximity of animals as a treatment of DMILS. The animals to which he directed the energy were either near him or viewed from live stream video feeds, photos, or video recordings. Once the directed energy built up near the animals he claimed it formed a vortex and manipulated the chi surrounding the animals in such a way as to put them to sleep or into a sleep-like state. He claimed to have performed these actions with 100% success on an almost weekly basis for one year prior to contacting the RRC.
When the volunteer performed this type of DMILS on animals, certain possibly synchronous environmental changes were noted near him and were observed when the video recordings of these sessions were viewed. For example, the friends and family of the volunteer, as well as the volunteer himself, noticed an increase in heat during the DMILS sessions. When an experienced bird handler viewed video recordings supplied by the volunteer as proof of his ability, many of the animals, especially birds, seemed to exhibit behaviors signifying an increase in temperature in their surroundings. Another example of potential synchronous environmental changes concerns the presence of ultraviolet (UV) light. Increased UV light emissions of chi masters have been documented and since the volunteer claims to be manipulating chi he might also be emitting UV light. When the video recordings of DMILS sessions facilitated by webcams were evaluated, it was noted that many animals, especially certain birds whose visual spectrums extend into the UV range, appeared to focus on a seemingly empty space between the webcam and themselves, possibly indicating the increase in localized UV radiation. Because of these potentially synchronous environmental changes, various methods to determine changes in heat and UV light will be used.

The experiment will be partially automated, blind, and randomized. Changes in heat, UV emissions, and random event generator data will be measured and recorded at both the source and treatment sites. In addition to analyzing data to evaluate the success of the application of DMILS on animals by the volunteer, environmental data will be examined for synchronous changes possibly indicating the occurrence of entanglement and nonlocality during this type of psi phenomenon.

Rhine Research Center
Durham, NC, USA

VISUAL CATEGORIZATION OF IMAGES OF LIVE AND DECEASED INDIVIDUALS

Arnaud Delorme, Alan Pierce, Leena Michel, & Dean Radin

After over a century of personal reports and research, the process and interpretation of evidence from mediumship studies remain unclear. This research project seeks to further our understanding of behavioral and electrocortical correlates associated with mediumistic abilities. In the experiment, participants view and classify photographs of people according to whether they think the depicted person is currently alive or dead. The first part of the experiment is performed online and the second part is performed in our laboratory with 12 individuals while EEG measures are taken.

We have set up a database of photographs of 200 individuals, half of whom were deceased. We balanced sets of features for the two groups of alive and dead individuals (including gender, age, gaze direction, glasses, head position, smile, hair color, and picture resolution). The same images were used for both the online and the laboratory experiment.

For both the online and the laboratory experiment, the participants’ task is to indicate if they think/feel that a given individual is alive or dead. Participants see pictures of individuals one by one on a computer screen and indicate if they think/feel they are alive or not by clicking on the corresponding buttons below the photograph. We have obtained preliminary results for the online experiment on 100 individuals (recruited from a private mailing list of individuals interested in mediumship). The average image classification rate was 50.9% with a 95% confidence interval of 50.6% to 51.3%. We also had robots (i.e., computers) performing the task. The robot’s performance was 49.7% with 95% confidence interval of 49.4% to 50%. The odds of obtaining such a result are 1 out of 150 in the direction of the hypothesis.

These results suggest that people might be able to sense whether the person depicted in a given image is alive or dead.

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A MIXED METHODS EXPLORATORY STUDY OF ALLEGED TELEPATHIC INTERSPECIES COMMUNICATION WITH DOMESTIC DOGS (*CANIS LUPUS FAMILIARIS*)

Deborah L. Erickson

This research explored the proposition that alleged telepathic interspecies communication, or human telepathic connection to animals, may be possible by learning simple contemplative/meditative techniques to quiet the mind and shift consciousness. Telepathy is a controversial topic not accepted by mainstream science, despite multiple research efforts over many years that have shown positive effects. This research is based on the hypothesis that the process to shift cognition into a pattern similar to daydreaming, or to the hypnagogic and hypnopompic states on the edge of sleep, may allow for a consciousness alteration that may enable telepathy.

The research explored the following questions: When a telepathic animal communication session was conducted with a domestic dog (*Canis lupis familiaris*), was accurate information received by the researcher for quantitative questions as judged by the human guardian on a Likert Scale of 1 (“entirely inaccurate”) to 6 (“entirely accurate”)? What qualitative comments did the human guardian have for other information received by the researcher from the animal related to the questions posed by the guardian? What was the overall session accuracy rating as judged by the human guardians? And finally, what common issues were raised by the guardians’ qualitative questions and comments?

This research completed 50 alleged telepathic animal communication sessions conducted by the researcher over the telephone with a human guardian and their domestic canine. All guardians were physically located in another city or state from the researcher; 36 cities and 35 breeds or mixed breeds were represented. Five standard questions were asked of the animal, three of which were quantitative and rated by the guardian; the remainder of the session was devoted to the guardian’s questions for their animal. The five standard questions were: (a) How many humans do you live with? (b) Have you lived with your current guardian since you were young? (c) What is your favorite food or treat(s) you get now? (d) What do you like most about your life? and (e) What would you like to change about your life?

All sessions were recorded via a conference call service and the recording was available to the guardian after the session. The researcher transferred all questions asked and responses allegedly received telepathically from the animal during the session to an online survey form. The guardian judged the accuracy of the information received by the researcher on the survey form after the communication session was completed, independent of further contact with the researcher.

The average overall accuracy rating for all 50 sessions was 5.12, indicating a possibility that accurate information was received by a human from a canine via a telepathic human-canine connection. An analysis of the qualitative data indicated several common canine issues, such as canines reactive to other dogs in public, canines reactive to strangers entering their home, conflicts between canines in a multi-dog household, affection between canines in a multidog household, physical health issues, and emotional/spiritual support issues.

The results of this study contribute to the body of research into animal cognition and consciousness. The findings further contribute to interanimal empathy studies as well as the wealth of literature that supports the human-animal bond. Further research is needed to explore additional nuances of this category of parapsychological research.

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CHILDREN AND THEIR PARENTS’ EXPERIENCE WITH AFTER-DEATH COMMUNICATION

Kimberly N. Jeska

After-death communication (ADC) is a subset of an overarching umbrella of what can also be referred to as an exceptional human experience. An ADC can also be classified as paranormal, indicating that which reaches beyond our physicality and tangible nature and transcends our dimension of earthly reality. ADCs can be direct or indirect
communications between the earthly world and other dimensions of reality. These contacts are a reciprocal engagement between the physical, living person and the spiritual, non-physical being. ADCs have also been referred to as apparitions, afterlife encounters, ghosts, and spirits, along with many other societal and scientific labels. Researcher intention for this study focused around the deeper meaning, understanding, and exploration of the lived experiences of the children and parents. Previous literature had not addressed the parent-child interrelationship and individual experiences with apparent after-death communications occurring in the present moment. Additionally, prior studies have often portrayed adults that have reflected back to childhood around spiritual experiences. A forum was provided for the participants in this study to express themselves with the potential for healing and integration of their experiences. This qualitative study included an Intuitive Inquiry and Thematic Content Analysis that incorporated a three-part procedure: (a) parent interview; (b) child interview (optional); and (c) creative expression art work component with the child (optional).

Results from the 14 participants indicated finding purpose and meaning in the experiences. These interactions appeared to co-create further understanding of death, validity of experiences, and potential for a life mission. Many unexpected findings unfolded from the study as only mother participants came forward. These female parents provided a wealth of understanding into the intricacies of living with ADCs throughout the generations and into the current lifetime with their children. The depth, perception, and awareness by both parents and their children portrayed the participants’ vulnerability around their ADCs into a more public awareness and understanding about what they are actually going through and desiring from their communities. The need for further education for parents that have children experiencing after-death communications was identified as well as for the community. Public education around this topic was an aspect that the parents requested to bring further awareness, as well as providing professionals with resources and support. Suggestions for how to work with clients is included and the need for additional resources, education, and public awareness for adults and children is greatly needed and desired. The study presents a strong framework offering validity to ADC experiences for children and families abroad. Mental health practitioners, spiritual directors, and educators are presented with perspective and a foundation from which to build a treatment plan and understand our dimensionally growing and expanding capabilities. This research provides a bridge between the earth world and the possibility of other dimensional life and how to move forward in working with our youth and parents in our homes, churches, schools, and other social and nonsocial gatherings/ settings. Personal lived ADC experiences by the researcher resonated with the participants as well as the findings in this study. An opportunity for growth exists for professionals working in the field and provides an expanded frame of reference for guiding clients as opposed to misdiagnosing and/or pathologizing.

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EXPLORATIONS INTO REMOTE VIEWING MICROSCOPIC ORGANISMS (“THE PHAGE”) AND THE EFFECTS OF BIOLOGICAL SCIENTISTS’ EXPOSURE TO NONLOCAL PERCEPTION WITHIN A MULTIDISCIPLINARY APPROACH

Debra Lynne Katz & Lance William Beem

In a double-blind, free-response experiment, 39 remote viewers (“viewers”) were given the task of describing a bacteriophage, or “phage,” a virus that attacks bacteria. Ten of the viewers were retasked on elements described in their first session; 35 viewers were then given a second target containing the task, “describe the trigger for replication.” This time, they were provided with information that the target was of a microscopic nature.

This remote viewing project was unique not only in its subject matter, but in its purpose, which was to evaluate remote viewing sessions not only for evidence of psychic functioning but to make use of the information in order to advance the work of scientists outside the field of parapsychology. Viewers submitted a total of 83 sessions, producing a total of 3,263 descriptors to be analyzed. They also provided dozens of detailed sketches, some of which are presented here.

Author Lance Beem is a biologist specializing in plant pathology, physiology, entomology, and nematology.
for 30 years; he was also trained as a remote viewer. He recruited expert virologists over a period of 2 years and reported his interactions with them. Five of 16 virologists that were approached agreed to participate as raters. Two offered to assist by providing a student to rate the remote viewing sessions. Eight refused, one calling the project “pseudoscience” before examining the data or expressing any interest in understanding what it entailed.

In all, four methods of analysis were employed, including what herein will be referred to as a “big data corroborative approach.” This method has not been used previously for evaluation of remote viewing data, although similar approaches are being used in other fields to evaluate large data sets for the purpose of making predictions and assessments.

Our primary phage expert, Dr. Julian Roberts, a molecular biologist, conducted a qualitative analysis of the remote viewing reports. He wrote: “At first appearances these data appear to show nothing more than some musings. On further inspection, however, I am convinced that they describe bacteriophage, and the uses of bacteriophage. This is my professional opinion as a scientist and a professional and impartial observer.” Off the record, he stated, “This blows my mind. How is this possible? It’s scary.”

All 39 viewers also participated in an extensive biographic survey evaluating their past remote viewing experience, methodologies, preparation techniques, number of words used in a session, time spent on sessions, and so forth. These survey data were compared with those sessions that received the highest and lowest accuracy ratings to help draw conclusions about what led to the most useful sessions when tasked with topics that are not typically explored with remote viewing. The survey suggested that experience with remote viewing, spending more than 30 minutes on a session, and using a training approach known as “controlled remote viewing” seemed to produce the highest-rated sessions, but none of those factors guaranteed success.

This project’s proposal was the first recipient of the International Remote Viewing Association (IRVA) War-collier Award (2011). It took over 3 years to complete in addition to 2 years of preliminary experiments involving thousands of trials in which remote viewers attempted to blindly identify the presence of the tomato mosaic virus in plants. The entire project required the help of 10 volunteers, and in all, 57 people were involved.

PRE & POST EFFECTS IN BIO-PK EXPERIMENTS

Hideyuki Kokubo \(^1,2\) & Takeshi Shimizu \(^2\)

**Background.** Since 2006, the present authors and other collaborators have been studying bio-PK using pieces of fruit of *Cucumis sativus* “white spine type” cucumber as a bio-sensor, and developed three methods to measure the magnitude of bio-PK quantitatively. Through their biophoton measurement method, they have reported that: biophoton intensities of experimental samples of cucumber pieces were larger than controls if healers did noncontact healing (bio-PK) for 30 min to increase biophotons emitted from samples; the difference of intensities between experiments and controls arose about 5 h after healing and the difference could be approximated by a two-step reaction; effects of noncontact healing differed from those of magnetic stimuli; and one of the production mechanisms of biophotons was biosynthesis of green odor. Their gas measurement method originated from their study of biosynthesis of green odor. Cucumber gases consist of C9/C6 aldehyde and alcohol such as violet leaf aldehyde and cucumber alcohol. 2-hexanol, one of the components of cucumber gases, which has a color reaction in a short-term gas-measuring detector tube (141L, Gastec, Japan). This was used for detection of healing effects in the gas method. They also have reported that there was a negative correlation between biophoton intensity and gas amount. And they were able to make the world’s first quantitative measurements of spatial distributions of healing power around a human body using bio-sensors arranged at equal intervals. In their most recent experiments (cucumber series Nos. 18 & 20), the effect of noncontact healing was measured using the gas and fluorescence measurement methods. They also discussed three kinds of healing ways in energy medicine. In addition, the cucumber series Nos. 18 & 20 consisted of two healing trials and a pair of pre- and post-dummy trials, and the authors also obtained results that dummy trials detected anomalous effects with a \(J\) value of about 0.1.

**Method.** They executed cucumber series No. 21 under the same conditions as Nos. 18 & 20. Experiments were done at the Institute for Living Body Measurements of the International Research Institute (IRI) from February 20 to April 7, 2013. Participants were five healers (two males, three females; age range of 31-72 years, with an aver-
age age of 46.6 years) who were energy medicine type healers recruited through a list of known participants, SNS, and the internet. Each of the healers did a 30-min healing trial twice a day, but they tried to change their healing ways for the second trial. In this series, an RNG was set under the top of the table where target samples were placed.

Results. The average $J$ value of the dummy trials was obtained as $J = 0.106 \pm 0.084$ (95% confidence interval, $n = 20$). The authors reconfirmed detection of anomalous effects. The average $J$ value of healing trials was $-0.061 \pm 0.094$ (95% confidence interval, $n =19$). Moreover, the average $J$ value of blank tests was nearly equal to zero ($J = 0.002 \pm 0.061$, 95% confidence interval, $n = 22$); blank tests were done on other days than the days of healing and dummy trials.

Conclusion. The authors concluded that dummy trials cannot be considered as blank tests if the dummy trials are done 1-2 h before or after healing trials. Anomalous effects seen in the dummy trials were speculated to be a kind of distance effect caused by such behavior as healers’ attentions being directed towards the experiments. However, further studies are needed to identify the reasons for these anomalous effects.

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FIVE QUALITATIVE RESEARCH METHODS AND THEIR APPLICATIONS IN PARAPSYCHOLOGY

John G. Kruth

Despite over 75 years of laboratory work, parapsychology continues to struggle with the basic questions about the nature of the psi experience and the development of a comprehensive theory that encompasses all psi phenomena. Laboratory techniques are limited due to the deductive nature of the studies and the strict barriers that are constructed between the researcher and the phenomena being investigated. The dispassionate nature of an experimental researcher is a benefit in laboratory work, but it does not allow the experience of the study participants to be included in the research process.

Parapsychology would benefit from the inclusion of various qualitative research techniques in the quest to understand the nature of consciousness and to help to develop a theory of psi and survival.

Five qualitative research methods are presented including information about data collection methods, data analysis techniques, sampling, reporting, and validation. Each of these techniques is described separately along with discussions about why a researcher might choose to utilize a qualitative research method rather than follow a strict experimental or quantitative protocol. The qualitative methods outlined are narratives, case studies, phenomenology, grounded theory, and ethnographies.

Finally, multiple examples are provided from the field of parapsychology which discuss how each method could be applied to help to advance the understanding of the psi experience and to contribute to future quantitative studies. Parapsychological researchers are encouraged to develop a better understanding of the value of qualitative research techniques and to utilize the results of qualitative studies to structure and develop better experimental methodologies in the laboratory setting.

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TAMING THE GHOST WITHIN: A CASE OF ELECTRONIC POLTERGEIST ACTIVITY

John G. Kruth

From the earliest reports of poltergeists, the experiences of the agents and those close to the activity have been uncomfortable at best and terrorizing in the most extreme cases. Early case reports were centered on religious activities and demonic possession, and the treatment for the affected person or location was typically an exorcism
or another religious rite. Since the late 19th century, parapsychologists have focused on examining the phenomena first to demonstrate that they exist, and second to understand the mechanisms that may enable the poltergeist activities. These are both important and essential approaches to contribute to the advancement of the science, but these approaches do little to address the needs of the people who are most involved with the poltergeist experience.

With the proliferation of electronic technologies, a new type of poltergeist is arising in the 21st century. RSPK is appearing as electronic disturbances in everyday devices and the disruptions in wireless signals. This paper presents a case investigation of an “electronic poltergeist disturbance” (EPD) along with the evidence that was observed during a field investigation using a controlled methodology. The investigation provided strong evidence of an RSPK agent, and numerous electronic disruptions were observed by the researchers. Despite this clear evidence of the poltergeist activities, the focus of this investigation was less on proof-oriented research work and more on addressing the needs of the poltergeist agent and his family.

As more electronic devices permeate our world, it is probable that more poltergeist activities will take the form of EPDs, and it is also possible that the phenomena will be recognized more often in the general population as electronic devices are more closely integrated with the human body. A growth in EPDs will put a strain on the agents and the people around them as electronic devices that are used in daily life begin to malfunction or even completely break. The investigation described in this paper put a significant strain on the agent and his family, and an intervention was necessary to assist the agent in alleviating the stress caused by the events.

This paper describes a process that helped the agent to successfully reduce the number of RSPK events and eventually to nearly eliminate the activity. An approach which emphasizes stress reduction using relaxation and visualization techniques, mindfulness, and a redirection of attention to a focus object is presented and it resulted in a reduction of electronic disturbances near the agent within a few weeks. The 10-step technique is outlined as a starting point for future research into methods that will address the needs of the poltergeist experiencers and to help to address an issue that may grow in importance as society integrates more electronics into the daily human experience.

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IGNORANCE IS BLISS? EXPLORING PARANORMAL BELIEFS, COPING AND HAPPINESS IN A UK AND SINGAPOREAN SAMPLE

Nicola Lasikiewicz

Previous research has indicated that belief in unusual phenomena and superstitious thinking may increase in times of stress. Further, believers in the paranormal often display avoidant coping strategies with little to no problem solving. These findings may, therefore, reflect a specific coping mechanism for stressful situations. However, little research has explored the possible interaction between the perception of stress and coping style on belief and further, an assessment of perceived happiness. Consequently, the current study aimed to explore possible associations between perceived stress and happiness, coping, and paranormal belief. Further, these relationships were explored and compared in a sample of Western (UK) and Southeast Asian (Singapore) participants.

Ninety-two male and female participants aged between 19 and 61 years (mean age = 36.56 ± 11.74 years) from the UK and 145 male and female participants aged between 18 and 57 years (mean age = 23.03 ± 5.51 years) from Singapore completed an online battery of psychological measures assessing paranormal belief (Revised Paranormal Belief Scale), superstitious thinking (Superstitions Questionnaire), perceived stress (Perceived Stress Scale), coping (Ways of Coping Revised) and happiness (Oxford Happiness Questionnaire). Participants were predominantly students recruited using convenience sampling. Data were analysed using a series of backwards enter multiple regression analyses to determine the predictors of paranormal belief.

For all participants, level of education (a higher score being indicative of a lower educational qualification), happiness, and coping were significant predictors of global paranormal belief, $R^2 = .22; F(6, 225) = 10.787, p < .01$. Specifically, a lower level of education ($\beta = .20; p < .001$) greater happiness ($\beta = .16, p < .05$) and coping in the form of low problem solving ($\beta = -.16; p < .05$) and greater positive reappraisal ($\beta = .24, p < .001$) were associated
with greater belief. Further, a lower level of education (β = .13, p < .05) and coping, low problem solving (β = -.18; p < .05) and high distancing (β = .31; p < .001) were associated with significantly greater superstitious thinking, \( R^2 = .21, F(7, 224) = 8.239, p < .01. \)

In terms of cross-cultural differences, paranormal belief was significantly greater in Singaporeans but no significant differences in happiness and perceived stress between the two nations were noted. In terms of predicting belief, level of education was a significant predictor of both global paranormal belief (β = .36, p < .001) and superstitious thinking (β = .25, p < .05) for participants in the UK, \( R^2 = .16, F(3, 85) = 5.345, p < .01, \) and \( R^2 = .12, F(2, 86) = 5.776, p < .01 \) respectively. For Singaporeans, greater happiness (β = .23, p < .001) significantly predicted greater paranormal belief in addition to low problem solving (β = -.19, p < .05), high avoidance (β = .21, p < .05) and greater distancing (β = .34; p < .001) coping strategies, \( R^2 = .22, F(5, 137) = 7.832; p < .01. \) Further, the combination of high perceived stress and low problem solving was also a significant predictor of superstitious thinking in Singaporeans, \( R^2 = .27; F(5, 137 ) = 9.919, p < .001; \beta =-.19, p < .001. \)

The findings support the suggestion that belief in the paranormal may reflect a specific pattern of coping characterised by greater propensity for distancing and avoidance with low problem solving. Further, the combination of high stress and low problem solving may increase the propensity for belief and superstitious thinking, particularly in a Southeast Asian population. Further research is required to determine whether this apparent coping mechanism is protective or maladaptive in the long run and whether cultural differences may mediate this effect.

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THE VOLUME FIELD MODEL ABOUT THE STRONG INTERACTION
AND THE WEAK INTERACTION

Rongwu Liu

The core foundation of natural science is the particle model. Over a long time, the particle model has been challenged in three fields of nature: (a) the physical mechanism of the strong interaction and the weak interaction in the structure levels below atomic nucleus, (b) the start and the end of the universe (that is, the “cosmic egg” and the black hole), and (3) the special life phenomena (including parapsychological phenomena and paraphysiological phenomena).

The quantum chromodynamics (QCD) and the quantum flavordynamics (QFD) established by imitating quantum electrodynamics (QED) haven’t provided a satisfactory form of mathematical description in theory, and they can only explain some experimental facts about the strong interaction and the weak interaction phenomenologically (usually obtained by particle collision in the particle collider). The speculation about the universe’s origin and the black hole by using general relativity has also made the particle model fall into the abyss of the singularities of the universe and the black hole. In addition, the awkward situation that the particle model treats the special life phenomena of the human body has also made people cast doubts on the authority and the range of application of the particle model, because the material ideas that the special life phenomena of the human body manifest are incompatible with the physical concepts of the particle model.

This article systematically proposes a group of new physical concepts which are totally different from the conventional physical concepts: volume field (corresponding to particle) is a form of material existence in plane space (corresponding to point space). It takes volume-changing motion (corresponding to displacement motion) in the form of noncontinuous motion (corresponding to continuous motion). The volume fields interact with them by overlapping their volume fields (corresponding to exchanging intermediary particles). On the basis of the above concepts, this article further proposes the complex structure models of the atomic nucleus (including the structure levels below) and the organism, the plane space model, and the periodic jump function model.

Based on these models, this article ultimately formulates a volume field model uniformly describing the strong and weak interactions both in the atomic nucleus and in the organism, consequently bringing the special life phenomena of the human body into the frameworks of the strong and weak interactions. In addition, this article also proposes a model of space-time conversion based on the volume field model, and suggests that the plane space-
time and the point space-time convert each other by means of rupture and merging, respectively; the essence of the
space-time conversion is the mutual transformation of material and energy respectively. The model of space-time
conversion also brings the cosmological singularities (cosmic egg and black hole) into the frameworks of the strong
and weak interactions, and explains some conventional and superconventional natural phenomena related to the
space-time conversion.

THOUGHTOGRAPHS FROM THE FUTURE AND FROM THE PAST

Lester Franklin Lomax III

Our images showed up in Serios “thoughtographs” taken in Polaroid cameras from 10 and 20 years in the past, from
over 5 years in the future, and from over a thousand miles away from the cameras, identifiable and identified.

A brief description of the strange experience of finding the first image is as follows. In a book written by Dr.
Jule Eisenbud about Ted Serios, I found an image procured in the presence of Ted Serios, of images of my friend
and I, as we were fishing at a lake in California. I had borrowed the book at a local library, on a whim; then I irra-
tionally decided to leave one employee to run a complex repair contract at a bridge jobsite, and I then traveled many
miles to visit the other guy in the image, before I opened and read the book. We witnessed some paranormal events
that attracted my attention to his identifying features, for example, a dark patch of hair behind one ear, surrounded
by blond hair. We both experienced the presence of a person looking down on us from behind us, as well. We felt
a vibration and noise, and, when asked, I verbally identified the effect as being caused by our mutual friend, who
showed up in some of the images looking down at us. I seemed, in hindsight, to have been primed to recognize the
dulled image in Eisenbud’s book.

An especially reliable correspondence between the image in the book and the scene at the site that was
thoughtographed many miles from the camera is that the event occurred on the same date as the experimenters
wrote on the border of the thoughtograph: November 29, 1965. That was the date of the fishing trip, as verified by
our recollection of the event, a very memorable birthday celebration. Even though I did not find the “decoupaged”
image until ten years after the event, we vividly remembered the fishing event that occurred on that date. The bridge
repair job helped to assuage my concern about the bridge in the background of figure 118, (there is no bridge at the
site), until I first saw the bridge, later in 1975, on a trip to Ohio, while traveling over the Missouri River. The col-
lage of features from different times was meaningful to us. These types of experiences are commonly experienced
by some folks. It is time for one of us to share what we have been doing, possibly as a lineage for many centuries!

When I received Dr. Eisenbud’s second edition in the mail, I found an image of another identified fishing
trip that we had just experienced, two weeks before, in it. We had a prepaid charter boat hired to go to the same site
in another two weeks, and we identified that imaged site, as well. That site and our pose was thoughtographed from
20 years in the past. That thoughtographed site is on the “East End” of Catalina Island; satellite views of the site are
available on the Internet. A tall rectangular stone appears to be similar to the stones in the target picture of Stone-
henge. It is still embedded, upright, in the sand in the surf, as of 2006. The site is ravaged by the current and tides. The data indicate that getting exact photographic images of the sites is masked and hindered by the phenomena. An example is that the exterior of the house at one of the scenes was extensively remodeled before I got to it. See Photo #2 in the photos of the island at: Geology.campus.ad.CSULB.edu/people/grantphotos/CatalinaFeb06/156sealrocks-RockQuarryFeb06L.jpg. The snapshots of the site, taken on fishing trips, are missing from my files.

I had used what I call (after others) the “Library Angel” to find books that answered questions, and I found Eisenbud’s book with that method, and I continued to use it for many years, in an attempt to understand how such type events could happen.

I recently found the work started by Dr. Yakir Aharonov, and others, on their mathematically and empirically derived time symmetry quantum mechanics conceptualizations. Their work, for this naïve, but highly experienced layman, provided the encouragement to share my experiences, and my findings, and my explicated tacit knowledge with you. My explanatory scheme, when generalized from the limited domain of Serios thoughtography to the possible thoughtographing of the world, may help scientists understand the nature of time and of the self obscuring the supranatural in our world.

I have contributed a copy of a 46-page background research brief on the conceptualized understanding of thoughtography to the Parapsychological Association. That report, and 14 analyses of the data procured in the 14 Eisenbud/Serios experimental conditions that are in the literature and detailed reports of interest to investigators who would understand the details of the nature of time, are available. The information must be couched in common sense first-person narratives, and personal and scientistic jargon, in order to not overtly attack the useful but naïve materialistic general consensus that there are no supernatural events in this world, yet. I have been working on gaining an understanding of thoughtography for almost 40 years. I believe that the Serios thoughtographical images provide a long series of replicated paranormal data, unlike anything else in parapsychology. It is time for me to pass what I have learned on to others.

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MACHINE-MEDIATED REMOTE VIEWING: AN INITIAL STUDY AND REPLICATION

Erik Maddocks and Garret Moddel

Remote viewing (RV) is well established using human participants drawing or describing perceived information about a location or object, without ever having directly observed it. Psychokinesis (PK) is well established using humans to affect a random number generator (RNG) output. If these concepts could be combined, RV could be carried out using human subjects unconsciously producing images via a local RNG.

An experimental setup was developed that allowed for the combining of RV and PK influence of RNG outputs. The experiment consisted of two collections of data. In one case, the “RV Sessions,” participants were instructed to RV targets in sealed envelopes that were randomly selected by the participants themselves while data were collected from a local RNG device. The other, the “PK Sessions,” consisted of PK data where the participants were instructed to focus or concentrate on visible targets randomly selected by the participants themselves while data were collected from a local RNG. In both cases the data streams from the RNGs were used to construct raster scanned images. These images were later judged, along with the more conventionally produced RV drawings, against five possible targets.

The experiment employed a forced-choice protocol to simplify the judging of the experimentally produced data. In the judging procedure, the experimenters were given a set of possible targets and ranked the targets from most like the machine-created image to least like it. There were always four decoys along with the target, giving a 20% hit rate by chance, over all trials.

This experiment was run in two separate groups. The first group, from a class experiment, had a total of 60 trials, producing a t-score of 2.33 for a p value of .01, one-tailed, and .02, two-tailed. The second group, from an
expanded follow-up experiment, had a total of 128 trials producing a \( t \)-score of 1.68 for a \( p \) value of .05, one-tailed, and .09, two-tailed. The first group of 60 trials showed statistically significant results while the second group of 128 trials produced a less significant result.

The results provide evidence that remote viewing information can be captured with an inanimate system that samples noise. Exploitation of the capability to influence an automated RNG output to produce an RV image might mitigate the psi experimenter effect, so long as the experiment is designed in such a way as to minimize the experimenter contact with the subject and the subject’s experimental data.

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**EVIDENCE FOR TELEPATHIC COMMUNICATION IN A NONVERBAL AUTISTIC CHILD**

Diane Hennacy Powell

Autistic savants have not undergone rigorous scientific investigation for psi, although many of their skills are very psi-like. For example, some give cube roots of six-digit numbers without knowing how to perform simple mathematical functions, such as addition or multiplication, and with no conscious derivation of the answers. These remarkable skills are accepted by science because they are reliably replicated.

By contrast, brief reports by physicians that are suggestive of psi in autistic savants have been ignored or criticized. The psi ability most frequently reported by parents to the author in her research has been telepathy, especially in nonverbal children. In 2013, the author received three homemade videos of a nonverbal, 9-year-old, severely autistic girl that were claimed to demonstrate telepathy. The videos were intriguing, but scientifically insufficient. Two therapists reported telepathic experiences with the girl, creating the opportunity to test both. The author conducted two controlled, 2-hour research sessions with Therapist A, and one 2-hour controlled research session with Therapist B. Randomized numbers, sentences, fake words, and visual images were presented to the therapists out of view of the girl, who was asked to “read the therapist’s mind.” The therapists were asked to write their own verbal descriptions of the images for comparison to the girl’s answers. Random numbers were generated for mathematical equations.

The girl was asked to give all the numbers involved in the equations and duplicate the answers generated by the author with a calculator. The therapist and child could not be tested in separate rooms, because even subtle changes to the environment are very distracting and disturbing for a child with severe autism. The experimental setup required the therapists and child to work with a divider between them. The child typed her answers after choosing them from a stencil. To assess for any possible visual and/or auditory cueing, five high definition point-of-view (POV) cameras and three microphones were strategically placed in the experimental space to capture coverage of the entire room, the therapist, and child, and their separate workspaces. All cameras were synchronized and time-stamped.

Data from the first session with Therapist A includes 100% accuracy on three out of 20 image descriptions containing up to nine letters each, 60 to 100% accuracy on all three of the five-letter nonsense words, and 100% accuracy on two random numbers: one eight digits and the other nine. Data from the second session with Therapist A includes 100% accuracy on six out of twelve equations with 15 to 19 digits each, 100% accuracy on seven out of 20 image descriptions containing up to six letters, and between 81 to 100% accuracy on sentences of between 18 and 35 letters. Data from the session with Therapist B showed 100% accuracy with five out of 20 random numbers up to six digits in length, and 100% accuracy with five out of 12 image descriptions containing up to six letters. There was no evidence of cueing or fraud. The data are highly suggestive of an alternative, latent, and/or default communication mechanism that can be accessed by people born with severely impaired language abilities.

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JOURNAL OF EXCEPTIONAL EXPERIENCES AND PSYCHOLOGY

Erika A. Pratte

This poster abstract is for the *Journal of Exceptional Experiences and Psychology* (www.exceptionalpsychology.com), an online, international, peer-reviewed scholarly journal dedicated to the exploration of exceptional experiences and is, in the words of Rhea White, an effort to discover what could be learned if all types of psychic, mystical, encounter, death-related, peak, and other anomalous experiences were viewed as members of a single class. Exceptional psychology is a field that studies experiences and phenomena traditionally germane to parapsychology and yet goes beyond in breadth. Some examples of experiences under investigation by exceptional psychology include survival after death, out-of-body experiences, extrasensory perception, psychokinesis, poltergeists, mediumship, and hauntings. Also included are abduction scenarios, possession, psychic healing, and synchronicity.

The *Journal of Exceptional Experiences and Psychology* supports an integrative and pluralistic approach. This includes embracing the varieties of inquiry such as experimentation, ethnography, phenomenology, personal narratives, art, and poetry. Each seeks to reveal, in its own way, the essence of the exceptional experience. In addition, exceptional psychology encourages the application of helpful aspects of certain exceptional experiences to clinical praxis, which is a growing trend and area of interest in the parapsychological literature. This is just as much a part of the project as research and theorizing. For instance, clinical approaches include the use of psychic healing practices and the application of mediumship to the bereavement process.

The *Journal of Exceptional Experiences and Psychology* encourages the submission of research manuscripts, creative writing, artwork, video, and other related creative pieces associated with exceptional experiences. The journal is published biannually while welcoming submissions all year.

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RESOLUTENESS OF AURA LIFE COLOUR ACROSS DIFFERENT GENERATIONS

N. Ramkumar & V. Vaidehi Priyal

The purpose of this study is to determine aura life colour across different generations. The questionnaire was framed to assess seven aura colours and consisted of 112 questions grouped under eight components (Approach to Physical Reality, Mental Attitude, Emotional Makeup, Social Style, Personal Power and Leadership Style, Financial Choices, Career Options, and Spirituality) and 27 questions from characteristics and motivations of different generations as well. Data from 189 respondents were drawn from service (academic and hospital) and manufacturing (textile and auto component) sectors in Coimbatore, Tamil Nadu (India). The questionnaire was subjected to reliability analysis and found to be reliable. Most of the individuals (45%) from four generations were identified as having a blue aura life colour of higher order chakras. Similarly, the majority of individuals working in academic (52%), hospital (50%) and auto sectors (50%) had a blue aura life colour, while in textiles (44%), individuals had a red aura life colour. The results of this study will be able to help researchers formulate further research to develop strategies and measures to bridge the conflicts that arise from the generation gap.

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ANOMALOUS AND REPLICATED HIGH AMPLITUDE PHOTON BURSTS
ASSOCIATED WITH SPECIFIC HYPOTHEZED SPIRITS

Gary E. Schwartz

Previous research in the Laboratory for Advances in Consciousness and Health at the University of Arizona has observed replicated effects of hypothesized spirit presence on measures of photon activity using a silicon photomulti-
The Journal of Parapsychology

Pplier (2010) system and Princeton low light cooled CCD camera system (2011). In addition to obtaining significant main effects of hypothesized spirit presence versus matched baseline control trials, we have observed replicated individual difference effects between hypothesized spirits (for ease of communication, from herein hypothesized spirits will simply be called spirits). In the 2010 study, using a silicon photomultiplier, two spirits (called S2 and S5) produced reliably larger magnitude effects on photon bursts compared to matched baseline control trials than two other spirits (called S1 and S3). In a follow-up series of experiments using the silicon photomultiplier focusing on S5, replicable large magnitude effects on photon bursts were again observed.

In the 2011 study, using a low light CCD camera, two spirits were retested using a completely computer-automated system (i.e., no experimenters were present when the data were collected). In two separate experiments, Spirit 2 produced significantly and substantially larger magnitude photon effects than Spirit 1. In the present experiment, eight different spirits were given the opportunity to be individually tested using a Hamamatsu photomultiplier tube with a Fluke frequency counter and Timeview 2.1 software running on a PC. The photomultiplier tube was housed in a completely dark Faraday-shielded subject room; the frequency counter and PC were in a separate room. The experiment was conducted at the California Institute of Human Sciences; the data were collected by Dr. Gaetan Chevalier and witnessed by Schwartz. Included in the sample were four spirits, labeled S2, S5, S1 and S3; baseline control trials were collected as well. S2 and S5 produced larger magnitude photon bursts than S1 and S3 (p < .002). Also S2 showed a particularly high anomalous value (larger than any that Chevalier had seen in more than 2 years using the equipment).

Based on certain characteristics observed in S2, we predicted that a spirit labeled S4 might produce similar high values; this prediction was confirmed. In a final set of observations involving the measurement of high frequency photon activity at the University of Arizona using a Vernier radiation detector, S2 and S4 again produced particularly high anomalous values. The importance of carefully considering individual differences in hypothesized spirit presence research is discussed.

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SIX PROTOCOLS, NEUROSCIENCE, AND NEAR DEATH: AN EMERGING PARADIGM INCORPORATING NONLOCAL CONSCIOUSNESS

Stephan A. Schwartz

[Excerpted from text] It has been more than six decades since Gilbert Ryle, Waynflete Professor of Metaphysical Philosophy at Oxford, coined “the ghost in the machine,” in his book *The Concept of the Mind*, as a way of criticizing what he saw as Descartes’ absurd mind-body dualism. Since then the nature of consciousness has been largely explored only from the assumption that it was an as yet not understood neurophysiological process entirely resident in the organism. Its inherent physicality became an ironbound axiom. However, a growing body of experimental research now challenges this and a fundamental transition is underway in science. Still a minority position, it is nonetheless the trend direction in a wide range of disciplines, from medicine to biology to physics. Whole new subdisciplines have emerged driven by the results of this research since Ryle’s dismissive words.

This work is pushing toward a new paradigm, one that is neither dualist nor monist, but rather one that postulates consciousness as the fundamental basis of reality. Max Planck, the father of quantum mechanics, framed it very clearly in an interview with the respected British newspaper *The Observer* in its January 25, 1931 edition. Context is always important, and Planck understood very well that he was taking a public position, speaking as one of the leading physicists of his generation, through one of Britain’s most important papers. He did not mince words: “I regard consciousness as fundamental. I regard matter as derivative from consciousness. We cannot get behind consciousness. Everything that we talk about, everything that we regard as existing, postulates consciousness.”

Two corollaries flow from Planck’s assertion: First, is the existence of *nonlocal consciousness*. An aspect of consciousness independent of space time and not resident in an organism’s physiology. Second, that all con-
sciousnesses are interdependent, and interconnected.

One sign of the power of this trend is that most scientists doing research concerning consciousness tend to cite in their papers only work within their own discipline, or a closely related one. Physicists rarely cite physicians, and physicians rarely cite physicists. As a result separate literatures dealing with consciousness, both local and nonlocal, are developing independent of one another. It is only when seen collectively, however, that the emerging paradigm this research is producing becomes clear.

THE CREATIVE PATTERN, NONLOCAL CONSCIOUSNESS,
AND SOCIAL CHANGE

Stephan A. Schwartz

[Excerpted from text] For almost 200 years, the nature of consciousness has been largely explored from the assumption that it was an as yet not understood neurophysiological process entirely resident in the organism. Its inherent physicality became a canon. From this materialist perspective moments of genius are genetics and conditioning, spiritual epiphany is delusional, and psychic functioning—or nonlocal consciousness as it should more properly be called—impossible. And yet these three mysterious human experiences have played, and will continue to play, a major role in social change. The experiences of individuals and our reactions to them create the social trends that transform our world. An individual has an insight, it impresses others, and, in response their small quotidian acts, individual choices, made in the same time frame, create social trends. Think how quickly “gay” was supplanted by LGBT, representing a change in the social gestalt. Consider the change in attitude towards marriage equality, or shift in consensus concerning marijuana prohibition in the United States.

The process is most easily seen in religion, where it can be particularly powerful, even though it may be intellectually irrational, which emphasizes the emotional quality of these insights at the social level. An individual has a nonlocal consciousness experience which they share. If it finds social acceptance, the interpretation of that nonlocal experience(s) becomes the dogma of a new religion. Joseph Smith and the rise of Mormonism in the 19th century, or L. Ron Hubbard and the growth of Scientology in the 20th are two clear examples of this process.

It may be less charismatic but it is no less powerful in science. Consider the German chemist Paul Ehrlich. His name has been forgotten by the public, even as history and the lives of millions have been profoundly affected by his creativity. He, and the teams he led, were responsible for a long list of pharmaceuticals, including the first synthesis of a quinine substitute, a cure for sleeping sickness, and the most effective preantibiotic cure for syphilis. Although he died in 1917, so great was the creative momentum produced by this man that, as historian Henry Hobhouse notes, “In explosives, fertilizers, pharmaceuticals and synthetic substitutes of all kinds the German chemical industry was able to survive defeat in World War I, poor government and inflation in the 1920s, even the slump [i.e., the “Depression”], largely because of the technological lead derived from Ehrlich and his pupils.”

History tells us that creativity is a broad river flowing through any culture. From our collective mass, with an egalitarian democracy that confounds elites, and breaks through privilege, mothers and fathers seen as the most ordinary folk, bring in souls whose lives blaze like comets through our history. These individuals speak to us from some deep place in our collective psyche and these singular people compel us to transform our world. Illumined moments, whether religious, psychic, creative, or scientific, come to individuals, but their power arises from their social acceptance.

The challenge for science is not to dismiss what the individuals say is happening to them as a delusion or fantasy, but to seek to understand the processes by which they occur, and the domain into which they lead us. It is important I think to learn what we can about invoking this state of consciousness, and nurturing it in our culture. We’re going to need a 21st century equivalent of the two bicycle mechanics from Ohio who taught humanity to fly, two young men in a garage creating the personal computer, a lone woman geneticist living above a garage who showed us how part of evolution worked and, three decades later everybody understood what she had seen, and Barbara McClintock was awarded the Nobel Prize.
DECOMPOSITION OF FIELD RNG OUTPUTS DURING MASSIVE TWEETS DURING THE FILM “LAPUTA: CASTLE IN THE SKY” IN JAPAN

T. Shimizu & M. Ishikawa

Analysis using Haar wavelets, proposed by Shimizu, Kokubo & Ishikawa, would be expected to be useful in field RNG studies because all the variance of the RNG outputs can be decomposed by period (frequency) expectancies. This solves issues that field RNG studies have had with arbitrary bit-generation speeds, typically of 1 sample per second. However, there remain several unsolved practical issues. The current study focused on two of those issues as follows.

First, the previous analyses were restricted to event lengths equal to the power of 2 (e.g., 256 s, 1024 s). This had little practical use. The current study solves this point methodologically using zero padding. In wavelet analysis, sequences are repeatedly converted into two arrays: scale coefficients and detail coefficients. Here, (a) we create an array with length of power of 2. (b) The elements in the array out of the event range were filled by zeroes. (c) Additionally, degrees of freedom were counted independently, also being filled by zeroes during the nonevent time range. This zero-padding can solve the above problems, and it is also useful in dealing with processing missing data.

Second, available information from the highest level of wavelet analysis is limited by the RNG’s generation speed. In previous experiments, the RNGs generated 64 bits per 125 ms (1/8 s) or 512 bits per second. Under this condition, one second with eight trials can provide only two levels of wavelet decomposition (4 = 2^2). To moderate such inefficiency, we propose an alternative way to decompose variance of RNG outputs more deeply, by recording binary data of RNG outputs into the database without splitting trials. Since binary data in the database consist of arrays of bytes (eight bits), this could record all the information from the RNG outputs. Under the condition of 512 bits per second generation, wavelets can provide five levels (depth) of analysis for 1 second (32 = 2^5).

After the improvement of the methodology, we analyzed the data recorded during broadcasting of the film Laputa: Castle in the Sky in Japan, during which a huge TV audience tweeted simultaneously in the final stage of the film. Two RNGs generated random numbers, both of which were connected to two true RNGs, Psyleron and Rpg102, respectively. The two machines were about 7 m distance from each other. Then, the outputs of four RNG devices were analyzed, respectively. All the RNGs generated 512 per second, recording as binary data into databases. Creating cumulative chi-square statistics for each decomposed level, results revealed no biases for the four devices, confirming the validity of the methodology. Finally, we discuss several tasks for future research.

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DO SYNESTHETES PERFORM BETTER AT A PRECOGNITION TASK THAN A GROUP OF MATCHED CONTROLS?

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Previous studies suggest that synesthetes might do well at ESP. In this study, a group of strong synesthetes and a group of matched controls were compared on their performance on a psi task (Bem’s precognitive memory), scores on the Time Style™ questionnaire, and performance on a task designed to tap mental time travel (MTT; autonoetic consciousness). Correlations between scoring on the Synesthesia Experiences Questionnaire (SEQ), orientations to time (as tapped by the Time Styles™ inventory), mental time travel ability (MTT), and performance on a psi task were explored. Twenty-seven synesthetes were matched to 27 controls by age and gender and scoring on the SEQ. Participants completed the Time Styles™ inventory and the synesthesia subscale of the Tellegen Absorption questionnaire (TAS), then took part in Bem’s precognitive memory task, an MTT task, and an interview about their experiences. Synesthetes completed two versions of the Synesthetic Characteristics Questionnaire (SCQ). Scoring on the SEQ and TAS subscale were significantly different between synesthetes and matched controls, lending
construct validity to the SEQ. Scoring on the SEQ correlated positively and significantly with scoring on the TAS, lending concurrent validity to the SEQ.

There was no evidence for ESP in the group as a whole, or among the controls or synesthetes; the overall effect size was $d = .05$, the effect size for the controls was $d = -.014$ and that for the synesthetes was $d = -.00$. The difference in scoring between the synesthetes and controls was not significant. The correlation between scoring on the SEQ and psi was not significant. A post-hoc test found that synesthetic associators did better at the psi task than those who were projectors ($z = -2.00$, $p = .047$). There was no difference between time-related and non-time related synesthetes on their psi scores. Synesthetes and controls did not differ in their overall recall ability, and recall had no relationship with psi scoring. Scoring on future time style correlated positively and significantly with scoring on the SEQ, $\rho = .35$, $p = .011$. There were no differences between the synesthete and control groups in terms of scoring on any of the time styles. Past thinking correlated negatively and significantly with psi scoring, but this was not confirmed with a nonparametric correlation. There were significant correlations between a number of variables tapping MTT and scoring on the SEQ.

When synesthetes were compared with matched controls, this was only significantly different for smell related imagery, although the difference approached significance for the extent to which one felt as though they were re- or pre-experiencing life events. None of the variables relating to MTT correlated significantly with scoring on the psi task. Findings are discussed and suggestions for future studies are made.

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THE “WEAK” VS. “STRONG” INTERPRETATIONS
OF RHINE’S ESP RESEARCH

Jerry Solfvin

Methodological debates and discussions are an important part of any field of science. In psychology, from which parapsychology has traditionally drawn much sustenance, debates and discussions about significance testing and interpretation of research results provide fertile fields to remind us that data need be carefully collected but also thoughtfully converted into meaning, and that meaning communicated effectively to others. The small and often controversial field of parapsychology has done a rather good job with the technical details of methodology and statistical soundness but may experience the most problems with the end-process, the articulation and communication of the meaning of its results more broadly.

This paper explores how the historical development of methodology by J. B. Rhine and others may be affecting today’s progress. Science is one method for seeking answers to questions. But science is not primarily about knowing. Not-knowing and maintaining one’s sense of mystery may be the key to great breakthroughs in science. The paradox is that finding an answer—or thinking you’ve found an answer—may turn the open-minded seeker into a not-so-open-minded marketer. As Andre Gide put it, “…Believe those who are seeking the truth. Doubt those who find it.” The early development of Rhinean parapsychology may have followed such a path and handed down a sort of methodological “poison pill” to our field.

There are three issues I’d like to discuss with regard to this: (a) defining ESP negatively, that is, as something that occurs “without” or “in the absence of” a normal (physical, sense-based) explanation; (b) the lack of control groups in testing ESP; and, (c) the conceptual leap Rhine took from the “weak” interpretation of his ESP results to the “strong” interpretation. The first issue makes it difficult to provide good examples of what “it” is that we study and left parapsychology open to all the tricksters who produce faux ESP—consciously and unconsciously—for their own reasons.

The second issue, lack of control groups, meant using statistical controls, thus putting much too much weight upon statistics, and further encouraging researchers to equate ESP with any deviation from chance. Early researchers began discovering a wide variety of different ways that their results were statistically significant, and this was welcomed as new evidence of ESP.
But the real poison pill was planted by the third issue, when Rhine favored the “strong” interpretation of his results over the “weak” (more conservative) interpretation. The weak interpretation is itself a rather powerful and important one—it is that Rhine had falsified the materialism assumption that Western science clings to. That wasn’t enough for Rhine, so he went the next step, to the strong interpretation—that he had found the specific actions of the mind that caused the statistical deviations from chance. He even named it—ESP. Further, he said that he and his researchers knew how it worked. It was a universal ability, could be tested by a simple (though laborious) Zener card test. Women had more of it than men, believers had more than skeptics, children more than adults. It was independent of time and space and came in several varieties. It was mind-stuff, nonmaterial, and can be studied by studying individuals, especially their brains, and by finding correlations with other traits and abilities of individuals. I suggest that this view, or something like it, still dominates our field to this day. While the weak interpretation is clearly supported by Rhine’s work and by many studies since then, it is not at all clear how much of our “knowledge” about psi—the strong interpretation—is supported by data.

The concern today is that this tradition starting with Rhine is a tradition of “knowing,” and may cause us to lose our sense of mystery. This may actually blind us to other, potentially more productive avenues of research. For myself, I believe that this discussion suggests that there may be good reasons to take a step backwards before attempting to move parapsychology forward. This may be the time to return to our roots, to re-examine the foundations, the basic concepts, the definitions, the methods, and results. This may also be the time to look for similar methodological and interpretational issues in our sister sciences. (Note: I know of two such areas—experimenter expectancy effects, and placebo studies—which I will briefly discuss if there’s sufficient time). Finally, and most importantly, I want to leave you with an appreciation for the power of not-knowing, to start a return to the sense of mystery, to encourage the individual and collective cultivation of pluralism in science and in our lives, and to embrace Andre Gide’s comments about seekers.

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THE CURRENT STATE OF PSI STUDIES AND THE PUBLIC CONCEPT OF PARAPSYCHOLOGY IN THE UKRAINE

Sergii Vakal1, 2 & Igor Bombushkar1

Background. For more than a half of a century Ukraine was an integral part of the USSR—a country with a big tradition of parapsychological research and even governmental grant programs for psi studies. Moreover, there is a relatively high level of paranormal belief among the Ukrainian population (according to Haraldsson’s 1999–2002 years survey). The extremely high popularity of the TV show Psychic Challenge with 13 seasons (a higher number can be found only in Russia) and the existence of such “paranormal” professions as fortune-teller and astrologer in the National Occupational Classification of Ukraine (edition of year 2010) are two striking examples of the high belief in the authenticity of psi phenomena in Ukraine. At the same time, there are no publicly available data on current research projects, educational programs, and professional establishments in the field of parapsychology in the Ukraine. Moreover, no data on the concept of parapsychology among the modern Ukrainian population is available at the moment.

Purpose. (a) To perform a comprehensive assessment of the current state of parapsychological research and education options in the Ukraine; (b) to analyze the dominant public concepts of parapsychology in modern Ukraine.

Methods. A list of establishments and societies claiming their relation to parapsychology was formed through iterative search with Google and Yandex search engines. Available educational programs, financial support and periodicals were searched both with search engines and manually on the sites of establishments found on Step 1 and official higher institutions. Publications of Ukrainian authors on parapsychological subjects in peer-reviewed journals were searched in the Lexscien and Scopus databases. Analysis of all found data was performed manually. The public concept of parapsychology was formed on the basis of two mini-studies: (a) street interviews in Kiev, Lviv, and Odessa (more than 1,500 responders in total); (b) advanced searches in Google by keywords and word
combinations in the Ukrainian, Russian, and English languages with/without territorial consideration. The integration of results and drawing of conclusions were carried out manually.

Results. Totally 22 establishments claiming a relation to parapsychology were found; two of them were entitled as “institutes,” others as “centers” or “schools”; 96% of them (22 from 23) were found to be related with nonparapsychological bizarre activity, such as fortune-telling, white or black magic, and so forth. Psi research was conducted at only two of the above-mentioned establishments, and only one of these two wasn’t related with occult or profane activity—the Ukrainian Center for Parapsychological Research. A current direction of the Center’s research activity is the development of new methods for the evaluation of reincarnation hypotheses and the development of new software for computer-aided evaluations of ESP abilities. No periodicals on nonconventional psi studies of Ukrainian origin were found. Only one funding program (mini-grant) from a private person was revealed.

25 courses on parapsychology of different length and form (distant/on-site) were found; 24 of them (96%) were related with esotericism and occultism, so only one was really related to parapsychology (introductory level distant course). No courses based in universities were found. No publications of Ukrainian origin on any aspect of parapsychology were revealed in peer-reviewed journals. Only one adequate introductory textbook on parapsychology is currently available in Ukraine (written in Russian) and there is only one (supporting) member from Ukraine in the Parapsychological Association.

A street survey revealed that 90% of responders (average from three cities) define parapsychology as something related with mysticism, occultism, psychic practices or mediation (people were asked to give a definition of the term “parapsychology”). Only 7% gave an adequate definition to the term (and 2% gave no definition). An analysis of the Ukrainian part of the internet revealed only 1,190 hits for the query “allintitle: parapsychology” in Ukrainian (in comparison: 27.9 K—in Russian, and 40.2 K—in English). Thus, there are very low amounts of information on parapsychology in the Ukrainian language. At the same time, according to Google scoring and manual semantic analysis, 28% of the 100 best hits correspond to pages on scientific parapsychology (in Ukrainian), 19% in Russian, and 74% in English. Irrelevant hits contained links to sites providing occult-mystic services for money, esoteric forums, and so forth. So, it’s not a simple task for an average person to find adequate information on parapsychology through the internet in a heap of bizarre sites. After the subtraction of occultism-related keywords, the most dramatic decrease in number of hits was established for the Russian language (43%). In our opinion, improving search engines results (the creation of more adequate hits at the top) is an actual task for Ukrainian parapsychologists in the near future.

Conclusions. (a) Thus, currently there is only one organization performing research and introductory education in the field of parapsychology in Ukraine. (b) Overwhelming parts of the Ukrainian population treat parapsychology as synonymous with such bizarre topics as occultism, esotericism, magic, and so forth. (c) The terms “parapsychology” and “parapsychologist” are often used by charlatans, thus worsening its reputation on Ukrainian territory.

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PANEL: REMOTE VIEWING’S GREATEST HITS, AND LESSONS LEARNED

Chair: Russell Targ

Panelist: Stephan Schwartz

A REMARKABLE DECADE: REMOTE VIEWING AT SRI

Russell Targ

In the decade 1972 to 1982, I was involved in a number of experimental series at Stanford Research Institute, in which Hal Puthoff and I were investigating a new parapsychological protocol called remote viewing. This free-response clairvoyant approach using a viewer and interview pair was proposed to us by the great American psychic
artist Ingo Swann. The interviewer, who was blind to the target pool, would work with the viewer, who was to psychically describe the location where a traveling beacon person would be located. I was the interviewer for most of the trials comprising the following unusually successful experimental series:

1974: Pat Price, policeman, thought to have psychic abilities, 9 outdoor targets. Seven of nine targets were matched first place. Prob. = 3 in 100,000. Effect size = 1.3. In the same year, Price located the car that kidnapped Patricia Hearst, and read classified code words inside a secret NSA facility in Virginia.

1975: Hella Hammid, photographer, formal control subject, with no previous ESP experience. Nine outdoor target locations. Prob. = 2 in a million. Effect size = 1.5.

1995: Hella Hammid, Formal IEEE precognition experiment: Four outdoor targets chosen from a pool of 60. Each trial was matched first place against the pool of four. Prob. = .04 (1/4!), Effect size = 0.87. The three above, were published in Proc. IEEE, 1976.

1978: Six Army intelligence officers, including Joe McMoneagle and Mel Riley; 36 outdoor targets; 19 first place matches, six expected. Prob. = 3 in 100,000. Effect size = 0.67. Four of the viewers were independently significant. SRI Final Report: Special Orientation Techniques, June 1980. Russell Targ, et al.


My conclusion is that the set and setting for the remote viewing session is very important. The interview should follow the plan described by Carl Rogers for nondirective interaction with the viewer. He taught unconditional positive regard, featuring compassion and interest in the viewer as a person. Since the interviewer is blind as to the target, he can be active and helpful to the viewer in many ways. He can always expect a miracle, and actively lead the viewer to look for surprising images, and away from naming, guessing, and analysis.

MIND ROVER: THE MOBIUS LABORATORY—1977 TO 1993

Stephan Schwartz

These are the research areas and achievements of the Mobius laboratory from 1977–1993 that will be discussed in this presentation:

1. Establishing that nonlocal consciousness is not electromagnetic, concluding a line of research begun by Soviet academician Leonid Vasiliev.
2. Creating the associational remote viewing (ARV) protocol in which a target is associated with a particular outcome, or for communicating a message. Often done precognitively, frequently before the target exists as a target.
3. The development of a protocol for applied parapsychology that used nonlocal consciousness as the primary information source to guide a series of successful archeological projects including both location and complex reconstructions of:
   • Santa Catalina Island off the coast of California (1977), location of a previously unknown coaster cargo vessel.
   • Alexandria, Egypt, Project Leader and Research Director (1979-80), resulted in the first modern mapping of the Eastern Harbor of Alexandria, and the location of numerous shipwrecks covering many centuries, as well as the Emporium and the Timonium, Mark Antony’s palace in Alexandria, the Ptolemaic Palace Complex of Cleopatra, the remains of the Lighthouse of Pharos, one of the seven wonders of the Ancient world, and a major civic building in the nearby buried city of Marea;
   • The location of the cargo vessel Dean Richmond in Lake Erie.
   • The location and reconstruction of the wreck site of the Brig Leander as well as 18 other wreck sites, and compilation for the Bahamian government is the first survey of all significant wrecks on the Grand Bahamas Banks;
   • The location of remnants of a caravel from the fourth voyage of Christopher Columbus, and reconstruction of events leading up to the loss the caravel.
4. Two international mass tests which were correlated with a wide spectrum of personality measures, and which made it clear that while introverts and extroverts may open to nonlocal consciousness using different psychophysical strategies both could do so.

5. Demonstrating a reliable objective measure of therapeutic intention in changes in the structure of water, as measured by infrared spectrophotometry.

PANEL: FROM MATERIALISM TO PLURALISM:
RESTORING A SENSE OF MYSTERY TO SCIENCE

Chair: Jerry Solfvin
Panelists: Dianne Hennacy Powell, Robert S. Gebelein, David Scharf, Beverly Rubic, John Klimo

There is good reason to be concerned about scientists who so easily disregard potentially valuable data and concepts that come out of other fields. We here, of course, are especially concerned when others ignore parapsychological evidence. Scientists are human beings, subject to the same foibles as other human beings, and it takes real effort to overcome tendencies towards bias, prejudice, and inattentional blindness. It is precisely because we are human beings that we get so caught up in our own egocentric worlds, theories, philosophies, beliefs, and current fixations, and that we ignore anything contradictory, unsettling, or even just peripheral to our current focus. This manifests in many ways. The materialism or physicalism assumption of western science may blind scientists to viewpoints which are peripheral to that assumption. We must be grateful for the field of philosophy of science, whose primary goal, according to Cambridge philosopher Hosak Chang, is to inform scientists about the perils of monism and to guide us towards scientific pluralism. When one assumption, theory, or philosophic position dominates a science to the exclusion of alternatives, scientific progress suffers. On the other hand, the pluralism which welcomes alternative theories and methods—especially strange or unusual ones—can facilitate science. In individuals, a symptom of monism is scoring low on the yet-to-be-studied trait or state I call sense of mystery. Among scientists, knowing is overvalued, and not-knowing and expressing a sense of mystery are undervalued. In fact, many of us were taught that the opposite of knowing is being stupid. The opposite of knowing could be described as preserving a sense of mystery. That is, there is power in not-knowing, in holding onto one’s sense of mystery. Scoring high on the sense of mystery trait is the basic stuff of pluralism.

So, where do we parapsychologists stand on the issue of pluralism? While we correctly scold other scientists for ignoring our data, do we welcome pluralism within our own field? What happens when someone tells you a personal, unexplained, potentially parapsychological experience? Do you think or say, “that’s telepathy,” or “presentiment,” or “remote sensing”? If so, you may be losing your sense of mystery. You may be becoming one of the very people you’ve been complaining about, the ones who “…disregard potentially valuable data and concepts…” in favor of your own favored explanation. Pluralism means openness to another viewpoint, maintaining the sense of mystery about someone else’s experience or beliefs. It’s a rare joy to stumble upon a person, scientist or not, who maintains a sense of mystery about the world, who remains open to new data, and it’s a special joy in a scientist. This panel is a discussion of loss of sense of mystery in science, but more importantly, inside of parapsychology where it may impede us. Pluralistic thinking may revivify science, beginning with us!

THE “CLOSEMINDING” OF THE AMERICAN MIND

Diane Hennacy Powell

Arthur Koestler published The Roots of Coincidence: An Excursion into Parapsychology in 1972, a very optimistic time for parapsychology. He listed dozens of eminent scientists and clinicians from the 20th century who became convinced of the merits of studying psi. Among them were Sigmund Freud and Professor Eysenck, the Psychology Chair at the University of London and Director of Psychology at the Maudsley and Bethlehem Royal Hospitals,
where I studied child psychiatry. Eysenck boldly wrote:

> “Unless there is a gigantic conspiracy involving some 30 University departments all over the world and several hundred highly respected scientists in various fields, many of them originally hostile to the claims of the psychical researchers, the only conclusion the unbiased observer can come to must be that there does exist a small number of people who obtain knowledge existing either in other people’s minds, or in the outer world, by means as yet unknown to science.”

Psychic phenomena are still labeled “supernatural,” even though they share many parallels with what modern physics tells us. In fact, the field of physics is actually much stranger than parapsychology. And physicists now have proof for many of their previously unfathomable concepts, such as entanglement and, most recently, gravitational waves and the Higgs boson. Meanwhile parapsychological research was declared a pseudoscience and thrown under the proverbial bus. Programs at Duke, Princeton, Stanford and elsewhere were dismantled despite intriguing data. What happened?

And on the clinical side, psychiatry became increasingly biological. Psychiatrists increasingly prescribe medications, while other mental health professionals do the psychotherapy. Now most psychiatrists rarely get to know their patients to the depth possible in Freud’s time, or even mine. This means they miss out on stories that could challenge their view of human consciousness, like happened when a patient at Cambridge Hospital rocked the theoretical boat for me. Within minutes of first meeting, she told me detailed information … about me … from my past, present, and on into the future. According to my training, this was not possible.

My intellectual curiosity was piqued, leading to my book, *The ESP Enigma: A Scientific Case for Understanding Psychic Phenomena* in 2008. But less than two years after its publication, just having “ESP” in the title, along with “scientific understanding,” was enough for the state medical board to question my sanity.

As Professor Eysenck had astutely noted, “Scientists, especially when they leave the particular field in which they have specialized, are just as ordinary, pig-headed and unreasonable as anybody else, and their unusually high intelligence only makes their prejudices all the more dangerous.” Dangerous? Yes, indeed. But there is one thing we know for certain about pendulums, including that of scientific opinion. They swing.

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1 In *The Closing of the American Mind* (1987), Allan Bloom proposed that “higher education has failed democracy and impoverished the souls of today’s students.” According to Bloom, deconstructionism, and the “openness” of relativism, had paradoxically led to the end of critical and rational thinking … and the closing of the American mind.

### THE POLITICS OF PHYSICALISM

Robert S. Gebelein

Physicalism, the assertion that there is no reality beyond the physical or what can be explained by known physical laws, dominates scientific thinking, in the face of all evidence to the contrary, even though this view was not determined by scientific methods. In order to verify, logically, a proposition of the form “There are no such things,” one must have a complete knowledge of everything. And conversely, in order to refute it, only one instance of the given phenomenon is necessary. In 1958 I dreamed of my grandmother’s death an hour before I received the telegram. That was all the evidence I ever needed to know that there was a reality beyond what I had learned in my physics courses at Harvard.

Physicalism is supported by illegitimate arguments like ridicule, authoritarian pronouncements, or simply ignoring the data. These arguments are widely accepted by people who should know better. Ultimately, physicalism is enforced by denying publication, funding, and employment to those who would go beyond its boundaries. These are not the arguments of science, but of power politics.

The problem of physicalism has no simple solution. It is woven into the tapestry of what a human being is. If you belong to a social group, you must conform to its opinions, attitudes, and beliefs, which are determined by the most dominant members of the group. This is the pecking order, which human beings, even scientists, have in common with animals. Ideally, those scientists who practice the scientific method most faithfully and accurately
would be most dominant, but as science has attained success, it has gained status and therefore power in the larger community, and more general rules of dominance (like character assassination) apply. Scientists are believed simply because they are scientists, whether their arguments are legitimate or not.

We can’t just say, “I don’t know.” We have to have some explanation of the unknown. People seek out “authorities” to explain what is beyond their own sphere of knowledge. They believe there are such “authorities” because as small children their parents were as gods to them, all-knowing and all-powerful, and so psychologically they are primed for this kind of belief. Because people seek “authorities,” other people become “authorities” to accommodate them.

And so we have religion, which is primarily fiction, to explain and deal with the unknown. Religion is a rigid authoritarian belief system, claiming to be the absolute and ultimate truth. The existence of the spiritual need not be associated with any religion, as younger generations starting with the hippies have come to recognize.

Science is more plausible than the old religious beliefs, so scientists have taken over the authoritarian role of the priesthood in explaining the unknown. In order to do this, they simply assert that nothing exists beyond the physical, and then of course science has explored the physical thoroughly, from ultradistant galaxies to ultrasmall subatomic entities.

By making discoveries that are not explained by their physical laws, we are upsetting their role as priesthood and disturbing the security of their followers, who are the ones, ultimately, who provide the money for scientific research.

TWO DOGMAS OF MATERIALISM: TOWARD A QUANTUM FIELD THEORY OF THE PSYCHIC FIELD

David Scharf

Materialism, as a philosophy of mind, is the thesis that consciousness and mind are entirely dependent on the brain, they could not exist without the brain, and, in its strongest form, that consciousness and mind are held to be reducible to the brain and its electro-chemical processes. But even its most steadfast proponents acknowledge that materialism cannot account for the fact of consciousness and that it involves extremely unpalatable consequences for our sense of autonomy and moral responsibility. Moreover close analysis shows that the cognitive dissonance inherent in materialism makes it virtually untenable as a coherent theory of mind. How then should we account for its resilience as the default mainstream theory in contemporary neuroscience, cognitive psychology, and the philosophy of mind?

This resilience stems from the seeming plausibility of two central dogmas. First, that impairment to critical brain regions causes a degradation of corresponding cognitive function and that this proves that mind is dependent on the brain. And second, the physical domain is presumed to be causally closed, which is taken to imply that mind is either reducible to the brain or else epiphenomenal and irrelevant. I propose to show that neither of these dogmas stands up to critical scrutiny. And that the result of this scrutiny can help delineate the outlines of a transmission model of the mind/brain interaction, in terms of quantum field theory.

TOWARD AN INTEGRAL SCIENCE OF LIFE

Beverly Rubik

The dominant paradigm in biology and medicine with its reductionist methodology and epistemology led to a materialistic view of the organism comprised of biomolecules as well as life without context or meaning. This worldview has led to a plethora of biotechnology products and a level of control and manipulation of nature, with genes and genetically modified life forms as patentable commodities. Yet we cannot marvel that a human has even less genes than a grain of rice! Clearly this paradigm is sorely lacking. The most extraordinary aspects of life including consciousness, creativity, intent and will, the full potential for health, healing, and spiritual growth; evolution; and the extraordinary interconnectedness and interdependence of all life in the biosphere which includes the wonders of
psi phenomena—all are excluded from mainstream science. However, contemporary discoveries emerging from frontier areas of inquiry such as the mind-body interrelationship, the biofield, and evidence from integrative medicine, point to the need for a new science that would encompass more of the heart and soul of life. This presentation will point the way toward a life science that is integrated with spirit: one that inspires awe, wonder, respect, and care for life on earth.

A PROPOSED SPECTRUM FOR PARAPSYCHOLOGY:
FROM MATERIALISM TO IDEALISM

Jon Klimo

In the early nineties, I began presenting what I call a Hegel metric, which, over the ensuing years, I have come to see as a kind of spectrum that works particularly well for me in depicting the relationship between a physical reductionist material monism on one end and a monist idealism, with all being ultimately consciousness, on the other end. In between, fall a variety of dualistic perspectives, including the phenomena and experiences studied by parapsychology.

I call this a Hegel metric because it is informed for me by the German idealist George Hegel’s philosophical system where all that exists is ultimately what he calls absolute spirit. Nonetheless, we individual human minds and spirits as aspects of this one underlying Spirit mostly experience the reality we are embedded within as being much more materialistic or dualistic in nature than as possessing the monistic nature of an idealism that is ultimately spirit aware of itself as spirit in a self-same universe of Spirit.

Although I have elsewhere written in more detail about this Hegel metric, consider for now this very condensed version: On one end of the metric is what I’m calling Hegel three. This is where the individual human experiencer is essentially Hegel’s absolute spirit, or what I see as the all-creating, all-comprising, all-sustaining universal consciousness field manifest in us in human form and being, but where we are currently in Hegel’s antithesis state where we are out of phase with and unaware of ourselves as being, and being embedded in, an underlying field of consciousness or spirit. Using Charles Tart’s notion of state-specific or state-dependent consciousness, we are only able to experience a kind and level of reality as a function of the delimiting state of consciousness we are in at the time. Being at Hegel three, and in what Hegel calls the antithesis state, is what I have elsewhere called a level of cosmological dissociation where we are, to borrow from the psychological concept of dissociation, very dissociated from the true nature of our own beings and potential and dissociated from the true nature of the one underlying Reality. For me, to overcome the presenting problem of our current condition of cosmological dissociation is to become aware that we are localizations and individuations of absolute spirit or universal consciousness embedded within and interacting with fellow consciousness/Spirit. But so long as we are experientially constrained state-specifically to our present cosmologically dissociated condition, we can only know through the lens of a Hegel three perspective where all is materialistically experienced as different than, and distanced and separate from, our consciousness experiencing it. Hegel three is the home of materialism and physical reductionism.

As we as individuals and as a planetary species continue to overcome our kinds and degrees of antithesis and dissociation, we move on the Hegel metric to the domain of what I call Hegel two. If we go all the way over to Hegel one, we reach what Hegel called the thesis state, where all individual perspectives are now able to experience themselves and all else surrounding them as being absolute spirit, universal consciousness, aware of itself as such. But for the foreseeable future, we remain far from this. Rather, as a species we remain constrained to the experiential perspective of Hegel three, but now with the beginnings for more and more of us with a growing ability to be able to experience state specifically from the perspective of Hegel two. Hegel two contains an increasing interpenetration, confluence, and superimposition of earlier Hegel three’s self-estranging materialism and separatism and eventual Hegel one’s experience that all is consciousness and spirit aware of itself as such.

Hegel two will be confusing and confounding for many. What was once construed as being associated with the inner, subjective, and imaginable realm will take on more of the objective nature of what we have been long used to associating with an objective reality external to and separate from the consciousness experiencing it, and what was once thought external to and separate from us experiencing it will become ever less objective and
invariant. With Hegel two, inner becomes more outer-like and outer becomes more inner-like until eventually, more and more is realized as being the objects and contents, the localizations and individuations, of the one universal consciousness field, the one being of absolute spirit. But until Hegel one is reached, we will have magical-seeming goings-on; we will have ever more of nonlocal mind and its objects and contents hard to place and predict; we will have magical thinking and magical causation, all manner of remote viewing, manifestation and materialization, and eventual increasing evidence of our omniscience and causal capacity as our cosmological dissociation continues to be overcome. Needless to say, parapsychology will come fully into its prime as we continue to move from current Hegel three into Hegel two.

INVITED ADDRESS: A MULTIPHASIC MODEL OF PRECOGNITION

Sonali Bhatt Marwaha & Edwin C. May

Precognition is defined as the acquisition and cognition of information emerging from a distant point in spacetime that is space-like separated (i.e., not causally related), which is blocked from the usual sensory systems by distance, shielding, or time. Despite the controversies, there is sufficient empirical evidence for the validity of the phenomenon. In this paper we present the multiphasic model of precognition (MMPC) which is capable of addressing the experimental data. The MMPC identifies two distinct phases: The physics domain addresses the question, How is it possible for information to traverse from one space-time point to another? We suggest that the solution might be found within entropic considerations. The acquisition and interpretation of retrocognitive signals is via three stages in the neuroscience domain: (a) perception of signals from an information carrier, which is based upon psychophysical variability in a putative signal transducer; (b) cortical processing of the signals is mediated by a cortical hyperassociative mechanism; and (c) cognition, which is mediated by normal cognitive processes that lead to a precognitive response. The model is comprehensive, brain-based, and provides a new direction for research, seeking the involvement of requiring multidisciplinary expertise.

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BANQUET ADDRESS: AUTHORS OF THE IMPOSSIBLE: WHAT THE HUMANITIES HAVE TO OFFER PARAPSYCHOLOGY

Jeffrey Kripal

I would like to make a few suggestions about what the humanities might offer parapsychology. I am going to assume that everyone in this room knows what parapsychology is. I am also going to assume that almost no one in this room knows what the humanities are. That is not a dig. It is a near universal truth. Many humanists, I think, would also struggle to define what they do. Toward our conversation this evening, allow me to venture three, increasingly technical, definitions.

1. The humanities are all those fields of study that attempt to understand and analyze the nature and construction of meaning, value, beauty, and narrative in the history of humanity as these have been crystallized in fields like philosophy, language, religion, literature, and art.

2. Put more technically, the humanities are all those forms of modern thought that assert that reality is not just made up of matter, numbers, objects, and causality (which is what the natural sciences assert), but also of experiences, meanings, values, words, subjects, and stories (which is what the humanities assert).

3. Put most technically, the humanities are the study of consciousness coded in culture.
BAYESIAN AND CLASSICAL HYPOTHESIS TESTING: PRACTICAL DIFFERENCES FOR A CONTROVERSIAL AREA OF RESEARCH

By J. E. Kennedy

ABSTRACT: The use of Bayesian analysis and debates involving Bayesian analysis are increasing for controversial areas of research such as parapsychology. This paper conceptually describes the philosophical and modeling differences between Bayesian and classical analyses, and the practical implications of these differences. Widely accepted statistical conventions have not yet been established for Bayesian analysis in scientific research. The recommendations from the FDA guidance on using Bayesian methods are appropriate for confirmatory experiments. This guidance recommends that the study design and protocol include (a) specification of the prior probabilities and models that will be used, (b) specification of the criteria that will be considered acceptable evidence, (c) operating characteristics for the probability of Type I error and power of the analysis, and (d) an estimate of the relative roles of prior probability versus the data from the current experiment in producing the final results. Both classical and Bayesian methods are valid when properly applied with confirmatory methodology that includes prespecification of statistical methods, and prospective evaluations of inferential errors and power. Evaluations of inferential errors and power measure the validity of a planned hypothesis test, including Bayesian analysis. Unfortunately, the use of confirmatory methodology has been rare in psychology and parapsychology.

Keywords: Bayesian analysis, classical analysis, inferential errors, confirmatory research, subjective probability

The use of Bayesian analysis has been rapidly increasing in science and is becoming conspicuous in scientific controversies. For example, Wagenmakers, Wetzels, Borsboom, and van der Maas (2011) argued that classical analyses supporting parapsychological effects are evidence that classical methods are faulty and should be replaced with Bayesian methods. Bem, Utts, and Johnson (2011) responded that certain aspects of this analysis were flawed, but agreed that Bayesian methods have advantages that will be increasingly utilized in scientific research. Debates like this typically focus on specialized technical points without presenting the fundamental assumptions and models that provide the crucial context for understanding and evaluating the arguments.

The present article is intended to describe conceptually the philosophical assumptions, models, and practical aspects that differ between Bayesian and classical hypothesis testing. This discussion should allow a person to conceptually understand the descriptions of methodology and the findings for experimental research that uses Bayesian analyses, and to follow debates about conflicting conclusions from research data. In addition, some potentially controversial claims and practices with Bayesian methods are described, as well as recommendations for methodology for confirmatory experiments. References are not provided for concepts that are commonly described in writings on Bayesian methods.

The discussion here focuses on evaluating the evidence for an ESP or psi experimental effect using a binomial analysis, as is common in parapsychology. Bayesian methods can also be used for other types of analyses. The basic principles discussed here also apply for other analyses.

When discussing current limitations, uncertainties, or debates about a statistical topic, I sometimes offer my opinion about the optimal strategy for handling the matter. Some of these opinions are prefaced with qualifiers such as “in my opinion” or “my perspective is.” These qualifiers are intended to indicate that a detailed technical discussion of the topic is beyond the purposes of the present article, and that others may have differing opinions.
Is Probability a Condition of the Physical World or a Condition of a Human Mind?

Bayesian and classical analyses are based on different philosophical perspectives about the nature of probability. Consider the case of a colleague who goes into a separate room and flips a coin. After the coin has been flipped, the colleague knows the outcome, but a person in the other room does not.

Objective Probability

One perspective is that after the coin has been flipped there is no uncertainty about the outcome. It is what it is. If the coin came up heads, the probability that it is heads is one, and the probability that it is tails is zero. The fact that a person in another room does not know the state of the coin is irrelevant. Probability in this case is objectively based on the state of the physical world. It is not an accurate representation to describe the state of the coin as being uncertain after the state has been physically determined.

Classical hypothesis testing is based on this philosophy of probability. A scientific hypothesis such as “do some people have psychic abilities” is a question about the existing state of the world. The world is what it is, and the fact that a particular person is uncertain about the truth of a hypothesis does not affect the existing state of the world. Variation and probability pertain to the outcomes of future experiments and observations, not to the properties of an existing state of the world. This perspective on probability is also called the frequentist interpretation because it assumes probability is based on the frequency of occurrence of an outcome when the random event or observation is repeated many times.

The logic for statistical analysis is to determine the probability for the outcome of an experiment given that a certain state of the world exists. The statistical models treat the parameters for the state of the world as constant and the outcome of an experiment as variable.

Subjective Probability

An alternative philosophical perspective is that probability is based on the beliefs in a human mind and therefore is subjective. The fact that the state of the coin has been determined does not resolve the uncertainty for a person who does not know the outcome. Uncertainty and probability exist for that person. The probability for a person in the room with the coin is completely different than for a person in another room.

Bayesian statistics are based on subjective probability and prescribe how a person’s beliefs should change as new data are obtained. A mathematical model determines the optimal beliefs given the initial beliefs and the new data. This strategy assumes that the uncertainty in a person’s mind can be quantitatively modeled and that a person’s beliefs should rationally follow the mathematical laws of probability theory.

A person’s initial beliefs and uncertainty are mathematically represented with prior probability distributions. These represent the person’s beliefs prior to collecting data for the current study. Ideally, everything that a person believes about a topic is quantitatively incorporated into the prior probabilities. For example, any concerns about misconduct or biased methodology in previous studies must be incorporated quantitatively into the prior probability values.

After the data have been collected for the current study, the analysis combines or updates the prior probabilities with the evidence from the new data to produce the posterior probability. This mathematically represents what the person should rationally believe given the prior beliefs and the data from the current study.

In Bayesian models, parameters representing the existing state of the world are treated as variable and the observed outcome of an experiment is treated as constant. The variability in the parameters for the existing state of the world represents the uncertainty in a person’s mind, not variation or fluctuations in the actual state of the world. Cases with variations in the state of the world, such as in a random effects analysis, are a different aspect of variability in the model.

Different Uses of Probability

Both philosophical perspectives on probability appear to me to be valid. They focus on different manifestations of probability. Objective probability attempts to directly model uncertainty in the physical world whereas
subjective probability attempts to directly model uncertainty in a human mind. Both approaches involve algorithms for drawing inferences about the world. Both quantify uncertainty using mathematical probability distributions of hypothetical possibilities for the value of terms in mathematical models. Both assume that the mathematical models can be verified and improved by making observations.

The key question is how useful the two approaches are in practice. I tend to favor one or the other, depending on the context for the use of probability.

In situations such as gambling games in casinos, the probabilities are precisely known. All possible outcomes of a series of random events can be fully enumerated. The probability that a certain outcome will occur on a series of trials is clear, and the concept of probability based on many repetitions seems natural. Many parapsychology experiments also have these well-defined properties.

The other extreme would be situations such as commodity markets and other investment decisions. In these cases, the probabilities are not precisely known and are not constant over time.

Another important factor is the type of question being asked. A question such as “do some people have psychic abilities” is about an existing state of the world. On the other hand, a question such as “should I invest my retirement savings in the commodities market based on the predictions of a psychic” is a personal decision about future actions more than a scientific question about the state of the world. These latter situations usually involve potential risks and rewards, and are more difficult to conceptualize in terms of repeated observations.

For me, when a question focuses on an existing state of the physical world and can be evaluated with repeated observations using clearly applicable probability models, the methods of objective probability are a natural fit. When a question focuses on a personal decision that involves risks and rewards or poorly defined probabilities, subjective probabilities are a natural fit. Note that the operation of a market is based on the assumption that people have different subjective probabilities about the outcomes of future events. If everyone had the same beliefs, commodity markets and stock markets would not be possible because there would be only buyers or only sellers.

Scientific Research

Scientific researchers have traditionally taken great pride in being objective. They have modeled the basic properties of the world as being independent of the human mind. The philosophy of objective probability emerged from and is consistent with that worldview.

Subjective probability brings the diversity of a market environment to scientific research, and complicates analyses by including models of the personal beliefs in a human mind as well as models of the external world. Advocates of Bayesian methods, of course, argue that pure objectivity does not occur and that subjective probability is more realistic of what actually happens in science. However, another perspective is that a prominent injection of subjectivity into scientific methods will unnecessarily further degrade the admittedly imperfect objectivity of science and hinder the development of consensus.

These debates have no clear resolution at present. Both approaches have assumptions about how human beliefs should ideally be influenced by evidence. From my perspective, the claims that one approach is better than the other need to be evaluated empirically—and that remains to be done. Most scientific research, and particularly experiments, can be reasonably evaluated with either approach.

A more pragmatic question is what are the differences between these two approaches in practice? It appears to me that both approaches are logically valid and should eventually reach the same conclusions for scientific hypotheses about an existing state of the world. Ease of use and efficiency in reaching those conclusions may differ. Of course, classical methods currently have advantages from much more widely available software and more extensive practical experience with the methods and software. In addition, classical methods have widely accepted conventions for statistical methodology and simpler mathematical methods.

Differences in Describing Results

Classical hypothesis tests evaluate an experiment by comparing the observed outcome to the distribution of other outcomes that could have occurred if the results were produced by chance fluctuations. If the probability
or \( p \) value of the observed outcome under this null hypothesis is less than a prespecified criterion, the outcome is interpreted as a significant result that provides evidence for the alternative or experimental hypothesis.

A Bayesian analysis typically compares the probability that the alternative or experimental hypothesis is true with the probability that the null hypothesis is true, given the prior probabilities and the experimental data. The null hypothesis is that only chance is operating. The comparison is made by forming the ratio of the two probabilities. This ratio is the \( \text{odds} \) that the alternative hypothesis is true. Larger values of the odds are favorable for the alternative hypothesis.

For an estimate of the effect size with 95% confidence, a classical frequentist analysis describes the \( \text{confidence interval} \) as having a .95 probability that the range contains the true value. A Bayesian analysis describes the \( \text{credible interval} \) as having a .95 probability that the true value lies within this range. Obviously, few statistical users will consider the theoretical distinction between these descriptions to be important in practice.

**Differences in Calculating and Interpreting Probability**

Classical methods are based on hypothetical repetitions of the experiment independent of a person’s beliefs, whereas Bayesian methods are contingent on certain prior beliefs and give different results for different prior beliefs. The results of a Bayesian analysis cannot be assumed to apply to persons who have different prior beliefs. Classical methods have the assumption that objective scientific evidence can be developed without regard for a person’s prior beliefs.

The difference between Bayesian and classical perspectives can be seen easily when evaluating data with the binomial probability model. The binomial model has two key parameters: \( P \), which is mean chance expectation for a particular outcome occurring in an event or trial; and \( X \), the number of times that the outcome actually occurs in a group of trials. The probability model for a classical hypothesis test assumes that \( P \) is constant and \( X \) is a random variable. Data analysis is based on a probability distribution for \( X \) that represents all possible outcomes that could have occurred. Bayesian analysis switches this and takes \( X \) as constant and \( P \) as a random variable. Data analysis is based on a probability distribution for \( P \) that represents the beliefs and uncertainty in a person’s mind about the true value of \( P \).

**Classical Hypothesis Testing**

The usual convention is that the null hypothesis is rejected and the alternative hypothesis is accepted if the \( p \) value for the experiment is .05 or less. The \( p \) value is the probability of obtaining an experimental outcome as extreme or more extreme than the observed outcome if the null hypothesis is true. An incorrect conclusion or inferential error can occur for a hypothesis test. Accepting the alternative hypothesis when the null hypothesis is true is a Type I error. When the null hypothesis is true and the usual convention is applied, 5% of experiments can be expected to have a Type I error.

The technically justifiable conclusion for a classical hypothesis test is the simple binary outcome of whether or not the null hypothesis is rejected. Note that the \( p \) value is used to infer whether the null hypothesis is true, but it is not a direct measure of the probability that the null hypothesis is true. The philosophical assumptions and mathematical derivations for classical analyses provide the probability of obtaining the experimental outcome given that the null hypothesis is true. This is conceptually different than the probability that the null hypothesis is true given the experimental outcome—which is evaluated with Bayesian analysis. Greater confidence occurs for outcomes with smaller \( p \) values; however, this greater confidence is basically qualitative.

For a hypothetical example, consider an experiment attempting to detect a PK influence on an electronic random event generator (REG). If the outcome is 5,100 hits in 10,000 trials where the probability of a hit is .5 by chance, the \( p \) value for this outcome under the null hypothesis is \( p = .046 \) two-sided. This result is less than .05 and would be considered significant evidence for a PK effect.

The statistical power of an experiment is an important limiting factor that is often overlooked when interpreting the results. An experiment with a small sample size can fail to support the alternative hypothesis when that hypothesis is actually true. This is a Type II inferential error. The statistical power is the expected proportion
of experimental outcomes that will support the alternative hypothesis when that hypothesis is true. Power is usually determined based on estimated effect sizes from previous studies. The usual recommendation for experimental design is that the power should be at least .80, and preferably higher. Unfortunately, many experiments in parapsychology and psychology have been designed without regard for power and have had much lower power (Kennedy, 2013a).

When experiments with low power produce nonsignificant outcomes, the interpretation is ambiguous. The results could be due to the alternative hypothesis being false or due to the small sample size. Most experiments with low power can be expected to produce nonsignificant outcomes and to contribute little to scientific knowledge beyond providing estimates for designing more powerful studies.

Power analysis evaluates the statistical validity of a hypothesis test. Although a classical hypothesis test does not provide a direct probability that a hypothesis is true, power analysis provides probabilities that correct inferences will be made about hypotheses, and it can justify confidence in the experimental conclusions.

Bayesian Hypothesis Testing

A typical Bayesian analysis for a simple experiment uses methods for comparing which of two probability models is correct. One model is for the alternative hypothesis that an experimental effect is occurring and the other model is for the null hypothesis that only chance is operating. The posterior probability is calculated for each model. As usual, the posterior probability is calculated by updating the prior probability with the evidence from the data in the current study. The ratio of the posterior probabilities for the two models is the odds that the alternative hypothesis is true given the prior probabilities and the experimental data. These odds are a convenient way to compare two probabilities. The odds provide a direct measure of the probability that the alternative hypothesis is true, rather than an indirect inference as with classical analysis.

Widely accepted conventions have not been established for the magnitude of odds that is considered adequate evidence. Discussions of this topic usually reference Jeffreys (1961, p. 432), which (with minor rounding) describes odds of 3 to 10 as “substantial,” 10 to 32 as “strong,” 32 to 100 as “very strong” and greater than 100 as “decisive.” Odds of 1 to 3 are “not worth more than a bare mention.”

Odds of less than 1 can be inverted to provide the odds that the null hypothesis is true. This ability to provide direct quantitative evidence supporting the null hypothesis is an important feature of Bayesian analysis.

Jeffreys (1961, p. 435) said that he used an odds of 3 the way classical analysts use \( p = .05 \), and an odds of 10 the way classical analysts use \( p = .01 \). He also noted that inferential errors will sometimes occur with these criteria.

A Bayesian analysis of an experiment can have three possible outcomes: the final odds can (a) exceed the criterion supporting the alternative hypothesis, (b) exceed the criterion supporting the null model, or (c) fall into the intermediate zone that does not convincingly support either model. An experiment with a small sample size will likely have the latter result.

Odds near one indicate that the sample size is not adequate to evaluate whether the null hypothesis or alternative hypothesis is true. Probabilities for Type I error and power are based on classical ideas about hypothetical repetitions of an experiment. Inoue, Berry, and Parmigiani (2005) note that Bayesian methods for determining sample size have not become standardized and widely used. They also note that many Bayesian analysts use the concepts of Type I error and power in practice, and that this mixing of approaches is useful. Kruschke (2011) provides a useful discussion of power in the context of Bayesian analysis.

Prior probability for the alternative (psi) hypothesis. The starting point for a Bayesian hypothesis test is the prior probability that the hypothesis of interest is true. For a properly conducted experiment, this prior probability will be specified at the design stage prior to collecting data for the experiment. This prior probability is typically expressed as prior odds, which are the prior probability that the alternative or psi hypothesis is true divided by the prior probability that the null hypothesis is true. These prior odds will be adjusted or updated based on the evidence from the data in the current experiment to produce the final (posterior) odds that the alternative hypothesis is true.

In theory, the optimal strategy is to set the prior probabilities based on information from previous research. These are called informative priors. In an ideal world, the posterior probabilities from the first study would become the prior probabilities for the second study, and this would continue with each subsequent study. This strategy is optimal if the prior information is accurate. However, this strategy can also propagate bias if the previous studies have methodological errors, misconduct, selective reporting, or other biases—as sometimes occur in the real world.
If methodological bias is suspected in previous studies, the posterior probabilities from those studies need to be adjusted for the possible bias when developing the prior probabilities for the next study. This situation demonstrates the general principle that posterior probabilities may need to be modified based on subjective opinions about factors that are outside the mathematical models used for data analysis.

Informative priors are problematic for a controversial area like parapsychology because subjective beliefs about previous research vary dramatically. The points of dispute typically focus on the methodology in previous research. Skeptic David Marks (2000, pp. 306–307) states that when he began investigating the claims for remote viewing and ganzfeld experiments, his subjective prior probability that these effects can occur was one-tenth. After delving into the methodology and findings for these experiments, his subjective probability for ganzfeld ESP ability was one-millionth and for remote viewing was one-billionth. Wagenmakers et al. (2011) argued the prior probability for the psi hypothesis should be very close to zero and gave $10^{-30}$ as an example. Proponents of psi who find the existing methodology and evidence compelling have equally strong subjective beliefs. For example, Radin (2006, p. 276) presented the “odds against chance” for ganzfeld research to be $3 \times 10^{19}$ to 1 and the overall evidence for psi as $1.3 \times 10^{104}$ to 1. Although Radin appears to have mistakenly interpreted classical $p$ values as the probability that the null hypothesis is true, these odds can be accepted as his personal beliefs. These strong subjective beliefs, pro or con, will dominate the evidence from almost any reasonable experiment.

An alternative strategy is to use a noninformative prior probability that biases neither for nor against the alternative hypothesis. One obvious option is to assign equal prior probabilities to the alternative hypothesis and the null hypothesis. With this strategy, the prior odds are 1 and the experimental conclusion will be based on the evidence from the data in the current experiment. Noninformative priors emulate the classical assumption that scientific evidence from a study can and should be developed without regard for a person’s prior beliefs.

**Bayes factor.** The Bayes factor is a measure of the evidence from the data in the current experiment. It is the ratio for the probability of obtaining the experimental outcome under the alternative hypothesis divided by the probability of obtaining the experimental outcome under the null hypothesis. For a parapsychological experiment, this ratio is the odds that the experimental outcome is due to psi rather than to chance fluctuations.

The final (posterior) odds that the alternative hypothesis is true are derived by multiplying the prior odds that the alternative hypothesis is true by the Bayes factor. If a noninformative prior is used, the prior odds are 1 and the Bayes factor gives the final odds for the analysis. The Bayes factor is sometimes discussed as a likelihood ratio, or more precisely, the ratio of *marginal likelihoods*.

Although the Bayes factor is not influenced by the prior probability that the alternative hypothesis is true, the calculation of the Bayes factor incorporates a different prior probability in a very fundamental way—as discussed in the next section.

**Prior probability distribution for effect size.** Calculation of the Bayes factor requires that the probability for the experimental outcome be estimated under the alternative hypothesis. This calculation requires assumptions about the effect size for the alternative hypothesis ($P$ in the binomial model). The assumptions about effect size for this calculation are derived from a prior probability distribution that represents the beliefs and uncertainty in a person’s mind about the magnitude of the effect. For properly conducted research, this prior probability distribution for effect size should be specified before data collection begins. The strategy for selecting a prior probability for effect size can be informative or noninformative.

Not surprisingly, selecting a prior probability distribution for effect size based on previous research is subject to widely differing opinions for an area such as parapsychology. Different prior probability distributions for psi effect size were a major factor in the debates between Wagenmakers et al. (2011) and Bem et al. (2011), and between Jefferys (1990) and Dobyns (1992). The arguments may have no clear resolution given the subjective nature of probability in Bayesian analysis.

Unfortunately, attempts to find unbiased, noninformative prior probability distributions for effect size have also been problematic. Biases in favor of either the experimental or null model appear to be virtually inevitable—and can be counterintuitive. For example, a diffuse prior probability distribution that allows a wide range for effect size appears on the surface to represent a very open-minded prior belief. However, a diffuse prior makes the Bayes factor favor the null hypothesis.

For example, a uniform distribution is frequently recommended as a noninformative prior distribution for binomial effect sizes. The uniform prior gives equal probability to any effect size ($P$ in the binomial model) between
Using the online binomial Bayes factor calculator provided by Rouder (2012), 5,100 hits in 10,000 trials with a uniform prior of $\text{beta}(1,1)$ for the calculator gives a two-sided Bayes factor of 10.8 supporting the null hypothesis. This is considered strong evidence in favor of the null hypothesis. As noted above, the classical binomial analysis gives $p < .05$ supporting the alternative or psi hypothesis.

The fact that Bayesian analyses of small effect sizes tend to support the null hypothesis when classical analyses support the alternative hypothesis is well known among statisticians. Some proponents of Bayesian analysis argue that this shows that classical analyses are flawed (e.g., Jefferys, 1990). However, applying Bayesian analyses to simulated data indicates that these discrepancies can reflect low power and inferential errors in Bayesian hypothesis testing, particularly with diffuse prior probabilities (Kennedy, in press).

Much work remains to be done to understand the consequences of different prior probability distributions for effect size and to develop unbiased methods. Debates about appropriate prior probabilities for effect size can be expected given the current limited understanding and high potential for unrecognized bias. Most books on Bayesian analyses recommend model evaluations and sensitivity analyses that evaluate how different prior probabilities affect the final conclusions from an analysis.

**Objective Bayesian analysis.** Methods for objective Bayesian analysis attempt to minimize the subjectivity and potential biases from prior probabilities. One common practice is to base conclusions on the Bayes factor. However, the prior probability distribution for effect size is a fundamental part of the Bayes factor and currently can be expected to be a source of potential bias and controversy.

Like classical analyses, objective Bayesian methods strive to identify what a person should conclude given the experimental data and without biases from prior beliefs. As the use of Bayesian analysis for scientific research matures, conventions for objective analysis will likely become established. At present, widely accepted statistical conventions have not yet been established for generating objective scientific evidence from the intrinsically subjective nature of Bayesian analysis.

**Cumulative conclusions for each experiment.** An ideal Bayesian analysis with informative priors analyzes an experiment as a research synthesis that incorporates the findings from previous studies. The prior probabilities incorporate all previous information and are combined with the data from the current study to provide a cumulative conclusion. A separate research synthesis would be redundant.

That is a very different strategy than the classical concept of independent replications that are evaluated in a separate research synthesis. Independent replications have an important role in establishing the validity of a scientific finding because they counteract potential biases such as methodological errors, misconduct, and selective reporting. However, independent replication can be compromised by informative priors in Bayesian analyses. With informative priors, the results of previous studies are directly incorporated into the statistical analysis of an experiment, which makes the experiment not independent of the previous studies.

Objective Bayesian methods minimize the dependence between studies and enhance the validity of scientific findings. Both Bayesian and classical approaches assume that replications tend to be unbiased and will eventually counteract methodological biases. This assumption is discussed in a later section.

**Other Differences**

Bayesian methods require more information and more complex models than classical methods. Proponents argue that this is beneficial because it makes the subjective aspects of an analysis explicit and conspicuous. An alternative view is that the complexity and subjectivity significantly reduce the efficiency of reaching conclusions about scientific hypotheses. My opinion is that these speculations need to be supported with quantitative evidence. An obvious first step is to compare the statistical power or the amount of data needed to establish evidence for an effect using classical analyses versus the more complex Bayesian models (e.g., Kennedy, in press). Another important comparison is the amount of data needed to raise concerns about and counteract a case of scientific misconduct or methodological bias.

Adjustments for sequential analysis, optional stopping, and multiple analyses are very important in classical hypothesis testing. Many Bayesian analysts believe that these need no adjustments with Bayesian hypothesis tests. However, there are differing opinions on this (e.g., Berry & Hochberg, 1999). Here too, these discussions have been primarily based on theory with little practical empirical evaluation of inferential error rates.
In the 1990s, computer approximation methods were developed for Bayesian analyses. The writings prior to that time often discussed the great computational difficulty in working with Bayesian methods. Those discussions are no longer applicable. In fact, the general consensus now is that complex models such as hierarchical or multi-level random effects are often easier to work with using Bayesian methods. However, the methods use approximations, which raise concerns about identifying cases for which the approximations do not work well.

Advocates of Bayesian methods frequently criticize the use of $p$ values. Without getting into the myriad of technical details, my perspective is that classical hypothesis testing is useful and reliable for confirmatory experiments with prospectively set power and probability of a Type I error. I have not seen criticisms that invalidate the acceptance/rejection of hypotheses in this situation. A nonsignificant result in a well-powered experiment is evidence supporting the null hypothesis.

Both Bayesian and classical methods have certain assumptions that must be met for the results to be valid. Most classical analyses assume that the error terms (and thus the observations within a group or treatment) are independent and identically distributed. Bayesian methods assume the observations in a group or treatment are exchangeable, which means the observations are from the same distribution and any permutation of the observations is equally likely. Unrecognized confounding factors compromise both Bayesian and classical assumptions. In general, the applicability of these assumptions is most clear for studies that properly utilize randomness.

**Bayesian Analyses for Confirmatory Experiments**

The prior probabilities with Bayesian analyses apparently can alter the conclusions from almost any reasonable experiment. The influence can be in either direction—from conservative analyses to exaggerated effects. My impression from the past debates in parapsychology is that skeptics can relatively easily find and justify prior probabilities that produce unfavorable results, and proponents can as easily find and justify prior probabilities that are favorable. Dobyns (1992) shows this quantitatively for one example of psi data.

Those presenting conservative Bayesian analyses of psi experiments have generally assumed that the Bayesian results were correct and that they demonstrate that classical statistical methods are flawed (Berger & Delampady, 1987; Jefferys, 1990; Wagenmakers et al., 2011). The possibility that the selected Bayesian methods had low power or were biased was not seriously considered in these writings. However, initial investigations with simulated data suggest that these discrepancies may reflect low power and inferential errors for the Bayesian methods (Kennedy, in press).

Inferential error rates and power are useful measures for evaluating statistical methods and are important factors in designing confirmatory research. A statistical hypothesis test requires several methodological decisions that affect how the test performs. These decisions include the selection of the prior probabilities, specific statistical models, acceptance criteria, and sample size. Evaluations of inferential errors and power indicate how well these factors work together to provide an effective decision-making process. These evaluations indicate how reliably the Bayesian models of the human mind correctly detect conditions in the external world.

Bayesian analysts often argue that prespecifying acceptance criteria and sample size are not needed with Bayesian analyses. That argument implicitly assumes that inferential errors can be ignored with Bayesian analysis. That may be applicable for exploratory research; however, when the purpose of an experiment is confirmation of a controversial effect, more formal methods and quantitative evaluation of expected error rates are needed.

In theory, Bayesian hypothesis tests start with prespecified prior probabilities, which are then updated from the data in the current experiment to produce posterior probabilities and conclusions. In practice, the current state of Bayesian analysis has potential for researchers to start with the experimental data and then to adapt the prior probabilities to produce the desired conclusions. These adaptations can be applied in the context of the sensitivity analyses and model evaluations that are needed given the lack of established conventions and limited understanding of the implications of different prior probabilities. The potential for maneuvering to produce the desired results is enhanced by any ambiguity about the criteria that are considered acceptable evidence.

One important distinction between confirmatory and exploratory research is that decisions about analysis methodology should be made prospectively for confirmatory research. Methodological decisions for exploratory research can be made during data analysis.
FDA Recommendations

The U.S. Food and Drug Administration (FDA, 2010) developed guidance for using Bayesian statistics that offers a pragmatic and balanced perspective. The great majority of writings on Bayesian analyses focus on the exploratory stage of research. The FDA guidance is a rare exception that discusses methodology appropriate for confirmatory research. The guidance is for using Bayesian methods when seeking approval of medical devices. It was developed after public review and comment, and it would be appropriate in any situation in which the experimental results will be challenged, including parapsychology.

The FDA guidance takes the position that Bayesian analyses can be advantageous in some situations, but supporting analyses are usually needed to provide convincing evidence. The guidance recommends that the study design and protocol include (a) specification of the prior probabilities and models that will be used, (b) specification of the criteria that will be considered acceptable evidence, (c) operating characteristics for Type I errors and power of the analysis, (d) an estimate of the relative roles of prior probability versus the data from the current experiment in producing the final results, and (e) sensitivity analyses that determine how different prior probabilities and models affect these evaluations. These analyses require a substantial amount of effort that will typically be done prospectively with simulations. This information is also appropriate for Bayesian reanalyses that challenge findings from previous experiments.

Note that the FDA guidance does not indicate that a fixed sample size be specified. The guidance does not discourage Bayesian sequential analyses, but it does recommend that the decision criteria be specified in advance and that simulations be run to evaluate the effective probability of a Type I error and the power of the test.

In addition, the FDA recommendations are an effective way to address the uncertainties, differing opinions, and potential controversies with Bayesian analyses. Possible biases for small effects and diffuse prior probabilities need to be evaluated. As another example, two-sided tests in Bayesian analyses tend to be sensitive to prior probabilities and to favor either the alternative or null hypothesis. The evaluations recommended by FDA should show the net practical effects of any potential analysis issues that may or may not be recognized by the investigators.

My impression is that the theoretical developments in Bayesian analysis are significantly ahead of the practical experience in using the methods. I expect that significant pitfalls and reality checks will be found as more experience is gained with these methods. For example, with classical regression analyses, considerable practical experience was required before analysts began to usefully understand the implications of correlations among predictor variables (Hocking, 1983). At present, the writings on Bayesian analyses focus on theory with few simulation studies or empirical investigations of the applicability of the theory in practice. The supportive analyses recommended by the FDA guidance provide valuable assurances that the main analyses actually perform as advertised.

FDA takes the position that both Bayesian and classical approaches are valid if properly applied. The FDA guidance notes that “[w]hile the Bayesian approach can often be favorable to the investigator with good prior information, the approach can also be more conservative” (p. 10). The guidance also notes that “[t]he flexibility of Bayesian models and the complexity of the computational techniques for Bayesian analyses create greater possibility for errors and misunderstandings” (p. 10).

I would add that the flexibility, complexity, and subjectivity of Bayesian analyses make potential biases very difficult for a typical scientist to understand. Reliance on retrospective sensitivity analyses and model evaluations can introduce significant potential for biases. Without the supplemental analyses at the planning stage, an experiment cannot be expected to provide convincing evidence if challenged. Of course, more casual methods can be used for exploratory research.

Exploration, Confirmation, Application

Both classical and Bayesian analyses assume that experimental research is self-correcting and will eventually produce valid, compelling conclusions. Biased results from an experiment will be rectified by other experiments that are unbiased.

This idealistic philosophical hope does not consider the evidence that sustained levels of methodological noise and bias can occur in academic research—particularly in psychology and parapsychology (Ioannidis, 2012;
Kennedy, 2013a, 2014; Pashler & Wagenmakers, 2012; Wagenmakers, Wetzels, Borsboom, van der Maas, & Kieviet, 2012). The typical research practices in recent years in psychology and parapsychology have been exploratory and provide many opportunities for biases and misconduct. The amount of methodological bias and misconduct that actually occurs cannot be reasonably estimated. Undetected cases are likely. These exploratory research practices cannot be expected to resolve a controversy such as the occurrence of psi—particularly when the findings consistently differ among experimenters (Kennedy, 2014).

**Confirmation**

Well-designed confirmatory experiments are required to make experimental research self-correcting and to provide convincing, valid conclusions (Kennedy, 2013a, 2014; Pashler & Wagenmakers, 2012, Wagenmakers et al., 2012). Confirmatory methodology is well established for regulated medical research, but it has not been part of the research culture for psychology and parapsychology. Key components of confirmatory methodology include prespecification of statistical methods and acceptance criteria, sample size based on power analysis, public prospective registration of experiments, experimental procedures that make intentional or unintentional data alterations by one person difficult, documented formal validation of software, and sharing data for analyses by others (Kennedy, 2013a, 2013b, 2014; KPU Registry, 2014). Meta-analysis of exploratory studies does not eliminate the need for well-designed confirmatory research (Kennedy, 2013a).

With the advent of the KPU study registry (KPU Registry, 2012), confirmatory research methodology can be implemented for parapsychological research. Preregistration of statistical methods, including prior probabilities for Bayesian analyses, is fundamental for convincing confirmatory research. The standard for study registration in medical research and with the KPU registry is public, prospective registration, with requirements for the content of the registration (International Committee of Medical Journal Editors, 2005; KPU Registry, 2012). For comparison, study registration as currently offered by Open Science Framework (https://osf.io/) is basically self-registration that allows an experimenter to determine the content of the registration and to wait until the experimental results have been obtained before deciding whether to make the study and registration known to others. These options undermine much of the methodological value of study registration and allow an experimenter to withhold the experiment and/or registration if the results do not come out as hoped. However, if the results are favorable, the experimenter can misleadingly (and retrospectively) present the study as preregistered.

Skeptics have suggested that psi experiments are actually a control group that provides empirical evidence for the magnitude of methodological bias that occurs with current exploratory practices. That hypothesis is applicable to most experimental research in psychology as well, and remains plausible until confirmatory methodology is implemented—or until there is convincing evidence that does not rely on experiments.

**Application**

The most convincing evidence for psi would come from the development of practical applications. Psi clearly has great potential for practical applications that would substantially and conspicuously alter daily life. Sufficient effort and money have been devoted to developing applications of psi that tangible progress would be expected if the psi effects were even minimally reliable (Kennedy, 2003). Although there have been a few sporadic episodes of impressive results, the prevailing experience has been that efforts to develop applications were dropped when the results did not sustain the interest of those supporting and/or conducting the research. From this perspective, the absence of useful applications is evidence for the absence of convincing psi effects.

Given the lack of well-designed confirmatory research, my impression is that many scientists implicitly look to application rather than experiments when evaluating the evidence for psi. However, the rationale for that strategy is rarely discussed openly because it raises doubts about the integrity of scientific research far beyond parapsychology.

**Conclusions**

Both classical and Bayesian hypothesis tests are valid when properly applied. Well-designed confirmatory experiments with prespecified analyses and prospective evaluations of inferential errors and power are essential
for convincing evidence with either approach. However, the research culture for psychology and parapsychology has focused on exploration with very little attention to convincing confirmation. This situation undermines both classical analyses and Bayesian analyses. Unless confirmatory methodology is more widely implemented, strong evidence for psi will need to be based on the development of practical applications rather than on experimental research.

Evaluations of inferential errors and power measure the validity of a planned hypothesis test and are needed for both Bayesian and classical analyses. Classical analyses focus on modeling the uncertainty in the physical world—which tends to keep the models grounded in reality. Bayesian analyses focus on modeling the uncertainty in a human mind—which adds a layer of abstraction that provides greater flexibility and higher potential for the models to become unrealistic. Evaluations of inferential errors and power verify that all the components of a hypothesis test combine to provide useful inferences about reality. In addition, an estimate of the relative roles of prior probability versus the data from the current experiment is needed when planning confirmatory Bayesian analysis. Given the current lack of widely accepted conventions for Bayesian methods, prospective sensitivity analyses that evaluate different prior probabilities are also useful.

For most experiments, classical hypothesis tests require significantly less effort at present. One useful option would be to use classical methods as the primary analysis, plus a Bayesian analysis to explore and compare the methods. The effort required for Bayesian analyses will likely be reduced as applicable software evolves and the implications of different strategies for determining prior probability become more clearly understood through experience.

With classical analyses, 80% or more of properly designed confirmatory experiments are expected to provide independent evidence for an effect, with the great majority of experimenters obtaining successful replications. Corresponding expectations will likely be developed for confirmatory experiments with Bayesian analyses, including objective methods that minimize the propagation of bias from mistakes and misconduct in previous research. Of course, practical applications can be expected with this degree of reliability.

References

Bayesian and Classical Hypothesis Testing: Practical Differences


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Abstracts in Other Languages

Spanish

PRUEBAS DE HIPÓTESIS BAYESIANA Y CLÁSICA:
DIFERENCIAS PRÁCTICAS PARA UN ÁREA DE INVESTIGACIÓN POLÉMICA

RESUMEN: El uso del análisis Bayesiano y los debates relacionados con tal análisis están aumentando en áreas controvertidas de investigación como la parapsicología. Este artículo describe conceptualmente las diferencias filosóficas y de modelamiento entre los análisis Bayesiano y clásico, y las implicaciones prácticas de estas diferencias. No se han establecido convenciones estadísticas ampliamente aceptadas aún para el análisis Bayesiano en la investigación científica. Las recomendaciones de la FDA sobre el uso de métodos Bayesianos son apropiadas para los experimentos confirmatorios. Esta guía recomienda que el diseño del estudio y protocolo incluyan: (a) especificación de las probabilidades previas y modelos que se van a utilizar, (b) especificación de los criterios que serán considerados como evidencia aceptable, (c) características de funcionamiento para la probabilidad de error Tipo I error y poder del análisis, y (d) una estimación de la importancia relativa de la probabilidad a priori frente a los datos del experimento actual en la producción de los resultados finales. Tanto los métodos clásicos como los Bayesianos son válidos cuando se aplican correctamente en la metodología de confirmación que incluye especificación previa de métodos estadísticos y evaluaciones prospectivas de los errores inferenciales y el poder. Las evaluaciones de los errores inferenciales y el poder miden la validez de una prueba de hipótesis planeada, incluyendo el análisis bayesiano. Desafortunadamente, el uso de la metodología de confirmación ha sido poco frecuente en la psicología y la parapsicología.
LE TEST D'HYPOTHESES BAYESIEN ET CLASSIQUE :
DIFFERENCES PRATIQUES DANS UN CHAMP DE RECHERCHE CONTROVERSE

RESUME : L'utilisation de l'analyse bayésienne et les débats impliquant l'analyse bayésienne prennent une place de plus en plus importante dans des champs controversés tels que celui de la parapsychologie. Cet article décrit conceptuellement les différences philosophiques et de modèle entre les analyses bayésiennes et classiques, ainsi que les implications pratiques de ces différences. Des conventions statistiques largement acceptées n'ont pas encore été établies pour l'analyse bayésienne dans la recherche scientifique. Les recommandations de la FDA pour l'emploi des méthodes bayésiennes sont appropriées pour des expériences confirmatoires. Il est conseillé que la conception de l'étude et du protocole incluent (a) une spécification des probabilités a priori et des modèles qui seront utilisés, (b) une spécification des critères qui détermineront ce qui serait une preuve acceptable, (c) les caractéristiques opérantes pour la probabilité de l'erreur de Type 1 et la puissance de l'analyse, et (d) une estimation des rôles relatifs des probabilités a priori versus des données de l'expérience en question dans la production des résultats finaux. Tant les méthodes classiques que les méthodes bayésiennes sont valides lorsqu'elles sont correctement appliquées dans une méthodologie confirmatoire qui inclue la présélection des méthodes statistiques et des évaluations prospectives des erreurs inférentielles et de la puissance. Les évaluations des erreurs inférentielles et de la puissance mesurent la validité d'un test d'hypothèse planifié, y compris pour l'analyse bayésienne. Malheureusement, l'emploi d'une méthodologie confirmatoire est rare tant en psychologie qu'en parapsychologie.

German

BAYESSCHE UND KLASISCHE HYPOTHESENPRÜFUNG:
PRAKTISCHE UNTERSCHIEDE FÜR EIN KONTROVERSE FORSCHUNGS Gebiet

ZUSAMMENFASSUNG: Die Verwendung der Bayesschen Analyse und Diskussionen unter Einschluss der Bayesschen Analyse haben für kontroverse Forschungsgebiete wie die Parapsychologie an Bedeutung gewonnen. Dieser Artikel beschreibt konzeptuell die philosophischen und Modellierungsdifferenzen zwischen Bayesschen und klassischen Analysen und die praktischen Implikationen dieser Unterschiede. Bisher konnten sich statistische Konventionen in Bezug auf die Bayessche Analyse in der wissenschaftlichen Forschung noch nicht allgemein durchsetzen. Die Empfehlungen nach Vorgabe der FDA über die Verwendung der Bayesschen Methoden sind für Bestätigungsexperimente angemessen. Diese Vorgabe empfiehlt, dass die Studienplanung und das Protokoll folgendes einschließen: (a) die Spezifizierung der a priori-Wahrscheinlichkeiten und der verwendeten Modelle, (b) die Spezifizierung der Kriterien, die als akzeptable Evidenz eingestuft werden, (c) die wirksamen Kennzeichen für die Wahrscheinlichkeit des Fehlers 1. Art und der Analyse der Teststärke und (d) eine Abschätzung über die relative Rolle der a priori-Wahrscheinlichkeit versus der Daten im vorliegenden Experiment beim Zustandekommen der endgültigen Ergebnisse. Sowohl die klassische wie auch die Bayessche Methode sind zulässig, wenn sie korrekt bei der Bestätigungs methodologie angewendet werden, was die vorherige Spezifizierung der statistischen Methoden und prospektive Einschätzungen der Fehler beim Schlussfolgern und der Teststärke einschließt. Einschätzungen der Fehler beim Schlussfolgern misst die Validität einer geplanten Hypothesenprüfung, einschließlich der Bayesschen Analyse. Bedauerlicherweise wird die Bestätigungs methodologie in Psychologie und Parapsychologie selten angewandt.
ABSTRACT: Certain mediums are able to report accurate and specific information about the deceased loved ones (termed discarnates) of living people (termed sitters) even without any prior knowledge about the sitters or the discarnates and in the complete absence of any sensory feedback. This study aimed to investigate the phenomenology associated with, and accuracy of, readings for discarnates by claimant mediums under beyond double-blind conditions. At baseline, directly after a counterbalanced control condition, and after each of two identically formatted, scheduled phone readings for paired discarnates, 19 claimant mediums completed the Phenomenology of Consciousness Inventory (PCI). The two readings were then given global accuracy scores by the blinded sitters associated with the two discarnates. A significant anomalous information reception effect was demonstrated. However, this study did not identify any phenomenological variables that were correlates of mediums’ abilities. It would be prudent for future researchers to focus on the development of a quantitative measure specifically designed to investigate the phenomenology of mediumistic experience.

Keywords: mediums, phenomenology, Phenomenology of Consciousness Inventory

A growing public interest in the phenomenon of mediumship is clearly evident in the current rise of this topic in various aspects of popular culture. Numerous books, television shows, and movies featuring mediums—those who experience regular communication with the deceased—have moved from the obscure realm of the occult to the recognizable mainstream. The conventional scientific community has only just begun to recognize mediumship as a topic worth investigating when, in fact, the scientific study of mediums is over a century old.

Several comprehensive reviews of the history of mediumship methods (Beischel, 2007; Burdick & Kelly, 1977; Fontana, 2005; Schouten, 1994; Scott, 1972) and findings (Braude, 2003; Fontana, 2005; Gauld, 1983) are available. In addition, several recent single-blind (Robertson & Roy, 2001; Schwartz & Russek, 2001; Schwartz, Russek, Nelson, & Barentsen, 2001), double-blind (Kelly & Arcangel, 2011; Roy & Robertson, 2001, 2004; Schwartz, Russek, & Barentsen, 2002), and beyond double-blind (previously termed “triple-blind”; Beischel & Schwartz, 2007) studies producing positive results have been published. Also, one double-blind study that failed to obtain positive results (O’Keeffe & Wiseman, 2005) was performed, but the protocol contained numerous methodological flaws (discussed in Beischel, 2007). Finally, a beyond double-blind study also failing to produce a positive result was performed (Jensen & Cardeña, 2009) though the authors note that “this protocol did not provide an environment conducive to the medium’s confidence in her ability” (p. 70). (See also Rock, Thorsteinsson, & Tressoldi, in press.)

The positive contemporary studies generally replicate and extend the observations of early mediumship research: certain mediums can report accurate and specific information about the deceased loved ones (termed discarnates) of living people (termed sitters) through the process of anomalous information reception (AIR), that is, without any prior knowledge about the sitters or the discarnates, in the complete absence of any sensory feedback, and without using fraud or deception (e.g., Beischel, Biuso, Boccuzzi, & Rock, 2011). Mediums performing readings with proxy sitters provide information for living people who are not present at the reading. Consequently, “sitters” would be more completely defined as living persons who request a reading from a medium and who have a desire to receive information about one or more deceased people with whom they had an emotionally close relationship, irrespective of whether or not the sitters are present for or hear the reading as it takes place. Conversely,
“proxy sitters” are living persons who are present for the reading but are not the person for whom the information reported during a reading is intended. Proxy sitters may or may not have knowledge about the absent sitter or the deceased persons contacted during the reading.

Moreover, the information reported by these mediums cannot be explained as a result of fraud or “cold reading” (a set of techniques in which visual and auditory cues from the sitter are used to fabricate “accurate” readings) on the part of the mediums or rater bias on the part of the sitters (Braude, 2003; Fontana, 2005; Gauld, 1983).

Most previous—especially recent—mediumship research (e.g., Beischel & Schwartz, 2007) has been primarily concerned with empirically demonstrating a particular and replicable effect (i.e., AIR by mediums) without examining the phenomenological processes (subjective experiences) underpinning AIR during ostensible communication with the deceased. However, several authors have noted the importance of these types of experiences. Cardeña, Lynn, and Krippner (2000) proposed that “some anomalous experiences may have much to offer science in terms of clarifying its current boundaries and identifying how psychology, the neurosciences, and the social sciences can join hands to explain [the variety] of life” (p. 10).

“Phenomenology” is a term that refers to a philosophy, a research approach, and, in a more general way, the study of experience” (Pekala & Cardeña, 2000, p. 59). The phenomenological investigator engages in process-focused research examining “the way things are experienced by the experiencer, and ... how events are integrated into a dynamic, meaningful experience” (Hanson & Klimo, 1998, p. 286). With this type of analysis, the researcher is able to identify the essential aspects of the experience under investigation (Fischer, 1998).

Mediumistic experiences may be quantified using a methodology that was developed by Pekala (1985) to “operationally define, map and diagram states and altered states of consciousness” (p. 207). The methodology consists, in part, of a novel retrospective phenomenological assessment instrument referred to as the Phenomenology of Consciousness Inventory (PCI; Pekala, 1991). The PCI is a 53-item questionnaire consisting of 12 major dimensions or phenomenological (i.e., subjective) elements (e.g., Positive Affect, Altered Experience, Visual Imagery, Rationality), and 14 minor dimensions (e.g., Fear, Joy, Altered Body Image, Absorption). The PCI allows one to operationally define phenomena typically referred to as states of consciousness and altered states of consciousness (ASCs), considered by Rock and Krippner (2012) to be more appropriately described as “altered states of phenomenology,” by evaluating whether two necessary conditions for an ASC are satisfied: (a) the “pattern structure” (i.e., a covariance matrix of PCI major dimensions) for a treatment condition is significantly different from the pattern structure associated with a control or baseline condition, and (b) altered state of awareness scores are significantly higher for the treatment compared to control or baseline condition (Pekala & Kumar, 1986). The conjunction of (a) and (b) constitutes a sufficient condition for inferring an ASC (Pekala, 1991).

Rock and Beischel (2008) used the PCI to investigate mediums’ phenomenology during mediumship readings for discarnates versus a nonreading control condition. Analysis of the mediums’ PCI responses after each condition revealed that scores in the reading condition were statistically significantly higher than those in the control condition with regard to the PCI major dimensions Negative Affect and Altered State of Awareness. In contrast, scores in the reading condition were statistically significantly lower than in the control condition with regard to major dimensions Self-Awareness, Volitional Control, and Memory (Rock & Beischel, 2008). Thus, these phenomenological variables may correlate with ostensible AIR.

Indeed, one might argue, with some justification, that the next logical step in the field of mediumship research is to combine proof- and process-focused approaches to investigate the phenomenological correlates of the accuracy of discarnate readings by mediums. This would allow one to identify the subsystems of consciousness that are related to the accuracy of mediumship readings.

Objectives and Hypotheses

The present study aimed to investigate the phenomenological correlates of the accuracy of readings for discarnates by claimant mediums under beyond double-blind conditions. The following hypotheses were proposed:

**H1:** The sitters for whom blinded target readings were intended (i.e., intended sitters) will provide higher discarnate reading global accuracy ratings than sitters scoring decoy readings (i.e., control sitters).
**H2:** The intended sitters’ correct reading-choice rate will be above chance for the discarnate reading conditions.

**H3:** The PCI major variables Negative Affect and Altered State of Awareness will positively correlate with the accuracy of readings for discarnates, after controlling for baseline.

**H4:** PCI major variables Self-awareness, Volitional Control, and Memory will negatively correlate with the accuracy of readings for discarnates, after controlling for baseline.

**Method**

**Participants**

**Sitter participants.** Thirty-eight sitter participants over the age of 18 (31 females and 7 males) were recruited via the Windbridge Institute website (www.windbridge.org) and e-mail lists. Each sitter participant completed an online prescreening questionnaire including items in which one discarnate related to the sitter is chosen and is described in terms of personality and physical traits, favorite activities, and cause of death. Discarnates were paired based on an established pairing system (Beischel, 2007). Briefly, the information about each discarnate provided by the associated sitter is used to identify pairs of discarnates of the same gender that are most distinct in age, physical description (e.g., hair color, build, height), personality description (e.g., extraverted or introverted, rational or emotional), favorite activities (e.g., indoor or outdoor, group or solitary), and cause of death (e.g., part of the body affected, sudden or prolonged, natural or unnatural). This pairing process maximizes sitter-rater blinding and optimizes each blinded rater’s ability to distinguish between two readings during scoring. The pair of sitters associated with each pair of discarnates was also interviewed over the phone before being chosen to participate in the study.

**Medium participants.** At the Windbridge Institute, before participating in research prospective mediums are screened and trained using an eight-step certification procedure (Beischel, 2007). Briefly, the steps are as follows:

- Step 1: Written Questionnaire
- Step 2: Personality/Psychological Tests
- Step 3: Phone Interview (with an existing certified medium)
- Step 4: Phone Interview (with an investigator)
- Step 5: Two Blinded Phone Readings
- Step 6: Mediumship Research Training
- Step 7: Human Research Participants Training
- Step 8: Grief Training

Upon completion of the steps, the medium is termed a Windbridge Certified Research Medium (WCRM).

By collecting data from claimant mediums rather than certified mediums during this study, we created a wider range of mediumship ability in the sample, thus providing a greater chance of capturing any correlations that might exist between ability (i.e., accuracy) and phenomenology.

Nineteen claimant mediums (18 females and 1 male) participated in the study. Their mean age was 48.26 years ($SD = 8.20$).

**Materials**

The 53-item Phenomenology of Consciousness Inventory (PCI; Pekala, 1991) was used to quantify the mediums’ phenomenology at baseline and subsequently under three counterbalanced conditions: two readings and one control (see Procedure section below). As described above, the PCI items cover 26 phenomenological dimensions including 12 major (e.g., Positive Affect, Altered Experience, Attention, Volitional Control, Arousal) and 14 minor (e.g., Joy, Altered Body Image, Vividness of Imagery, Absorption) dimensions (Pekala, 1985; Pekala & Kumar, 1984, 1986;
Pekala & Levine, 1981, 1982). Minor dimensions are constituents of major dimensions. Participants are required to respond to each item on a 7-point Likert-type scale (Pekala & Wenger, 1983; Pekala, Wenger, & Levine, 1985). The PCI has been shown to possess adequate psychometric properties and good internal consistency (Pekala, Steinberg, & Kumar, 1986). In support of the scale’s criterion validity, Pekala et al. (1986) found that participants exposed to different stimulus conditions received statistically significantly different PCI scores. This finding suggests that the PCI can successfully distinguish between what are typically referred to as qualitatively different states of consciousness.

Analysis

Hypothesis 1 was tested using an independent *t* test. Hypothesis 2 was tested using an exact binomial test (one-tailed, as we were proposing a directional or psi-hitting effect). Hypotheses 3 and 4 were tested using a series of partial correlations.

Procedure

**Baseline.** To obtain a baseline measure of claimant mediums’ phenomenology, the PCI was administered to each claimant medium directly prior to the first of the three counterbalanced stimulus conditions to which they were exposed (see below for details of the DR₁, DR₂ and control conditions).

**DR₁ and DR₂ conditions.** Each of 19 claimant mediums performed Step 5 test readings, referred to as the DR₁ and DR₂ conditions, that consisted of two identically formatted, scheduled phone readings, each conducted during a separate phone call, for two paired discarnates and their respective absent sitters. The test reading protocol involved a blinded (Beischel, 2007) phone reading in which only the medium and an experimenter were on the phone and the experimenter served as a proxy for the absent sitter. These readings included five levels of blinding: (a) the medium is blinded to information about the sitter and the discarnate before and during the reading, (b) the sitter-raters are blinded to the origin of the readings during scoring, (c) the experimenter who consents and trains the sitter-raters (Experimenter 1) is blinded to which mediums read which sitters and which blinded readings were intended for which discarnates, (d) the experimenter who interacts with the mediums during the phone readings and formats the readings into item lists (Experimenter 2) is blinded to any information about the sitters and the discarnates beyond the discarnates’ first names; and (e) the experimenter who interacts with the sitters during scoring (i.e., e-mails and receives by e-mail the blinded readings; Experimenter 3) is blinded to all information about the discarnates, to which medium performed which readings, and to which readings were intended for which discarnates/sitters (Beischel, 2007).

In defense of providing mediums with the discarnate’s first name, Beischel (2007) argued:

The obvious criticism of this method is that the names themselves provide information to the medium that can be used for a type of cold reading. This does not appear to be the case.... Because the mediums are asked to provide specific information about the physical lives of each discarnate … it seems unlikely that they could obtain the necessary information solely from these names. In cases in which the names provide overt evidence about the discarnates’ ethnicities and in turn their probable physical descriptions (e.g., Scandinavian: Lars or Signild, African: Naem or Kianga, Irish: Seamus or Siobhan, Hispanic: José or Manuela, Japanese: Mamoru or Kiyoshi, and so on) or provide other identifying information (e.g., religion), either a pair is chosen to include two discarnates of the same ethnicity or religion or the discarnates are chosen only for studies in which blinding is not necessary. (pp. 41–42)

In the present study we adopted this methodological control.

During the test readings, the medium was given the first name of the discarnate and then asked the following questions about the discarnate’s physical life:

1. What did the discarnate look like in his/her physical life?
2. Describe the personality of the discarnate. What were the discarnate’s hobbies, activities, or interests?
3. What was the discarnate’s cause of death?
4. Does the discarnate have any comments, questions, requests, or messages for the sitter?
5. Is there anything else you can tell me about the discarnate?

Each reading was then transcribed, formatted into a list of individual items, and blinded to remove any reference to the discarnate’s name. Each of the two formatted readings were then scored for accuracy (or “fit”) by each of the two associated blinded sitters; each sitter scored that sitter’s own reading as well as the reading intended for the other sitter without knowing which was which. Thus, each sitter served as a control rater for the other sitter’s reading. The sitters provided a numeric score for the overall reading using the Whole Reading/Global Scoring System (WRGSS), estimated the percentage of items they felt are accurate, and chose which of the two readings they believed was intended for them. Sitters were provided with the readings for scoring and returned their scores by e-mail. The options for the WRGSS are as follows:

6: Excellent reading, including strong aspects of communication, and with essentially no incorrect information.
5: Good reading with relatively little incorrect information.
4: Good reading with some incorrect information.
3: Mixture of correct and incorrect information, but enough correct information to indicate that communication with the deceased occurred.
2: Some correct information, but not enough to suggest beyond chance that communication occurred.
1: Little correct information or communication.

Directly after each of the two test readings for the paired discarnates, the prospective research mediums completed and returned the PCI.

**Control condition.** The medium and the experimenter also participated in a “control” phone call in which the medium was read a story containing facts about a gender-matched deceased person’s life and reported the answers to the same questions contained in the reading conditions based solely on the information contained in the story. Subsequently, the medium completed the PCI.

The order that the mediums read each of the discarnates and the placement of the control condition in relation to those two readings was randomized. In addition, a 7-day “wash-out” period took place between each of the three phone calls.

During analysis, the accuracy scores given by the sitter for whom the reading was intended were compared to the scores given by the unrelated paired control sitter for the same reading. The comparison of the “intended” or target scores to the “control” or decoy scores reflects the specificity of an individual medium’s statements and is one of the criteria we use in determining if the medium goes on to the remaining screening steps. The sitters’ control scores should not be confused with the control condition. Each sitter must score two readings (one target and one decoy) without knowing which is which to control for rater bias. The control sitters’ scores serve as a control for the mediums’ accuracy, but not for the mediums’ phenomenology.

As previously stated, the two DR conditions were identical. Thus, the global accuracy and phenomenology scores associated with the two conditions were pooled in the statistical analysis below. Hereafter, “DR scores” denotes the pooled DR₁ and DR₂ scores, whereas “DR condition” refers to the combined DR₁ and DR₂ conditions.

**Results**

**Planned Analyses**

**H1:** The intended sitters will provide higher DR global accuracy ratings than control sitters. The intended sitters gave a significantly higher DR global accuracy rating ($M = 2.82$, $SD = 1.41$) than the control sitters ($M = 2.08$, $SD = 1.46$), $t(74) = 2.24$, $p = .03$. The hypothesis was supported.

**H2:** The intended sitters’ correct reading-choice rate will be above chance for the DR condition. Participants ($N = 38$) produced a correct reading-choice rate of $68.42\%$ ($z$ score = 2.12, 26 correct guesses with $P_{MCE} = 50\%$), which was statistically significant, $p = .02$ (one-tailed). The hypothesis was supported.
The corresponding moderate effect size (ES = $z/\sqrt{n}$) is .34. An effect size of zero is consistent with chance. Rosenthal and Rosnow (1991, p. 444) suggest that effects of .10, .30, and .50 are considered small, medium, and large, respectively. Utts (1995, p. 294) suggests effects of .20, .50, and .80 are considered small, medium, and large, respectively.

**H3: PCI major variables Negative Affect and Altered State of Awareness will positively correlate with the accuracy of readings for discarnates, controlling for baseline.** For the DR condition, after Bonferroni adjustments for multiple tests (adjusted alpha = .05/2 = .025), the partial correlation between Negative Affect and global accuracy was not significant, $r(16) = .254, p = .16$. Similarly, the partial correlation between Altered State of Awareness and global accuracy was not significant, $r(16) = .09, p = .36$. The hypothesis was not supported.

**H4: PCI major variables Self-Awareness, Volitional Control and Memory will negatively correlate with the accuracy of readings for discarnates, controlling for baseline.** For the DR condition, after Bonferroni adjustments for multiple tests (adjusted alpha = .05/3 = .017), the partial correlation between Self-Awareness and global accuracy was marginally significant and in the hypothesized direction, $r(16) = -.43, p = .04$. However, Volitional Control and Memory were not significantly related to global accuracy, $r(16) = .26, p = .15; r(15) = .14, p = .30$. The hypothesis was not supported.

**Post Hoc Analyses**

**Differences between the DR condition and the control condition with regard to the post-test PCI major dimensions scores, after controlling for baseline.** In the interests of exploring the possibility that the DR condition might be associated with an ASC relative to the control condition, after partialing out baseline scores, we conducted a series of post hoc tests. Table 1 presents the PCI major dimension means and standard errors for each condition controlling for baseline scores.

<table>
<thead>
<tr>
<th>Variable</th>
<th>1 Control $M (SD)$</th>
<th>2 DR $M (SD)$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Positive Affect</td>
<td>1.27 (0.21)</td>
<td>2.07 (0.16)</td>
</tr>
<tr>
<td>Negative Affect</td>
<td>0.89 (0.34)</td>
<td>0.91 (0.19)</td>
</tr>
<tr>
<td>Altered Experience</td>
<td>1.61 (0.31)</td>
<td>2.75 (0.24)</td>
</tr>
<tr>
<td>Visual Imagery</td>
<td>3.42 (0.40)</td>
<td>4.12 (0.28)</td>
</tr>
<tr>
<td>Attention</td>
<td>4.08 (0.25)</td>
<td>4.45 (0.13)</td>
</tr>
<tr>
<td>Self-Awareness</td>
<td>4.04 (0.33)</td>
<td>3.01 (0.25)</td>
</tr>
<tr>
<td>Altered State</td>
<td>1.76 (0.40)</td>
<td>3.17 (0.25)</td>
</tr>
<tr>
<td>Internal Dialogue</td>
<td>1.78 (0.48)</td>
<td>1.78 (0.42)</td>
</tr>
<tr>
<td>Rationality</td>
<td>5.17 (0.19)</td>
<td>4.48 (0.17)</td>
</tr>
<tr>
<td>Volitional Control</td>
<td>3.85 (0.37)</td>
<td>2.91 (0.19)</td>
</tr>
<tr>
<td>Memory</td>
<td>4.59 (0.26)</td>
<td>4.30 (0.19)</td>
</tr>
<tr>
<td>Arousal</td>
<td>1.72 (0.32)</td>
<td>1.75 (0.17)</td>
</tr>
</tbody>
</table>
It is more parsimonious to perform multivariate than univariate analyses when one wishes to examine group differences on multiple, related dependent variables. Consequently, in the case of the PCI major dimensions, multivariate analyses of covariance (MANCOVAs) were performed. MANCOVAs yield multivariate results (i.e., results concerning the combined dependent variables). If a significant multivariate effect is found, examination of the various univariate effects (i.e., results concerning each individual dependent variable) is warranted. Thus, a “multivariate effect” refers to an effect on combined dependent variables (for MANCOVA, see Tabachnick & Fidell, 2007). We chose to average the PCI scores across the two DR conditions because these two stimulus conditions were identical.

Due to the large number of covariates (12 PCI dimensions) and the small sample size ($N = 19$), two MANCOVAs were performed, each controlling for six covariates. After controlling for baseline PCI dimension scores (Positive Affect, Negative Affect, Altered Experience, Imagery, Attention, and Self-Awareness), there was not a significant multivariate effect between the DR condition and the control condition with regard to PCI post-test scores, $F(6, 6) = 3.78, p = .07$, partial $\eta^2 = 0.79$.

The second MANCOVA controlled for baseline scores on the remaining six PCI variables: Altered State of Awareness, Internal Dialogue, Rationality, Volitional Control, Memory, and Arousal. There was not a significant multivariate effect between the DR condition and the control condition with regard to PCI post-test scores, $F(6, 6) = 2.26, p = .17$, partial $\eta^2 = 0.69$.

**The pattern structure associated with the DR conditions is different relative to the pattern structure associated with the control condition.** Based on the decades-old assumption that various ostensibly psi-conducive procedures (e.g., the ganzfeld) induce ASCs (Alvarado, 1998), it seemed pertinent to evaluate whether the pattern structure (i.e., a covariance matrix consisting of PCI major dimensions) associated with the DR condition was significantly different relative to the control condition.

A Box M test of equality of covariance matrices (Jenrich, 1970) was performed to assess the pattern structure of the DR condition relative to the control condition. Note that the Box M statistic “tests the homogeneity of variance-covariance matrices” (Tabachnick & Fidell, 2007, p. 252). The Box M test revealed that the difference between the covariation matrices was not significant, $Box M = 154.08, F(78, 3842.99) = 1.23, p = .08$. In accordance with Rock, Storm, Harris, and Friedman (2013) we are mindful that

The Box test is typically held to be overly sensitive with regard to the detection of differences between independent correlation matrices. Consequently, convention dictates that the alpha level associated with the Box test should be set at $p < .001$ (Tabachnick & Fidell, 2007). We note that the Jenrich (1970) Test is the appropriate statistical procedure to assess pattern differences associated with the 12 major dimensions of the PCI (Pekala, 1991). However, Pekala (1991, p. 235) asserts that the Jenrich Test is a “large-sample multivariate procedure” requiring a minimum of 60 participants per condition (provided that all 12 major dimensions of the PCI are being examined). Given that the present study did not meet this sample size requirement, the Jenrich Test was not appropriate. Consequently, a Box M test comparison was performed (Pekala, 1991). (p. 110).

**Discussion**

The significant difference between intended and control sitters’ global accuracy ratings ($H1$) suggests that the claimant mediums were able to demonstrate anomalous information reception under test conditions with five levels of blinding. This finding is consistent with previous (e.g., Beischel & Schwartz, 2007) and ongoing (Beischel et al., 2011) research. The present study’s beyond double-blind design appears to eliminate telepathic scanning of the experimenter’s mind by the medium as an explanation for AIR; however, it is possible that the mediums scanned the minds of, for example, blinded sitters or the discarnate’s other family members. In addition, the design does not address the issue of clairvoyance whereby the medium remotely views physical objects (e.g., obituaries, family photographs) that contain pertinent information about discarnates. However, because we did not include a debriefing component (i.e., revealing the characteristics of the discarnate to the medium at the end of the experiment), the medium’s precognition of his or her own future mental state appears to be eliminated as a potential source of AIR.
The intended sitters produced a significant above-chance correct reading-choice rate. We note that the present study’s correct-reading choice rate (i.e., 68.42%) and corresponding ES (.34) is marginally lower than Beischel and Schwartz’s (2007) findings (correct reading-choice rating = 81%; ES = .50). This disparity may be due to the participation of claimant mediums in the present study rather than Windbridge Certified Research Mediums. Nonetheless, we note that the present study’s ES exceeds the ES for the ganzfeld in the major independent meta-analyses by Honorton (1985), Storm and Ertel (2001), Bem, Palmer, and Broughton (2001), and Tressoldi, Storm, and Radin (2010).

In the search for phenomenological correlates of accuracy of readings for discarnates (see results for H3 and H4), after controlling for pre-test Self-Awareness scores, the partial correlation between post-test Self-Awareness and global accuracy was marginally significant and in the hypothesized direction. The Self-Awareness dimension evaluates the degree to which the percipient is “aware of being aware of myself” (Pekala, 1991, p. 132). Consequently, it appears that during a discarnate reading, mediums’ self-awareness dissipates as their attentional focus is directed towards the discarnate. Additionally, the dissipation of self-awareness is perhaps to be expected if one accepts the findings of previous research, which indicates that mediums assume the self-sense of discarnates during readings for sitters (Rock, Beischel, & Cott, 2009).

In terms of post hoc analyses, a nonsignificant multivariate effect was found for condition (DR versus control) with regard to the various PCI major dimensions in this participant sample. This finding suggests that there was not a significant overall effect for the condition factor on the posttest PCI dimensions, partialing out the influence of pretest PCI dimension scores. Similarly, the pattern differences between the DR condition and the control condition were not significant. These results suggest that, compared to the control condition, the DR condition did not induce a “major reorganization in pattern structure that is hypothesized by Tart (1975) to be associated with an altered state of consciousness” (Woodside, Kumar, & Pekala, 1997, p. 84). In other words, the pattern structure of phenomenological elements (e.g., Negative Affect, Volitional Control, Rationality) that constitute the “state of consciousness” associated with the treatment condition was not significantly altered relative to the pattern structure of the “state of consciousness” associated with the control condition. This finding is consistent with Rock and Beischel (2008) and suggests that claimant mediums’ cognition associated with the DR condition was not fundamentally different from their cognition in the control condition.

It is noteworthy that various methodological issues including forgetting, reconstruction errors and confabulation, verbal description difficulties, lack of independent verification, demand characteristics, and inaccessibility due to “state-specific” memory, limit the usefulness of the PCI, and phenomenological inquiry in general, when used in the absence of other approaches (Pekala & Cardeña, 2000). Consequently, it is prudent to strengthen phenomenological methodology by using it in conjunction with, for instance, a neuroscientific approach (see Jamieson & Rock, 2014). This composite methodology is referred to as neurophenomenology (Laughlin & Rock, 2013). Thus, future research might use a PCI-based approach to mediumship in conjunction with, for instance, electroencephalography and functional magnetic resonance imaging.

We are mindful that “the PCI is a general measure of phenomenological responses to stimulus conditions and was, therefore, not specifically designed to quantify the phenomenology” of mediumistic states (Rock et al., 2013, p. 111). Thus, the PCI may not measure various phenomenological variables that are fundamental constituents of mediumistic states and, therefore, potentially significant correlates of AIR. Future research might use a complementary mixed method, whereby the phenomenology of a mediumship session is quantitatively assessed using the PCI and qualitatively assessed using semi-structured interviews designed to yield comprehensive constituent themes. The essential aspects of the experience of mediumship that the PCI dimensions fail to “tap” may be “captured” by the semi-structured interviews, and vice versa. By triangulating these methods, one may be able to extrapolate items for a self-report measure designed specifically to quantify the phenomenology of mediumship.

Future studies aiming to investigate the phenomenological effects of differential information sources (e.g., a discarnate versus a living agent) could modify the present study’s design by including a placebo or “sham” reading condition, whereby mediums are blinded to the fact that they are instructed to communicate with a fabricated, rather than a factual, discarnate. However, if the blind is compromised and the medium determines that the discarnate has been fabricated, the source of this determination could include living agent telepathy (e.g., the experimenter) or communication with a real discarnate that is presumably knowledgeable regarding the sham reading condition.
It is also possible that a nonbreaking of the blind is due to the medium communicating with a mischievous real discarnate masquerading as the fabricated discarnate. Consequently, the “slippery” dynamics of mediumship render problematic any attempt at blinding the medium to the reading condition (Rock, 2014). In addition, the use of a sham condition involves important ethical concerns that we have discussed previously (Beischel & Rock, 2009).

The present study demonstrated a significant anomalous information reception effect with claimant mediums under blinded conditions. However, this study did not identify phenomenological variables or sub-systems of consciousness that are correlates of mediums’ abilities and, thus, highlights the potential importance of developing a quantitative measure specifically designed to investigate the phenomenology of mediumistic states. Nonetheless, all significant findings should be considered tentative pending replication, unless they are replications themselves.

References


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**Abstracts in Other Languages**

**Spanish**

LECTURAS DESENCARNADAS POR PRESUNTOS MÉDIUMS: EVALUACIÓN DE LA FENOMENOLOGÍA Y PRECISIÓN EN CONDICIONES DE DOBLE CIEGO

RESUMEN: Algunos médiums aseveran ser capaces de proporcionar información precisa y específica sobre seres queridos fallecidos (desencarnados) y sobre personas vivas (asistentes) incluso sin ningún conocimiento previo sobre los asistentes o los desencarnados y en ausencia total de cualquier retroalimentación sensorial. Este estudio tuvo como objetivo investigar la fenomenología asociada y la precisión de las lecturas sobre desencarnados en condiciones superiores a las de doble ciego. Al inicio del estudio, directamente después de una condición de control contrabalanceada y después de dos lecturas de idéntico formato por teléfono programadas sobre desencarnados, 19 médiums completaron el Phenomenology of Consciousness Inventory (PCI). Dos evaluadores enmascarados (ciegos) con conocimiento de los desencarnados dieron su evaluación global sobre la precisión. Se demostró un efecto significativo de recepción anómala de información. Sin embargo, este estudio no identificó ninguna variable fenomenológica correlacionada con las habilidades de los médiums. Sería prudente que futuros investigadores se centraran en el desarrollo de una medida cuantitativa específicamente diseñado para estudiar la fenomenología de la experiencia mediumnística.

**French**

CONTACTS AVEC LES DEFUNTS PAR DES MÉDIUMS : ANALYSE DE LA PHENOMENOLOGIE ET DE LA PRECISION DANS DES CONDITIONS DE DOUBLE-AVEUGLE

RESUME : Certains médiums sont capables de fournir des informations précises et spécifiques sur des proches décédés (dits « désincarnés ») de personnes vivantes (dits « sitters »), même sans avoir de connaissances préalables sur les sitters ou les désincarnés, en en l’absence complète de tout feedback sensoriel. Cette étude vise à investiguer la phénoménologie associée et la précision de ces contacts avec des défunts par des personnes se revendiquant médiums, dans des conditions de double-aveugle. L’Inventaire de la phénoménologie de la conscience (PCI) a été rempli par 19 médiums au début de l’étude, directement après une condition de contrôle contrebalancée, et après chacun des deux appels téléphoniques, de format identique, pour des contacts avec des désincarnés apparisés. A la suite des contacts, les sitters donnaient en aveugle des scores globaux de précision avec chacun des deux désincarnés. Un effet significatif d’acquisition anomale d’information fut démontré. Toutefois, cette étude n’identifie aucune
des variables phénoménologiques qui avaient pu être corrélées avec les capacités des médiums. Il serait prudent, lors de futures recherches, de se focaliser sur le développement d’une mesure quantitative afin d’étudier spécifiquement la phénoménoLOGie de l’expérience médiumnique.

German

AUSSAGEN ÜBER VERSTORBENE DURCH SELBSTERNANNTTE MEDIEN: DIE BEURTEILUNG IHRER PHÄNOMENOLOGIE UND GENAUIGKEIT UNTER DOPPELBLINDBEDINGUNGEN

NATURAL ANOMALOUS COGNITION TARGETS:
A FUZZY SET APPLICATION

By Edwin C. May, Lory Hawley, Vinay Chaganti,* and Namita Ratra*

ABSTRACT: Fuzzy sets have previously been applied to photographic target stimuli. In this paper, we use the same technology to construct a set of natural sites in the San Francisco Bay area. We created a Universal Set of Elements containing 13 items which created five orthogonal categories: Buildings, Ponds, Towers, Gardens, and Bridges; 22 sites were identified and were independently coded by two volunteers. Thus, the fuzzy sets that represent these sites are a reasonable consensus of their content. This paper describes the construction process in detail, and the appendix shows each site and its associated fuzzy set.

Keywords: anomalous cognition, remote viewing, RV stimulus, RV targeting, target pool construction

A traditional set (called a crisp set) is simply a collection of items that share a common property. For example the set of cities that possess 1,000,000 people or more includes New York, New Delhi, London, and Hong Kong. However, a city that has a population of 999,999 (i.e., one person short of a million) is not a member of this set of cities by definition. Zadeh (1965) realized that this kind of reasoning is not the way people actually think about problems. In this example there is no real difference between a city of one million people and one that is one person short of that number. Like crisp sets, fuzzy sets are simply a collection of items that generally share a common property. Using fuzzy sets, it is possible to construct a set of “reasonably large cities.” Here cities that differ by an insignificant number of people are still member of this set. In other words, fuzzy sets allow the quantification of the ambiguous.

This concept has broad appeal outside the application to psi research. Because, for example, experimental psychology and cognitive psychology are concerned with how people “think/feel” about various concepts, using fuzzy rather than crisp descriptions of the concepts is valuable (Sternberg, 2008).

Honorton (1975) may have been one of the first to encode pictorial information in a quantitative way. Honorton used 10 concepts from Van de Castle’s dream research (Hall & Van de Castle, 1966; Van de Castle et al., 1972). Dream concepts included color, mythological characters, food, human artifacts, etc. These were turned into a binary system of characteristics either present or absent from a picture (usually a collage). For example, a colorful collage of superman sliding down a disembodied leg into a pile of bananas would have four of a possible 10 binary bits each with the value of one. For a variety of reasons this clever attempt was shortly abandoned, in part because of the lack of thematic content for any given collage and some of the 1,024 pictures in the set were a bit “racy” even for 1975 Brooklyn, New York. One of us (May) was present at Maimonides when this target pool was under construction. So much of this is first-hand recall. Then Jahn, Dunne, and Jahn (1980) advanced the free-response judging methodology by using a set of descriptors for the target material. Later, however their free-response methodology was critiqued (Hansen, Utts, & Markwick, 1992), whereas the PEAR group offered a rebuttal (Dobyns, Dunne, Jahn, & Nelson, 1992).

May et al. (1990) first applied fuzzy sets for the analysis of anomalous cognition (then called remote viewing) data and photographic targets; however their first attempt was cumbersome and the universal set of elements (USE)—all possible elements that might describe any of the target photographs—contained 130 separate elements, thus making it difficult to encode any given photograph. By “universal” we mean only within the jargon of fuzzy sets; that is, this “universal” set of elements is only for the design of the fuzzy sets for the particular sites and responses. It cannot be applied to any other circumstance. So by definition, it is not a generalized concept. For example, a USE for sites in India would not apply to sites in the US, and a USE for sites in California would not work for sites in the Eastern half of the US.

Using a different photographic target pool May et al. (2012) reduced the USE down to a more manageable set of 24 elements.
Our first attempt to apply this technique to natural sites occurred as part of the Laboratories for Fundamental Research workshop hosted by the School of Management at GITAM University near Visakhapatnam, Andhra Pradesh, India in 2011. Over the course of an 8 week, 8 hours/day, 6 days/week intensive, we carried out a preliminary search of sites within one half hour drive of the university. As with any new approach there were numerous false starts both with site selection as well as the design of a proper USE. This part of India is on the coast, and it also has a number of high-tech industrial sites. Figure 1 shows a sample of some of the sites that were under consideration.

![Sample outdoor sites near Visakhapatnam, India.](image)

**Figure 1.** Sample outdoor sites near Visakhapatnam, India.

The workshop did make progress with regard to developing a USE in that a cluster analysis showed promise with regard to creating categories of sites, buildings for example, that were orthogonal. Unfortunately, the workshop came to an end before we could complete the target pool construction and begin collecting anomalous cognition data.

**A San Francisco Bay Area Natural-Site Target Pool**

Before we embark on the target pool description, we provide a brief tutorial for the entropy concepts we use below. The concept of entropy arises from classical thermodynamics in physics. Although the mathematics of various theories rapidly becomes difficult, the conceptual framework is rather straightforward. One approach is to think of entropy as a measure of chaos or to the related idea of uncertainty. Ice, for example, has much lower entropy than does water. Why? Because the molecules of water in liquid form are bouncing around in a chaotic fashion; whereas in ice, these same molecules are all lined in an ordered array we call a crystal. Also in liquid water the position of a given molecule is very uncertain (i.e., high entropy); whereas in ice the position of a molecule is far more certain because it is trapped in a crystal and is not going anywhere. So when ice melts it undergoes a change of thermodynamic entropy, transitioning from ordered molecules of water (ice) to disordered molecules (water).

In the experiment described below, when liquid nitrogen becomes a gas, the molecules in the gas are far more disordered than they are in a liquid, and, thus they experience a change of entropy. We developed a natural site-target pool as part of a test of the hypothesis that changes of thermodynamic entropy at a natural target site will enhance the quality of the anomalous cognition of that site. In a future experiment, the change of entropy at the site will be tested by pouring 3 liters of liquid nitrogen (LN) at a randomly chosen site while an experimenter is located there.

When the very cold liquid nitrogen (-320.4 F°), becomes a gas, that phase change results in a change of thermodynamic entropy. The sites were chosen, in part, to be relatively entropy neutral so that the entropy change
associated with 3 liters of liquid-to-gaseous nitrogen would be substantially above any changes of entropy that might naturally be associated with a target site.

The LN condition will be true for half of the sessions, and for the remaining half there will be no LN poured at the site. The conditions will be counterbalanced across the six sessions.

Our previous work on entropy and its gradients (May, 2011; May, Spottiswoode, & James, 1994; May, Spottiswoode, & Faith, 2000) used computational Shannon entropy and its gradients of AC photographic target stimuli as the dependent variables. Thus, these parameters were intrinsic properties of the photographs. Here, however, we exercised great care to keep the intrinsic entropy and its gradients as qualitatively uniform across the natural sites as possible. For example, sites near power lines or substations, or sites for which the intrinsic entropy might be large and change, were all excluded. Thus, we are hopeful that the change of state of the liquid nitrogen will be larger than any other sources of entropic change. This, of course, is an empirical question to be addressed in the study.

Following the lead from the workshop in India, we first identified 31 candidate sites on which to develop a USE; however, we reduced these to 25 by inspection. The list was reduced further to 22 in that one site was adjacent to high-voltage transmission lines, another had limited hours when the site was available to the public, and access to a third was only through a prison school. As in earlier work, we computed the effective distance between each pair of sites as:

$$d_{j,k} = 1 - \frac{\sum_{i=1}^{N} \min(\mu_{i,j}, \mu_{i,k})}{\sqrt{\sum_{i=1}^{N} \mu_{i,j}^2 \times \sum_{i=1}^{N} \mu_{i,k}^2}},$$

where $N$ is the number of elements in the USE (13 in this case), and the $\mu$’s are the fuzzy set membership values for the respective sites $j$ and $k$. The distance is not a physical distance between two sites; rather, it is a conceptual distance based upon the universal set of elements. For example, a site with lots of man-made buildings and no trees is conceptually very far “away” from a park with nothing but trees.

The fraction part of Equation 1 ranges from zero to one, so that $d_{j,k}$ varies nearly continuously from one to zero, zero meaning the two targets are identical. One way to see that Equation 1 represents a “distance” measure in element space and thus adheres to a generalized version of the pythagorean theorem is to reduce the space to only two dimensions. So in Equation 1, we let $j$ have the values 1 and 2 and likewise for $k$. Now we have a two-dimensional parameter space with only two points in the space. Clearly the distance between these two points in this restricted space satisfies the requirement for the pythagorean theorem. Because, in general, these pairwise $d_{j,k}$ are “distances” in element space, standard cluster analysis can show visually if pairs of targets are alike or not or if groups of targets are orthogonal to other groups. Please see Figure 4 below as an example of our new target pool of sites.

Table 1 shows the USE that, by trial and error, ended up as the final set. It is important to note that these elements are site-specific and most likely would be completely different for another set of sites in the Bay Area or in any other location.

<table>
<thead>
<tr>
<th>Buildings</th>
<th>Land/Water Interface</th>
<th>Roads/Paths</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bridges</td>
<td>Congested</td>
<td>Isolated</td>
</tr>
<tr>
<td>Repeat Motif</td>
<td>Prominent</td>
<td>Vegetation</td>
</tr>
<tr>
<td>Man Made</td>
<td>Natural</td>
<td>Geometry</td>
</tr>
<tr>
<td>Towers</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Before we can apply Equation 1 using the USE shown in Table 1, the 22 sites must be coded into fuzzy
sets. We asked two individuals to code all 22 sites against the USE shown in Table 1. The metric for coding was the degree to which each element “characterized” the site. This, of course, is a subjective assessment, but that is part of the reason for using fuzzy sets in the first place.

The rows and columns in Table 1 have no significance at all; they are just a convenient way to display the 13 elements of the universal set of elements. Each site will have varying amounts of these elements.

However, to reduce the impact of the subjectivity, we had identified a designated spot to stand at each site and where the coders will be instructed to focus attention. Figure 2A shows one example of where to stand from the instructions given to the coders. Figure 2B informs the coders where to fix their gaze.

The coding instructions also included driving instructions from known Bay Area landmarks and a description of how each of the elements in Table 1 should be interpreted.

The metric for the evaluation of each of the sites is the degree to which each element characterizes the site. This metric can range from zero (i.e., not there or is totally insignificant) to one (i.e., totally dominant as you view the site). The score is constrained to be in steps of 0.1. That is, the only acceptable values are: 0, 0.1, 0.2, 0.3, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9, 1.0. The reason for this restriction is that over time we realized that the subjective difference between a score of 0.14 and 0.15, for example, was meaningless in this type of evaluation. The element descriptions follow:

1. **Buildings**: These can be of any size and shape.
2. **Land/Water Interface**: We are not concerned as to the type of the water. It could be a creek or a pond. The point is the degree to which water is “edged” in any way with land.
3. **Roads or Paths**: This element could be a major street in a downtown environment or a narrow dirt path in the hills.
4. **Bridges**: This element can be of any size and shape as long as it spans something such as a road, creek, or canyon.
5. **Congested**: By congested we mean full of a number of visually different things locally associated with the site. The element could be embedded in an otherwise isolated environment.
6. **Isolated**: By isolated we mean the site (congested or not) is generally standalone. For example a complex building with lots of structure variations and landscaping, but all by itself in a large open space, would be consider isolated.
7. **Repeat Motif**: Mostly this element means exactly repeated, usually man-made, repeating patterns. Examples include a fence with a series of posts, a series of light fixtures evenly spaced, or seats in a football stadium. Natural repeating features, including rows of evenly spaced trees in a row or carefully manicured fields with rows of things growing, also qualify.
8. **Prominent**: This element should get high marks if there is something that really catches one’s attention at the site and masks almost everything else that is there.
9. **Vegetation**: Although there may be growing vegetation at nearly all locations, sometimes the vege-
tation is characteristic of the site. For example, a park would be mostly vegetation, but so too a big building with fancy landscaping might get high marks. But a large overpass with a few bushes would get relatively low marks.

10. **Man-made**: Many of the sites are man-made but some more than others. Thus, this element is scored with regard to one’s subjective assessment as to how critical the manmadeness is with regard to the overall site.

11. **Natural**: It may be tempting to score this element equivalent to vegetation; however, there are a number of examples that are natural that may not contain much vegetation at all. An example is an interesting rock formation in the desert. In the proposed experiment, this element is a way of assessing the degree to which “natural” is characteristic of the site.

12. **Geometry**: By geometry we generally mean shapes that are not regular and linear. Swoops, curves, and round shapes are what we are thinking of here.

13. **Towers**: Towers differ from buildings. They are generally tall and have a specific purpose.

In developing the USE, two of us (May and Hawley) together coded each site. This exercise turned out to be quite instructive, because we approached the problem from the perspective of making sure that the group of 22 targets grouped into orthogonal categories. The categories were: Gardens, Ponds, Buildings, Towers, and Bridges. We required that each category be as different from all the others as possible, so judging packs could be constructed by choosing one site from each of the five categories.

We merged the two independent coders’ results with our original results. As we had found out in a group coding of our photographic targets, we agreed on most of the assessments (May et al., 2012). We provide black-and-white photographs of all 22 sites together with their consensus coding in the Appendix.

Figure 3A shows a church target taken from the designated location, and Figure 3B shows the coding for this site by the three independent coders.

![Figure 3](image)

*Figure 3. The element numbers correspond to the elements shown above. The black bars correspond to the original coding; the open and grey bars correspond to the independent codings.*

Element 1 is Buildings, and there is complete agreement among the coders that buildings characterize this site. However, the independent coders were in agreement with each other but not with the original coder with regard to how roads characterized the site. The original coder did not feel that Geometry (12) and Towers (13) characterized the site. This is an example of a coding bias by the original team, in that they did not see the more general meaning of the elements as they “forced” their coding to make the Towers category of sites more orthogonal to the Buildings category.

Figure 4 shows the result of a cluster analysis based upon a consensus of all three coders.
Figure 4. The thumbnails indicate the categories Gardens, Ponds, Buildings, Towers, and Bridges. The more similar sites are to one another, the closer they pack toward zero. So the four Building sites, for example are all quite similar to one another, but the group is significantly different from, say, the Bridges sites.

Three categories (Parks, Building, and Towers) each have four sites and the remaining categories each have five. However, the probability of selecting a category from which the intended site will be chosen is .2 regardless of how many sites are in that category.

Site Selection in the Future Experiment

Following the practice we used for selecting target photographs in the past (May et al., 2000), we first randomly generated a category number in the inclusive interval of 1 to 5. Then depending upon which category is selected, we randomly generated a site number either between 1 and 4 inclusively or between 1 and 5 inclusively. Note that three categories, Gardens, Buildings, and Towers, each have only four sites. Decoy sites for judging were chosen from the remaining four categories in a similar way.

Discussion

The method described here to identify a number of natural sites for an anomalous cognition experiment is quite general, although it is also region dependent. That is, desert communities will be characterized by a different kind of site than, say, the Pacific Northwest.

There is, of course, a problem using natural sites—seasonal changes. In winter they look totally different than in summer, and in California ponds that had water in them in the spring are dry in the fall. The best way to attend to this problem is to create season-dependent target sites.

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Society for Psychical Research, 69, 352–359.

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Abstracts in Other Language

Spanish

OBJETIVOS DE COGNICIÓN NATURAL ANÓMALA:
UNA APLICACIÓN DE CONJUNTO DIFUSO (FUZZY SET)

RESUMEN: Se han aplicado previamente conjuntos difusos a estímulos fotográficos. En este trabajo, usamos la misma tecnología para construir un juego de sitios naturales en el área de la bahía de San Francisco. Creamos un Conjunto Universal de Elementos con 13 reactivos con cinco categorías ortogonales: Construcciones, Estanques, Torres, Jardines y Puentes. Dos voluntarios identificaron y codificaron 22 sitios de forma independiente. Por lo tanto, los grupos difusos que representan a estos sitios son un consenso razonable de su contenido. Este artículo describe el proceso de construcción en detalle y el apéndice muestra cada sitio y su conjunto difuso asociado.
CIBLES NATURELLES POUR LA COGNITION ANOMALE :
UNE APPLICATION DES SOUS-ENSEMBLES FLOUS

RESUME : Les sous-ensembles flous ont préalablement été appliqués à des stimuli cibles photographiques. Dans cet article, nous utilisons la même technologie pour construire un ensemble de sites naturels de la baie de San Francisco. Nous avons créé un Lot universel d’éléments contenant 13 items qui s’articulent selon cinq catégories orthogonales : immeubles, étangs, tours, jardins et ponts. 22 sites furent identifiés et indépendamment codés par deux participants. Ainsi, les sous-ensembles flous qui représentent ces sites sont le fruit d’un consensus raisonnable à propos de leur contenu. Cet article décrit en détails le processus de construction, et l’annexe montre chaque site et son sous-ensemble flou associé.

NATÜRLICHE ZIELOBJEKTE ZUR ANOMALEN KOGNITION: EINE FUZZY-MENGEANWENDUNG

Appendix

The 22 Bay Area natural sites and associated consensus coding. Sites 6, 13, and 18 are missing.

**Japanese Garden – 1**

<table>
<thead>
<tr>
<th>ID</th>
<th>Name</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
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AN ANALYSIS OF EXCEPTIONAL EXPERIENCES INVOLVING TELECOMMUNICATION TECHNOLOGY

By Callum E. Cooper

ABSTRACT: This preliminary investigation set out to act as a revised edition of the original study conducted some 30 years beforehand by Rogo and Bayless. Both projects were produced in book form, surveying a variety of strange experiences people claimed to have had with telecommunication technology. These included seemingly impossible phone calls from the living that suggested the caller was in two places at once, and calls from people known to be dead. It was requested in 1979 by Schmeidler that such a study should be written up and presented for peer review, outlining the methods that took place and the exact findings. A sample of spontaneous accounts involving the telephone \((N = 50)\) was analysed using a thematic analysis. The methods and results are presented here to fulfil the requests of several peer reviewers, regarding the two previous books/studies of anomalous telecommunication experiences. Findings suggest that the majority of calls centre around bereavement, which could present arguments for both natural psychological phenomena and/or psi being involved—supporting previous studies of bereavement. Suggestions are made as to the progression of this research and approaches to obtaining as reliable accounts as possible.

Keywords: telephone, survival, death, bereavement, dreams, psychokinesis

Following the completion of the study into anomalous telephone call experiences carried out by Rogo and Bayless (1979), it was requested by Schmeidler that the next step for the researchers should be to publish the study, with its full methodology and findings explained, in a peer-reviewed journal. However, this was never done.

Following an elaboration of the original 1979 study 30 years on (Cooper, 2012a), Matthew Colborn provided a positive review of the study with some important constructive criticism. In discussing the analysis chapter, Colborn (2013, p. 102) states that “Although the analysis is welcome, I must admit that I found it hard to tell sometimes whether Cooper was analysing old cases from Rogo and Bayless’s archives, new ones that he had collected himself, or a mix of both. This chapter needed to be better signposted, and it might have been a good idea to have included a detailed account of the sources, methods used and results as an appendix.” This criticism was also recently echoed by Boccuzzi (2013), with both reviewers sharing the opinion that telephone anomalies are a neglected area of research worthy of further study. Previously, the methods and results of this study have been presented only via conference and guest lectures (Cooper, 2012b). Rogo and Bayless (1979) mentioned that they had used a content analysis but gave very little description of what this involved. Cooper (2012a, 2012b) discussed the use of a content analysis via the qualitative method of thematic analysis. It was these methods—and outcomes—that Colborn (2013) and Boccuzzi (2013) requested further information on, regarding their application, the data used, and the findings.

Taking the suggestions of Colborn (2013) and Boccuzzi (2013) into account, and to honour the request of Schmeidler, this paper addresses the methods used for data collection, the analysis, and the findings, thus finally providing a clear published record of the process that took place in the book/study entitled Telephone Calls From the Dead: A Revised Look at the Phenomenon Thirty Years on (Cooper, 2012a).

No mention of the electronic voice phenomenon (EVP) will be given in this paper, as it is generally considered a sought experience, whereas the telephone calls are spontaneous phenomena that are rarely anticipated and subsequently digitally recorded.

When people report spontaneous exceptional experiences, we could define the event as follows, “[Appearing] as unusual to the person having the experience or in the sense that the processes involved in the experience appear to be ‘non-ordinary’” (Smith, 2010, p. 1). Thus, in such situations, parapsychology is typically dealing with
a variety of phenomena that could involve some form of apparition (e.g., Tyrrell, 1953), death-bed visions with sights and sounds attributed to the dying (e.g., Rogo, 1974), poltergeist activity (e.g., Roll, 1976), and hauntings (e.g., Fontana, 2005, pp. 55–90), to name but a few.

Thirty years ago few parapsychologists had heard of people reporting exceptional experiences regarding the telephone (Rogo & Bayless, 1979, p. 10), specifically incidents of receiving calls purportedly from someone known to be dead, or a strange call from a living person who was later confirmed to be elsewhere when the call was made. Nothing was published specifically on such telephone experiences—in any field of research—from the mid-1980s until 2010 (see Cooper, 2012a, p. 18). And yet, such events have been reported to occur, and more commonly than most people would care to believe.

In 1979, D. Scott Rogo and Raymond Bayless presented what was to be the first publication of a 2-year investigation into the collecting and analysing of accounts of spontaneous anomalous telephonic experiences (Rogo & Bayless, 1979). The data came from a variety of places (worldwide): people wrote to them, some accounts were passed on to them by other parapsychologists (who weren’t sure what to do with them), and some had already been published in related parapsychology periodicals and books. The majority of accounts—where possible—were followed up by extensive interviews, typically carried out by Bayless. This led to a healthy sample for analysis in this preliminary study ($N = 50$), even though it was presented in the form of an academic and yet publicly accessible book rather than a research paper.

A content analysis was carried out on the case collection. This is assumed to be a thematic analysis, though they never stated it as such. They found several types of call characteristics. The majority of the calls were reported to be communication with the dead—as interpreted by the recipient from specific information mentioned by the caller and/or their recognisable voice characteristics. The categories of call types found by Rogo and Bayless (1979) are as follows:

1. **Type 1: Simple Calls**—These are the most commonly reported phone calls from the dead. Typically, the dead caller says only a few words and is unresponsive to any questions asked. At this point the caller may say nothing at all and the line will go dead without any sound of the caller hanging up the phone or being cut off.

2. **Type 2: Prolonged Calls**—These calls last for some time (30 min or so) and involve a conversation like any other telephone call. The recipient does not realise, until after the call, that the caller was in fact dead at the time. Due to the recipient of the call not knowing the caller is dead, this somehow seems to allow the conversation to last longer. Simple calls show that the shock of knowing the caller is dead somehow leaves the conversation short, or the dead caller unresponsive.

3. **Type 3: Answer Calls**—These are cases where a living person makes a call to someone they do not realise is dead (or someone who simply wasn’t home at the time the call was made) and yet they get an answer from what seems to be that very person. These calls are usually prolonged.

A fourth type of call was found in which people would intend to make a call but changed their mind at the last minute. However, the person they intended to call insisted that the call still took place. These were labelled *Intention Calls*. Below is a case that happened to Scott Rogo, during data collection for the original 1979 study by Rogo and Bayless (Rogo, 1986, pp. 116–117):

It was 4 o’clock on a bright Thursday afternoon, and I was lying on my living room couch thinking about making a phone-call to a psychologist I knew at the U.C.L.A Neuropsychiatric Institute. Although I intended to make the call, I never did. About six that evening, though, I got the shock of my life when a call came in from the Institute and from the office of the very psychologist I had thought about calling. The call was from her research assistant saying that he was “answering my message.” When I asked what in blazes he was talking about, he told me at 4 PM a call had come in to them from me. The caller had left my name, and had asked the call be returned!
Data collection was assisted by radio interviews, newspapers coverage, and articles published in specialist magazines/journals advertising the phenomenon (e.g., Rogo, 1977). Other cases were collected from previously published accounts in the historical literature, mainly dealing with psychical research. Percipients who came forward with personal accounts were interviewed where possible as were any other eyewitnesses. The authors specifically encouraged people to tell them about any unusual phone calls they had received or knew about (see Rogo & Bayless, 1979, p. 110). This was done to avoid sampling bias, as they believed that critics might assume that they encouraged people to report “phone calls from the dead” rather than any other form of paranormal call.

Aside from the categories of call characteristics, 10% of the cases occurred on a significant anniversary/emotionally meaningful day, and 22% occurred within 6 months to a year after the death of the caller. Type 1 calls outweighed Type 2 calls by 4:1, and calls from the dead outweighed strange calls from the living (i.e., some cases of Answer and Intention calls) by 8:1 (Rogo & Bayless, 1979, p. 110; Rogo, 1986, p. 118). The overall conclusion was that although many cases have potential conventional explanations, there are some that defy explanation and therefore offer support to the notion of psi and even survival of consciousness beyond death. Bayless (1980) later hypothesised that as technology advances, so too will associated reports of paranormal occurrences.

The research received mixed reviews. Some reviews were highly negative (e.g., Anderson, 1981; Hardy, 1979; Paul, 1982), suggesting that spontaneous cases have no scientific value due to being nothing more than anecdotes and completely explainable via rational means, to which Rogo (1981) replied and defended the research. A couple of reviews made it obvious that the methods and data were not clearly presented by Rogo and Bayless (1979), thus leading to the size of the collection being miscalculated and assumed to be N = 100 (Cox, 1980), or the case collection size was presumed not to have been included at all, among other methodological problems (Hardy, 1979). Other reviews were highly supportive of the research, arguing that the anomalous telephone calls must be taken seriously and the research continued (Clark, 1979; Cox, 1980; Foley, 1996; Tribbe, 1979). After considering the debate between Anderson (1981) and Rogo (1981), Chari (1981) highlighted the point that Phone Calls From the Dead was a preliminary study, and therefore negative criticism should be withheld until the researchers had the chance to “produce evidence of better quality and quantity in future experiments” (p. 227).

One person who gave a peer review of the phenomenon in the work of Rogo and Bayless (1979) was Gertrude Schmeidler, who suggested that the next step for the researchers should be to publish the study, with its full methodology and findings explained, in a peer-reviewed journal. However, this was never done, although further mention of telephone related phenomena was given by both researchers in further publications (McAdams & Bayless, 1981, pp. 129–133; Rogo, 1986, pp. 107–119). But no full discussions of methods and data analysis were provided.

A follow-up study by Biondi (1984) was conducted in Italy regarding anomalous telephone calls. This study took a year to carry out. Data collection was similar to the previous method, in that radio and newspaper articles were used to encourage people to come forward with accounts of “exceptional experiences involving the telephone.” This wording was once again used to avoid a bias in the data collection, due to critics assuming data collection included targeted accounts specifically interpreted as discarnate communication—which was not the case. Around 40 accounts were gathered. These accounts were then investigated by interviewing percipients and checking information with telephone companies. This was done to clarify the accounts and to rule out conventional explanations for the events as far as possible. This subsequently led to a smaller sample of accounts which presented few—if any—normal explanations for their occurrence (N = 15). The study found that the Italian sample of anomalous call cases could be fit into the categories of call types generated by Rogo and Bayless (1979) from their content analysis. Yet, as Rogo (1981) argued, these patterns do not suggest paranormality, but they at least demonstrate some form of internal consistency and suggest how such events occur. Biondi concluded that it is doubtful that such experiences present evidence for survival, as many conventional explanations could account for the calls. However, this still leaves room for the possibility of some form of psi process being present.

Biondi (1984) further noted that the majority of the calls he collected fell into the category of impossible calls from living people (i.e., Answer Calls and Intention Calls), rather than cases of communication with the dead. It was concluded that the evidence is too limited to verify that these calls are what they appear to be, owing to the numerous conventional explanations that could apply (e.g., misinterpretation, fraud, hypnagogic and hypnopompic states, electrical faults). However, Biondi still felt such phenomena were worthy of further study, in order to attempt...
to identify the nature and origin of the calls. Just over a decade later, Biondi (1996) again encouraged a serious
examination of telephone anomalies, but he doubted that further research would be carried out in the near future.
This was for several reasons: (a) even though the study was advertised to thousands of radio listeners on mainstream
stations no more than 50 respondents were recruited; (b) the study took time and effort, taking a year to collect the
data and do follow-up interviews; and (c) the study required more than one researcher to gather more accounts,
absorb the financial commitment, and produce serious and meaningful results.

**Benefits of the Research**

Telephonic anomalies are not often discussed in parapsychology and are relatively novel compared to other
spontaneous experiences such as apparitions (see Cooper, 2011; Gurney, Myers, & Podmore, 1886; Tyrrell, 1953).
Nonetheless, the study by Rogo and Bayless (1979) presented some useful outcomes. At the very least, it led people
who had experienced similar phenomena to realise that they were not delusional or emotionally disturbed, as Krippner (2006) pointed out. Even several psychologists and scientists contacted the authors and admitted they too
had experienced similar events (see Rogo & Bayless, 1979, pp. 11–17). If the research had never been made public,
those people may never have come forward and presented their accounts, typically due to fearing ridicule.

Since the publication by Rees (1971) regarding “hallucinations of widowhood,” more and more people
have felt it increasingly acceptable to discuss exceptional experiences they may have had during bereavement (or indeed at any time in their life). Rees (1971) found that the vast majority of his participants believed a post-death
experience to be comforting and beneficial to them, due to their establishing a continued bond with the deceased.
These experiences can even occur several years after suffering a loss and the grief having passed (Rees, 2000). Such
findings are consistent with telephonic anomalies, as people have reported these experiences occurring anywhere
from between 24 hours to some 40 years after the loss of a close friend or loved one (Rogo & Bayless, 1979, p. 54).

Wright (1998, 2002, 2006) presented many examples of psychokinetic effects being reported during the
bereavement stage. They include all kinds of electrical disturbances, especially with lights, radios, and telephones
forming a system of alleged communications with the bereaved. These events allow a process of healing to take
place for the bereaved, as well as provide therapeutic benefit from establishing a continued bond with the deceased
through such exceptional experiences (e.g., Cooper, 2013a; Drewry, 2003; Haraldsson, 2012; Krippner, 2006; Park-
er, 2004).

For these reasons—and more—anomalous telephonic experiences are worth researching. They are a re-
latively new form of anomaly for parapsychology to consider, and for many individuals such experiences can be
highly beneficial in terms of a positive psychology, understanding bereavement, and understanding of personal life
transitions caused by exceptional experiences (e.g., Cooper, 2013b; Evenden, Cooper, & Mitchell, 2013; Kennedy
& Kanthamani, 1995).

These issues, and the positive psychological effects of spontaneous experiences for peoples’ lives, reinforce
the importance of spontaneous case investigation within parapsychology, as argued by Alvarado (1997). By investigat-
ing anomalous telephone experiences (and other relevant psi phenomena) we develop a greater understanding
of the characteristics of such experiences and any personal transformations that may take place. Some parapsychol-
ogists have become keen to embrace these psi events and attempted to experience them for themselves, adopting an
alternative subjective or “first-person” approach (Luke, 2011). Additionally, spontaneous cases allow us to continue
the pioneering work of the Societies for Psychical Research and that of Louisa Rhine at Duke University (Irwin
& Watt, 2007, pp. 32–37), acting as a useful alternative methodological approach to laboratory studies and giving
more attention to the potential value of psi in the real world.

**The New Research**

Since previous publication of the initial studies and research into exceptional telephone call experiences
(Cooper, 2010a, 2010b), a complete study was carried out and published to serve as a revision of the work of Rogo
and Bayless (Cooper, 2012a). Although this second study is also in book form, the aim was to act on the feedback of
the first study into telephonic experiences and make the book not only publicly accessible, but also to be scientific,
informative, and provide an outline of the study methods and findings step by step.

This new research (Cooper, 2012a), much like the original (Rogo & Bayless, 1979), received positive reviews from various scholars in relevant academic journals (Haraldsson, 2013; Martinez, 2012; Müller, 2013; Sewell, 2012; Taylor, 2012; Tilley, 2012). Gilbert (2013), in addition to making some mild criticisms of the research, desired a comparison of digital technology to analogue technology. However, it had already been explained that although cases of digital technology had been collected and previously discussed, there simply weren’t enough cases involving mobile phones, text messages, and e-mails to allow a meaningful analysis. (To read further about this debate, see Cooper, 2014a, followed by Gilbert, 2014, and then Cooper, 2014b). Therefore, the sample that was analysed consisted purely of exceptional experiences regarding the telephone, including a mix of both digital and analogue cases (i.e., mobile phone and landline). This was then followed up with a paper exploring the psychological and parapsychological interpretations of anomalous telephonic communications (Cooper & Foley, 2012).

Method

**Receivers of the Telephone Calls**

A number of methods were used to acquire percipient accounts, which were then individually considered for inclusion. The primary method could be considered as “snowball sampling,” the collection of data from a group of people who believed that they had had spontaneous anomalous communications via the telephone. Some had heard about the author’s previous work (e.g., Cooper, 2010a, 2010b), whereas others had heard about the research through radio and newspaper discussion. It was made clear by the researcher that any form of paranormal experience involving the telephone was sought—so as not to produce a bias of interest towards alleged paranormal calls from the dead. Following this, people made contact with the researcher to discuss and submit their accounts.

It should be noted regarding the radio and newspaper advertisements to the general public that a similar issue was encountered to that reported by Biondi (1984, 1996) regarding his data collection. Because the researcher reached out to such a large audience (especially through popular radio stations), a large number of people immediately came forward wanting to share their accounts. However, on average, for every dozen people that came forward, only one seemed to be prepared to follow up their claim by working with the researcher to construct an eyewitness account of the event, or be interviewed about their experiences. This in itself says a lot about the validity of reported paranormal phenomena that are just generally-accepted anecdotes that sometimes feature in popular books but are not necessarily authenticated accounts. If participants are not willing to follow up their claims, they should be avoided at all costs for inclusion and politely declined.

Other accounts were gathered from Rogo’s archived files, which are housed at the California Institute of Integral Studies library. The accounts include percipient statements and follow-up interviews carried out by either Rogo or Bayless, depending who was presented the account first. Therefore, the analysis involved a mixture of Rogo’s and Bayless’s unpublished and researched accounts (from 1979–1982) and new accounts collected between 2010 and 2012 (roughly 20% old accounts and 80% new accounts). Mixing accounts to increase sample size (and statistical power) has been advocated by various spontaneous case investigators. For example, MacKenzie (1979) argued that second-hand data can help support first-hand data in helping us understand experiences of anomalous phenomena. This suggestion was also supported by Rogo (1981), which he called the “bundle of sticks approach”: many cases become stronger than a single case, and in a collection of cases, the stronger ones help balance out the weaker ones.

Accounts of spontaneous telephone calls in this present paper, which is a summarised and formal version of their first presentation (Cooper, 2012a), were submitted from Great Britain, the USA, Canada, Italy, France, Germany, and Spain, although the majority were from Great Britain and the USA. All cases were followed up as far as possible in terms of available information, with interviews and long-term correspondence. In total, 70 cases were collected and submitted. After following the cases up—much like in the Biondi (1984) study—50 accounts were considered robust enough for inclusion.

The cases that provided adequate content for inclusion involved a detailed description of the call and what took place, some background information, and sometimes multiple witnesses. Cases that weren’t included generally did not involve a telephone conversation. They may have been short and symbolic to the percipient, such as
“someone died and the phone rang just the once a week later.” This is still an interesting interpretation of events for parapsychologists investigating bereavement experiences, but not strong enough to meet the criteria for this study.

**Procedure**

Spontaneous case reports are qualitatively based, as the account is written by the percipient or typically an interview is conducted with the percipient by the researcher. This is no less valid than the quantitative approach: the qualitative documentation of human experiences—exceptional or otherwise—allows us to see the social issues involved on an objective level, from a first-person perspective (Kirk & Miller, 1986). In other words, we are provided with more naturalistic data and ecological validity and can investigate to what extent a certain feature is (or is not) present.

As soon as all percipient accounts had been gathered/constructed or transcribed from interview, coding of the cases was carried out using thematic analysis and a phenomenological approach. Creswell (1998) outlines five traditions of qualitative research, describing the phenomenological approach as allowing the researcher to become familiar with a personal experience of a particular phenomenon. Then the account/transcript is coded on elements of the experience that took place, recurring themes, and significant statements (horizontalization of the data); the statements are treated as equally worthy. This form of analysis is useful for investigating experiences and events that we are not generally familiar with, to explore the nature of their occurrence and common themes. This analysis was based on previously-employed methods, which can be seen in Louisa Rhine’s research (e.g., Rhine, 1957, 1963, 1978) on her spontaneous case collection primarily through the 1950s and 1960s (for a brief history, see Irwin & Watt, 2007, pp. 32–37) and subsequent related publications (e.g., Hanefeld, 1968).

Coyle (2010) argues that although a qualitative approach to parapsychological data might not provide any further validity to the truth of the claims; however, such an approach does allow us to examine how people make sense of such experiences and the additional features—humanistic or environmental—that may have contributed to the experienced phenomena. This qualitative aspect allows the researcher more freedom to approach, investigate, and interpret, spontaneous events from a variety of methodological standpoints (theoretical, historical, phenomenological, discourse/conversation analysis, etc.).

The content analysis involved reading through all of the accounts several times and becoming familiar with the types of experiences reported. Following this, the reports were read one by one with themes such as “did the percipient report hearing static on the line” or “was the voice hollow in tone or sound far away” being noted. Other themes that were explored were call duration and whether the percipient was or wasn’t aware that the caller was dead at the time of the telephone conversation. During this process, once an account was read and features of the experience were noted, the account was placed in a separate pile with other accounts containing similar themes. One pile was reserved for cases that did not seem to fit any pattern. The classification of call types was re-examined once all of the accounts had been placed into relevant piles. Additional significant features of the phone calls were identified, which provided quantitative information.

**Results**

The analysis demonstrated consistent themes regarding how the anomalous telephone calls were experienced, and related to the call types previously discovered in the American study (Rogo & Bayless, 1979) and Italian study (Biondi, 1984). However, there were some exceptions. For example, there were a few call types that displayed elements of both Type 1 and Type 2 calls: The percipient could be aware of the caller being dead and yet claim to have had repeated calls and extended conversations. This led to the development of a fourth call category involving the dead that was labelled Mixed Calls, thus moving Intention Calls up in the ranking due to no discarnate contact (Type 5).

The telephone call characteristics and frequency remain fairly consistent with the original findings (Rogo & Bayless, 1979). For example, the original study found that Type 1 calls outweigh Type 2 calls by four to one, the new analysis found this to be around three to one. The original study also found that calls perceived to be contact from, or to the dead, outweigh calls from the living by eight to one, the new study found this to be seven to one.
Regarding characteristics of the calls, in 34% of cases reported there were audible anomalies such as static on the line or the caller sounding, faint, hollow, or distant in tone. Around 8% of calls occurred on significant anniversaries for the percipient, whilst the original study found this to be 10%. And 6% of calls appeared to display features of the caller giving a message of warning or emergency to help the receiver of the call avoid some form of impending danger. The original study also found cases of these “precognitive warning calls,” which were also low in number ($n = 2$; Rogo & Bayless, 1979, pp. 106–107).

![Figure 1. Frequency of call types from the current analysis.](image)

In Figure 1 the frequency of call types within the current sample is displayed ($N = 50$). Type 1 calls appeared the most common ($n = 14, 28\%$) then Type 2 ($n = 5, 10\%$). Type 3 were the least common ($n = 1, 2\%$), and Type 4 and Type 5 produced an equal number of accounts (both $n = 6, 12\%$). Besides the exception of the newly labelled Type 4 calls, there were accounts that simply didn’t fit the pattern of the reported telephone conversations but still involved elements of perceived paranormal telephone communication. Purely because they were grouped together as miscellaneous accounts, they are responsible for a large part of the sample ($n = 18, 36\%$).

Below are brief descriptions of the additional cases listed in Figure 1 as “Other.” Detailed accounts are presented in the original work (Cooper, 2012a, pp. 90–115).

**Haunted Telephones**

Eight percent ($n = 4$) of cases contained a haunting element. These typically involved a haunted location—which was not necessarily lived in—having a telephone which from time to time people would hear ringing. Upon answering the phone, strange voices would be heard, messages, static, and so forth.

**Dream Accounts**

Six percent ($n = 3$) of cases contained dream elements. These were percipients who submitted accounts of having dreamt a paranormal telephone call, and in all cases the account was described as the dead relaying messages.
Though not involving actual physical experiences, these cases are nevertheless interesting to consider, and the phone calls were typical of the ones reported in the waking state.

It is noteworthy that Barrett (1991–1992) analysed the content of dreams within a group of bereaved individuals and found that the most common dream scenario reported was that of communicating with the deceased via the telephone.

**Coincidence Calls?**

Sixteen percent \((n = 8)\) of cases contained questionable coincidental elements as *potential* conventional explanations for their occurrences. Three typical things would happen:

1. Someone would lose someone close and then immediately following this they would experience a long period of telephone calls day and night, whereupon answering they just heard static and faint voices.
2. Someone would lose a child through death and would receive a call from a childlike voice saying either “Mum” or “Dad” (depending on who answered the phone), and then the voice would fade out.
3. Someone would report burying a friend or loved one with personal items in the coffin, including the deceased’s mobile phone. Following the burial, the living received a (missed) call from the buried phone which resided with the corpse. The subsample also included cases of text messages allegedly being sent from the grave.

**Voicemail Messages**

Six percent \((n = 3)\) of cases contained voicemail message accounts. Rogo and Bayless (1979, p. 138) received two such accounts, but one was instantly discovered to be fraudulent, so the remaining case was not included in their analysis. Several of these cases have been reported in the *ITC Journal*.

**Discussion**

The findings of this study appear to closely match the findings of the original study conducted some 30 years ago (Rogo & Bayless, 1979), in spite of incredible advances in telecommunication technology since the time of the original study. This merely supports the hypothesis set forth by Bayless (1980) that as technology advances, so too will reports of paranormal electronic communication. The results demonstrate that anomalous telecommunication events are being widely experienced and reported, and therefore we can assume them to be a cross-cultural phenomenon.

Although the psychological and parapsychological considerations pertaining to the interpretation of the telephone calls have already been presented (Cooper, 2012a), let us once again consider some of those key issues here, regarding the research, its weaknesses, and next steps.

**Psychological Considerations, Misinterpretation, and Fraud**

Anderson (1981) argued that patterns within a phenomenon do not suggest that any element of psi is present. This is true; however, no one ever suggested it is in any of the original work (Rogo, 1981). This study has found a consistent pattern of call types, and even some additional experiences that percipients felt relevant to submit (i.e., dreams, hauntings, possible coincidences, and voicemail message calls). Among the five types, Type 1 calls seem to be the most commonly experienced. It is possible that conventional explanations exist for these calls if the percipient was home alone when they took place. The percipient is typically in a state of bereavement and perhaps requires some form of contact to establish a continued bond with the dead. In such cases, the telephone call could be a relatively similar phenomenon to the hallucinatory experiences of widowhood discussed by Rees (1971). These experiences are perfectly natural, common, and potentially (but not definitely) the products of a grieving mind. Baker (1992) considered such experiences to be a psychological projection of grief. He believed the bereaved would
experience “selective amnesia” when a regular telephone call to the house came through; thus, upon answering, the bereaved assumed the caller to be whomever they desired (i.e., the deceased).

If the percipient was home alone in this state, and the telephone company had no record of the call taking place, without any form of recording of the event, we are stuck in a catch-22 situation. Either the call was a psychological product of grief, or the call was genuine. Separating one from the other is not so easily done, and we cannot just assume that because the percipient was in a state of grief that the experience was entirely psychological, nor can we say it was paranormal. In some cases of Type 2-5 calls, and rarely in Type 1, there were multiple witnesses to the calls, and yet, the phone company is found to have no record of the call taking place. Multiple witnesses add more weight to the evidentiality of the calls, much as in collective cases of apparitions (i.e., multiple witnesses; Gurney & Myers, 1898; Hallson, 2014; Tyrrell, 1953).

For people who have lost a child, receiving a potential “wrong number” from a young voice that simply says “Mum” or “Dad” leaves great potential for misinterpretation, which researchers must consider. Young callers might realise their mistakes and instead of apologising, they simply hang up, thus explaining the brief fleeting voice on the line.

And finally, fraud is something which must be considered. It is a possibility that someone might wish to play a prank on the bereaved, or pursue a personal vendetta for whatever reason, and creating such a prank call around the time of bereavement could be the explanation for the call. (It is wise to tentatively ask percipients if they have had any recent arguments or are generally on good terms with people they encounter in their day to day lives). As the late John Randall wrote to me some time ago regarding exceptional experiences involving telephones, “There is also the possibility of deliberate hoaxes although, as you say in your paper, that would be very cruel. Unfortunately some people are naturally cruel” (John Randall, personal communication, January 27, 2011).

**Dream Experiences**

Other issues to consider are whether the person experienced the telephone call whilst in a sleeping state, or more specifically, while in an altered state of consciousness? In both the original and current study, someone claiming they awoke to a telephone call was rarely reported, but such cases were still noted. However, it is worth considering that if such instances are experienced, hypnagogic (falling to sleep) and hypnopompic (waking up) states of sleep could account for certain experiences and their interpretation (Klemperer, 1992), especially during a period of grief. In the present study, several cases which were not included in the analysis (from the original 70 cases collected) involved instances of a percipient suffering a bereavement and claiming they awoke to the sound of their mobile phone ringing. On checking the phone, it hadn’t actually rung at all. Even so, the percipient still interpreted the experience as attempted contact from the dead and the dead letting the bereaved know that everything was “OK.” However, in this study, such instances where the percipient was in a hypnagogic or hypnopompic state leans conclusions towards a psychological interpretation of the events, given that only a ringing was reported and assumed to be the deceased trying to call. This kind of data is useful for research on post-death phenomena and the adaptive grief outcomes of such experiences. For example, there are accounts published in the literature where the bereaved have reported telephones to ring at night and other electrical appliances to act out of the ordinary during the time of recent loss. This kind of phenomenon has been investigated by Wright (1998, 2002, 2006). However, as mentioned previously in this paper, such cases do not meet the stringent criteria for inclusion in this present study.

From the accounts that were analysed, only 6% \((n = 3)\) of cases involved percipients claiming they dreamt of a telephone call from the dead (or were daydreaming in one instance). In some cases of percipients stating they were in bed at the time of the event, it is not sufficient for one to assume that in every instance they must have dreamt the experience, as some critiques have casually concluded in the past (Anderson, 1981), nor does it invalidate the case. If the event can be recalled—whether dreamt or not—then meaningful information relayed by the dead (previously unknown to the percipient) could be verified—adding weight to the potential presence of psi processes involved. Such instances are no less important than Type 1 calls where the percipient may have been alone at the time of the call, and all too often critics have shrugged the experience off as a purely hallucinatory reaction of the grieving mind. This is not a sensible assumption to make. Not only do we understand the vast majority of such experiences to be therapeutic for the bereaved (although potentially damaging, if their interpretation was explained to them as something conventional or just psychological), but each case deserves careful consideration of details, circumstances, communication,
and other available information, whether experienced in the waking state or not. In keeping with Creswell (1998), each case was (and should be) treated with equal worth and respect to the percipient.

Parapsychological Considerations

Rogo and Bayless (1979) reported that around 50% of their cases could not be traced, and those that could appeared to be placed through a long-distance call. In this follow-up study, incidents of checking with the phone companies have been noted, but the exact figures of how many cases have been verified by phone companies has not yet been calculated. The main aim of this study was to follow up any developments of anomalous phone calls within the 30-year gap of absent reports and present the preliminary findings. Collective cases were also noted within the various call types, often reported as Type 1 or 2 calls. As the data collection is still ongoing, the exact figures will be reported at a later date, but at present no more than 20% of the cases appear to have multiple witnesses to the call taking place. Even rarer (accounting for no more than 4% of the cases) are instances such as the following case from an interview transcript, where the percipients passed around a handset so at least two people could speak to the alleged deceased caller (later discovering after the call that the caller had died 2 weeks previously):

Cooper: Were you actually having a two-way conversation with her on the phone then? Can you recall some dialog? Do you believe some of the things she was saying were out of the ordinary?
Sandy: Well yes, Aunty Lil doesn’t usually ring up on your birthday, normally she’d send you a card or something, but she rang us both up and we were talking on the phone, both of us, because my Mum was living across the road. So, we were having this conversation and it was just sort of things she was saying which didn’t sort of register with her really.
Cooper: Did you both share the phone? [Sandy and her Mother]
Sandy: Yes, we were talking in one conversation you see; she was talking to my Mum and then I was talking after.
[Following the call, Sandy called Aunty Lil’s daughter]:
Sandy: I felt so bad when I phoned the family you see, and they said “well two weeks ago she died” and I thought “Oh my God,” you know, “I’m sorry…”

With conventional explanations considered, now let us focus on the possibility of psi being involved in anomalous telephone calls, or even potential survival evidence. When we look at a telephone call we have two physical events taking place and therefore two objective elements involved: (a) the ringing of the phone and (b) the voices heard. If these calls were anything other than Type 1, or there were multiple witnesses, we are left with an increasingly difficult task in finding conventional explanations for the calls. If the “dead caller” relays information that would only have been known to the deceased, then we have the suggestion of the presence of psi and/or survival being involved. However, as John Palmer noted, “the demonstration of psi says nothing directly about the source of the psi—in particular whether it was a discarnate entity” (Cooper, 2012a, p. 171). To reach the stage of considering whether we are left with accounts presenting evidence for psi, or indeed survival, we need to develop a method of separating the calls that may be paranormal from all of the other possibilities, as Randall suggested (Cooper, 2012a, p. 167). We are left with two possibilities:

1. The call was created via a psychokinetic (PK) effect by someone alive making the telephone ring, in order for the percipient to be alerted to answer it. And then the caller’s voice was either hallucinated or a further product of ESP and/or PK by someone living somehow acting on the telephone itself (which would explain why some calls were not registered with the phone company as having been made).
2. Whatever form consciousness may take beyond bodily death—if indeed it does continue—it is responsible for physically manipulating the telephone to (a) ring and (b) produce a recognisable voice.

However, even a thorough investigation would not lead to a solid conclusion as to the source of the psi, especially for an event that has passed that we cannot observe and have control over in present time. The issue of
separating psi from survival has been noted for a long time, and Roll (1980) noted this issue well in what he called the “catch-22 of survival.” We can only prove that the calls were created by the deceased if we can disprove that some form of psi from the living created the call. In follow-up investigations of spontaneous phenomena, especially with telephone calls, at best we are left only with evidence to suggest that some form of psi process was involved. Our best option, at present, is to ensure that the spontaneous case collections we continue to gather are not accepted as pure anecdotes and that every case is researched to its full capacity, with additional eyewitnesses, following up leads and alternative possibilities. It is the duty of any serious investigator to not leave any stone unturned before allowing such cases to be included in a file of collected cases for analysis.

Conclusion

In presenting this preliminary study into anomalous telephone calls and the scientific methods applied, the request of Schmeidler has now been answered and fulfilled. Now that three initial studies have been carried out into what anomalous telephone calls are, and how they are experienced (Rogo & Bayless, 1979; Biondi, 1984; Cooper, 2012a), there is still great potential for taking the research forward. One further step could be to take the existing samples of accounts—or indeed new accounts—and target particular cases that show potential for psi (owing to limited conventional explanations for their occurrence) and approach them one by one as case studies. By doing this, complete attention can be given to a single case, following all possible avenues of alternative explanation. If conventional explanations can be ruled out, then we are left with few options but to accept the presence of psi at the very least—and survival at the very most. This will be costly, time consuming, and require dedication, as Biondi (1996) pointed out, but it is a perfectly acceptable next step in the research. It is also a further step in expanding the available research on exceptional experiences involving the telephone, which is a seldom-considered phenomenon.

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Abstracts in Other Languages

Spanish

UN ANÁLISIS DE EXPERIENCIAS EXCEPCIONALES CON TECNOLOGÍA DE TELECOMUNICACIONES

RESUMEN: Esta investigación preliminar es como una edición revisada del estudio realizado hace 30 años por Rogo y Bayless. Ambos proyectos vieron la luz en forma de libro, como encuestas de una variedad de experiencias extrañas que algunas personas afirmaron haber tenido con la tecnología de telecomunicaciones. Se incluyeron llamadas telefónicas aparentemente imposibles por personas vivas que sugerían la persona que llamaba se encontraba en dos lugares a la vez y llamadas de personas muertas. En 1979 Schmeidler pidió que dicho estudio fuera escrito y mandado a revisión por otros pares, esbozando los métodos que se habían utilizado y las conclusiones exactas. Usé una muestra de relatos espontáneos en el teléfono (N = 50), utilizando un análisis temático. Los métodos y los resultados se presentan aquí para cumplir con las peticiones de varios revisores en relación a los dos libros/estudios previos de experiencias anómalas en telecomunicaciones. Los resultados sugieren que la mayoría de las llamadas tienen que ver con el duelo, lo que presenta argumentos tanto para fenómenos psicológicos naturales como para psi, en apoyo a estudios previos de duelo. Hago sugerencias para el progreso de esta investigación y la obtención de cuentas lo más fiable posibles.

French

UNE ANALYSE DES EXPERIENCES EXCEPTIONNELLES IMPLIQUANT UNE TECHNOLOGIE DE COMMUNICATION

RESUME : Cette investigation préliminaire se veut une version révisée de l’étude originale conduite 30 ans plus tôt par Rogo et Bayless. Les deux projets furent publiés sous la forme d’un livre analysant diverses expériences étrang-
es impliquant des technologies de télécommunication dont les gens avaient pu témoignées. Parmi elles, on trouve des appels téléphoniques impossibles par des personnes vivantes, venant suggérer que l’appelant se trouvait à deux endroits en même temps, et les appels de personnes que l’on sait décédées. Schmeidler avait recommandé, en 1979, qu’une telle étude soit soumise à un comité de lecture, soulignant les méthodes employées et les résultats exacts. Un échantillon de tels témoignages impliquant un téléphone (N = 50) a été analysé en utilisant une analyse thématique. Les méthodes et les résultats sont présentés ici pour s’accorder avec les requêtes des nombreux pairs ayant eu connaissance des deux premiers livres/études des expériences anormales associées aux télécommunications. Les résultats suggèrent que la majorité des appels se concentrent autour d’une thématique de deuil, ce qui peut fournir des arguments tant pour un phénomène psychologique naturel que pour un processus psi, ou les deux, ce qui vient renforcer des études antérieures sur le deuil. Des suggestions sont faites pour que cette recherche progresse et sur les manières d’obtenir des témoignages aussi fiables que possible.

German

EINE ANALYSE AUSSERGEWÖHNLICHER ERFÄHRUNGEN
UNTER VERWENDUNG DER TELEKOMMUNIKATIONSTECHNOLOGIE

ABSTRACT: Stanford’s psi-mediated instrumental response (PMIR) model proposes that psi is an evolutionarily adaptive function that largely works in the service of the organism but which operates at an almost completely unconscious level. A series of successful experiments conducted by Luke and associates have explored the PMIR model with an automated nonintentional precognition task with postexperimental outcome-contingent tasks that vary in pleasantness commensurate with psi task success. Until now this test paradigm explored only nonintentional tasks so this study compares nonintentional with intentional psi task conditions to explore the unconscious psi proposition of the PMIR model. A sample of 40 psychology student participants completed 10 trials each of the automated precognition task, with 20 participants randomly allocated to the nonintentional condition and 20 to the intentional condition in an independent groups design. Contrary to previous findings psi scoring overall was below mean chance expectation (MCE), although nonsignificant. In line with predictions based on the PMIR model, however, task participants in the nonintentional condition scored above MCE and scored higher than those in the intentional condition, though these differences were not significant. Measures of belief in psi, openness to experience, and emotional creativity were found not to correlate with psi scores. The findings are discussed in light of previous studies with suggestions for future research.

Keywords: precognition, PMIR, intention, decline effect

Spontaneous psi experiences tend to be rather fleeting and rare, which would seem at odds with attempts to induce psi experimentally. However, a number of successful psi research paradigms have recently explored nonintentional and unconscious tests of precognition utilising time-reversed psychological (e.g., Bem, 2011) and psychophysiological (e.g., Mossbridge, Tressoldi, & Utts, 2012) measures, indicating that psi may be a largely unconscious process. Such successful test paradigms fit rather well with Stanford’s (1974) psi-mediated instrumental response (PMIR) model, which proposes that psi is an evolutionarily adaptive function that largely works in the service of the organism’s needs but which operates at an almost completely unconscious level.

Testing Stanford’s model Luke, Delanoy, and Sherwood (2008) developed an automated nonintentional precognition task to explore the supposedly unconscious psi processes proposed by Stanford (1974). Furthermore, to fulfil and test the supposedly need-serving and evolutionarily adaptive nature of psi within a PMIR model, the protocol utilised postexperimental outcome contingent tasks that varied in pleasantness commensurate with psi task success. The initial study used erotic images as the pleasant “reward” task for positive psi scoring, and a boring task alternative as a deterrent for below chance psi scoring and found a significant psi effect overall (Luke, Delanoy, & Sherwood, 2008). Later studies utilised this same protocol but exchanged the erotic images for apparently funny cartoons taken from Gary Larsen’s Far Side series (Hitchman, Roe, & Sherwood, 2012a; Luke & Morin, 2014; Luke, Roe, & Davison, 2008) or images predetermined to be pleasant (Hitchman, Roe, & Sherwood, 2012b). Of the six experiments (two studies are reported by Luke, Roe, & Davison, 2008) the first three were independently significant, and all six studies combined, with a total sample size of 298 participants, found above chance psi scores overall, giving a combined Stouffer Z = 4.13 (p = .00004).

An assessment of the success of this paradigm for testing Stanford’s (1974) PMIR model thus far indicates that having an unconscious contingent reward/punishment for successful demonstration of nonintentional psi appears to have some utility, insofar as psi has been demonstrated overall and that psi scores have tended to correlate with subjective task pleasantness regardless of whether there is a contingent task or not. Furthermore, psi scores also correlated significantly with the participants’ erotic reactivity in the first experiment, in which erotic images were used as the pleasant contingent task (Luke, Delanoy & Sherwood, 2008), indicating that participant desires
were rewarded for their unconscious psi task performance. What remains to be explored further regarding the PMIR model is the salience of unconscious psi tasks, and so the same paradigm begs exploration in both a covert and an overt manner. In the revised version of the PMIR model, Stanford (1990, p. 94) gave priority to unconscious psi, and perhaps hence nonintentional psi processes, by indicating that the “... adaptive response to implicit knowledge is extremely important . . . because it provides an economical base for adaptive response of the organism,” thereby implying “... that PMIR occurs in ways that make minimal demand on the individual’s processing capacities.” By these means conscious awareness of psi information “... might in many circumstances be disruptive, nonproductive, and even maladaptive if a more economical form of response [i.e., one driven unconsciously/non-intentionally] would be fully adaptive” (Stanford, 1990, p. 94). In this regard it would be reasonable to hypothesise that nonintentional psi would be superior to intentional psi, all else being equal.

There are also individual differences to be investigated. The initial five experiments explored the possibility that unconscious psi phenomena in the service of the organism might be considered euphemistically as lucky events, and so beliefs about luck and self-perceived luckiness were explored with the Questionnaire of Beliefs About Luck (QBL). However, despite some initial success none of the five QBL subscales consistently correlated with psi scores. A number of other individual difference measures were also explored as covariates of psi scores, with belief in psi (the “sheep-goat” measure) providing significant positive correlations in two of the three studies in which it was explored, and openness to experience—as a proxy measure for latent inhibition—produced significant positive correlations in two of the four studies in which it was examined. However, other purported measures of latent inhibition, such as the Creative Cognition Inventory (two studies—Hitchman et al., 2012a; Luke, Roe, & Davison, 2008) and an auditory discrimination task (one study—Hitchman et al., 2012b) failed to correlate significantly, calling into question the utility of latent inhibition in regards to this paradigm.

Extending the research of the PMIR model using this automated paradigm, the current study aims to “destruction test” (whereby the parameters at which the effects disappear are systematically tested one at a time) the protocol in line with PMIR predictions and explore the salience of the nonintentional dimension of the psi task by having half the participants perform the task intentionally and the other half, as previously, unintentionally. Furthermore, the individual difference measures of openness to experience and belief in psi were further explored, along with a novel measure of creativity that may tap into latent inhibition, termed emotional creativity.

**Formal Hypotheses**

Hypotheses One: Participants will score significantly above mean chance expectation (MCE) in the precognition task overall.

Hypothesis Two: The number of mean hits in the precognition task will be greater for the nonintentional condition than the intentional condition.

**Exploratory Hypotheses**

Hypothesis Three: Scores on the precognition task will be correlated with scores on measures of openness to experience, belief in psi (sheep-goat), and emotional creativity.

**Method**

**Participants**

Psychology students were recruited to the study as part of their degree course research participation scheme and invited to take part via a standard e-mail sent to all first year psychology students and posters (with the same information) in the Psychology Department. Participants were invited to take part in an experimental study exploring precognition in relation to personality factors. Of the 40 participants, 20 were randomly assigned to the intentional condition and 20 to the nonintentional condition. As an incentive for taking part in the study, participants received 30 min worth of research participation points, which is a standard practice in studies utilising the research participa-
tion scheme. Students participating in the research scheme get to use the same scheme to recruit participants in their third-year projects as an incentive, and most participating students complete their required 10 hours of participation. The number of participants was prespecified. No data from participants with complete responses were rejected, and no participants returned incomplete data.

Materials

**PMIR Visual Basic program.** A software program in Visual Basic (v. 6) was written specifically for this series of experiments by the first author and was identical to one used in the second study reported by Luke, Roe, and Davison (2008). The program comes with its own step-by-step instructions for the participants and consists of a fully automated, nonintentional precognition task with 10 trials, with reward/penalty tasks contingent on psi scores (see Procedure for details). The program has a pool of 40 fractal images as the decoy and target images for the forced-choice psi task. No images were repeated in any run.

All 40 images for this program were selected previously via a standardisation procedure from a pool of 72 such images, which had themselves been created randomly using the freeware fractal generator program Fractalus v. 4.02. Images had been presented to five independent judges via a presentation program written in Visual Basic and standardised using a similar rating process to that used in the creation of the International Affective Picture System (IAPS: Lang & Greenwald, 1993). Images had then been grouped together into the 10 best pools of four images each, based upon the homogeneity of their individual scores on scales of pleasantness and arousal (Luke, 2007).

**Belief in psi (sheep-goat) questionnaire.** This short questionnaire presents four items corresponding to the various criteria of the sheep-goat variable of belief in psi (Palmer, 1972)

**Openness to experience questionnaire.** A 20-item positively/negatively balanced measure with a 5-point Likert scale for each item was derived from the international personality item pool (IPIP; Goldberg et al., 2006) and designed to measure the openness to experience personality trait. The scale has satisfactory internal consistency, factor structure, and external validity (Buchanan, Johnson, & Goldberg, 2005). The questionnaire assesses an individual’s openness to experience pertaining to imagination, emotionality, adventurousness, intellect, and liberalism through statements such as “I believe in the importance of art,” “I enjoy wild flights of fantasy,” and “I have a vivid imagination.”

**Emotional Creativity Inventory.** This 30-item self-report questionnaire (Averill, 1999) measures three facets of emotional experience: novelty (e.g., “I have felt combinations of emotions that other people have probably never experienced”; α = .84), preparedness (e.g., “I think about and try to understand my emotional reactions”; α = .81) and authenticity and effectiveness of emotions experienced (e.g., “The way I experience and express my emotions helps me in my relationships with others”; α = .82). The items are scored on a 5-point Likert scale ranging from “much less” to “much more.” The overall ECI scores range from 30–150 and for the purpose of this paper the scales will be considered together. ECI’s construct validity indicates that the scale is a reliable predictor of creative behaviour (Ivcevic, Brackett, & Mayer, 2007).

**Task-evaluation questionnaire.** A single item presented after the experimental phase and before debriefing asked participants to rate the pleasantness of the experimental task (after the fact) on a scale of 1 to 10 (“very unpleasant” to “very pleasant”).

Procedure

The project was given ethical approval by the University of Greenwich Departmental Research Ethics Committee. The procedure of this study is identical to that of the second experiment in Luke, Roe and Davison (2008), except that participants were randomly assigned to intentional/nonintentional conditions and completed some different questionnaires.

Prior to commencement the experimenter determined by true-RNG (via www.random.org which uses atmospheric noise as the randomisation source) which condition the next participant would be in. Using open-deck allocation, by chance 20 participants were allocated to each condition. The participant was then briefed, with those in the intentional condition being told what the exact protocol was and that they should try and intuit what the target
image was in each trial. Participants in the nonintentional condition were informed that they would be doing a psychic test at some point, but the first task was presented as an activity that was intended to gauge their preferences. They were told to follow the instructions and that there would be a number of judging or response tasks to follow. The exact standardised wording for the nonintentional task was:

Now you have the computer task. First of all there is a preparation task to gauge your preferences. The preparation task consists of a number of images of fractals (geometric patterns). You are to select the one you most prefer (i.e., the one that you like the most). There will be a few rounds of this task, but this is a preparation task so just whiz through the fractals as fast as you can. Once you’ve done them there will be some further instructions given to you by the computer at this point but, because the nature of the task varies with each participant, I can’t tell you exactly what this next task will be but it will involve a simple psychic test. You are asked to follow the instructions and complete the tasks as well as you can, and the computer will inform you when the tasks are complete. Any questions?

The wording for intentional task participants was similar but informed them of the nature of the task:

Now you have the computer task. The first task is the psychic test. The instructions on the computer will tell you it is a preparation task but actually the choices you make will be used to gauge your psychic ability. This task consists of a number of images of fractals (geometric patterns). You are to select one of the images and when you do so the computer will then select one at random. Your task is to try and guess, in your own time, which one the computer will select; however you do not receive any feedback on your success. This is the psychic task. There will be a few rounds of this task. Once you’ve done them there will be some further instructions given to you by the computer at this point but, because the nature of the task varies with each participant, I can’t tell you exactly what this next task will be. You are asked to follow the instructions and complete the tasks as well as you can, and the computer will inform you when the tasks are complete. Any questions?

All participants were informed that their data would be recorded anonymously but that they could withdraw from the study at any time and withdraw their data by citing their unique participant identification number. Participants then signed a consent form and completed the questionnaires in the order they appear in the Materials section above, except for the fractal experimental task-evaluation questionnaire, which was filled out after all tasks were completed, but before feedback. Participants were informed that they should complete the following belief and attitude questionnaires and that the instructions and examples for completion were on the forms (so as to keep instructions standardised). Required item responses were self-evident from the questionnaires, but participants were informed that they could ask questions if they had any misgivings. Participants were then left in the private test area alone until they had completed the PMIR precognition computer task and were given detailed task instructions via the computer.

The initial screen of the PMIR-task computer program asked participants to relax and to follow the instructions and explained that they would be informed when they needed to try and use any psychic (psi) ability—although participants in the intentional condition had been informed that this was the actual psi task. Further instructions described how to indicate which one of four presented images they most preferred for each of a series of 10 “preparatory” trials. Images were fractal patterns, displayed in a random square arrangement (see Figure 1) from a unique pool of four images for each of the 10 trials.

Unknown to participants in the nonintentional condition, the 10 “preparatory” fractal trials were actually a nonintentional precognitive psi task. In each trial, once the participants had made their target selection, the computer randomly selected one of the four fractal images as the (post factum) precognition target. Thus, randomised selection of the target occurred each time an image preference was selected by the participant. This randomisation and that of the image position arrangement was achieved using the RND function in the Visual Basic program, which is seeded by the computer’s internal timer, which is not accessible to the participant and remains independent of the participant’s activity on the computer. No immediate feedback was given to the participant on target success.
Following the psi covert/overt task there was an outcome task contingent upon the participant’s psi task performance. Participants who correctly identified fewer than 3 (below MCE = 2.5) were directed by the computer towards a presumably unpleasant vigilance task, whereas those who correctly identified 3 or more targets were directed towards a presumably pleasant cartoon-preference task.

The vigilance task presented a set of instructions describing the task and requesting participants to observe a sequence of numbers and to press the left mouse key (or the return key) for every complete run of three consecutive odd numbers (e.g., 3, 7, 5) or three consecutive even numbers (e.g., 4, 2, 4). Once the task began, a series of singular random numbers ranging from 1–9 were presented in the centre of the screen; each digit was replaced every 500 ms. Duration of the unpleasant task was dependent on degree of success at the precognition task: those who correctly identified none of the precognition targets completed this unpleasant task for 4 min, those who identified only one correct target for 3 min, and those scoring two hits did this task for 2 min. Participants were not informed of how long the task would take nor did they receive any feedback on their performance. No record of the participant’s performance on this task was made.

Participants who performed the cartoon-preference task were first notified that the task would now change but they should continue to select the image they preferred. However, like the vigilance task, this was not a psi task and the cartoon images (taken from Gary Larson’s *Far Side*) were predetermined, not random. Two cartoon images were presented side by side with a tick box for the participants to indicate their favourite. Upon selection, a new pair of images was presented. Participants’ previous nonintentional precognition task performance determined how long the cartoon-preference task continued, such that the task lasted 30 s if they obtained 3 direct hits and increased by 30 s for every additional direct hit based on the premise that seeing more cartoons was an increasingly pleasant task. The experimenters remained blind to all conditions, responses, and performance of the participants until after the participant had completed the entire automated task and questionnaires and had presented all their data.
Upon completion of both tasks participants were asked to rate the pleasantness of the entire computerised experimental phase on the experimental task-evaluation questionnaire. They were then asked by the experimenter, in a neutral manner, to describe what they thought was the purpose of the two tasks so as to determine if they suspected that the first task required them to use ESP. As with previous studies (Luke, Delanoy & Sherwood, 2008; Luke & Morin, 2014; Luke, Roe & Sherwood, 2008) participants gave no indication that they suspected the true nature of the task, given that they were primed for a psi task later—all except one professional parapsychologist who suspected a psi task had been involved but did not know where (Luke, Delanoy & Sherwood, 2008); however, no professional parapsychologists or psychologists were recruited in the subsequent studies. Furthermore, to counter concerns over the possible candidness of responses, a pilot study (Luke, 2007) aimed at destructive testing the design required blinded colleagues to respond critically about the design, yet none suspected the nonintentional psi task. A referee of the paper queried whether or not participants had any potential fears allayed as to whether or not the task was some kind of projective psychological task, but no such reassurance was given. However, the authors regard this matter as somewhat irrelevant given that the main concern was whether or not they knew they were performing a nonintentional task, which according to self-reports they did not; neither did those in the original pilot tests, for which we were explicit in seeking candid responses as to what they thought the tasks were genuinely measuring. Participants thinking the validation question might also be a projective test also seems somewhat irrelevant because candid responses are expected from at least a few respondents in this and previous studies, and yet none determined the true nature of the nonintentional precognition task. Following validation checks, the participants were given feedback on their psi task performance and fully debriefed, with the opportunity to ask questions, and requested not to divulge the nature of the task to other students, who may be later participants. Analyses were preplanned.

Results

A number of data checks were run to ensure against any systematic patterning. The randomised computer target selection was analysed using a chi-square goodness of fit test and was found to be evenly distributed, $X^2(3, N = 400) = .90, p = .82$. Additionally, the randomised computer image display arrangement, with 24 possible square arrangements of the four images, was analysed using a chi-square goodness of fit test and found to be evenly distributed, $X^2(23, N = 400) = 17.74, p = .77$. Finally participant target image position preference was analysed using a chi-square goodness of fit test and found to be evenly distributed, $X^2(3, N = 400) = 4.78, p = .18$.

Regarding Hypothesis One, a lower than chance psi score of 2.3 ($SD = 1.45; MCE = 2.5$) was found overall for all participants, counter to prediction; however, this was not significant, $t(39) = -0.87, p = .39$, two-tailed. As can be seen in Table 1, as predicted by Hypothesis Two, scores in the nonintentional condition ($M = 2.6, SD = 1.27$) in this experiment were higher than in the intentional condition ($M = 2.0, SD = 1.59$), but again the difference was not significant, $t(38) = 1.318, p = .20$, two-tailed (Levene’s test for equality of variance nonsignificant). Further, the mean psi score for the nonintentional condition was above chance, although not significantly, $t(19) = .35, p = .73$, two-tailed. The intentional condition scores were well below mean chance expectation, although not significantly $t(19) = -1.41, p = .18$, two-tailed. Table 1 also shows the hit rates and inferential statistical values for the previous six studies (this being Study 7) also using this protocol.

Regarding Hypothesis Three, the three individual differences measures were correlated with the participants’ psi scores, resulting in a number of small nonsignificant correlations. The sheep-goat correlation was calculated using Spearman’s nonparametric test, given that this measure had a score range of less than 20 (Clark-Carter, 1997).

Exploring subjective task pleasantness (Table 3), participants in the nonintentional condition found the task significantly more pleasant than those in the intentional condition, $r(38) = 2.24, p = .031$, two-tailed (Levene’s test nonsignificant). However, differences between task pleasantness in the intentional versus the nonintentional condition were nonsignificant when inspected specifically for the penalty-contingent outcome, $t(21) = 0.44, p = .66$, two-tailed (Levene’s test nonsignificant), and the reward-contingent outcome, $t(15) = 1.64, p = .12$, two-tailed (Levene’s test nonsignificant). Nevertheless, there was a significant positive correlation between psi scores and subjective task pleasantness ratings overall, $r(38) = .61, p = .00003$, two-tailed, as would be expected if the outcome-contingent task was valid, because psi task success was intended to be directly related to contingent-task pleasantness via duration of the pleasant and unpleasant tasks. Further, the correlations between psi scores and subjective task
pleasantness ratings remained significantly positive when inspected by condition: intentional, \( r_s(18) = .45, p = .046 \), two-tailed; nonintentional, \( r_s(38) = .71, p = .00042 \), two-tailed. The difference between these correlations using Fisher’s \( r \) is not significant, \( z = 1.17, p = .12 \) one-tailed.

Table 1
Hit Rates and Inferential Statistics Across Conditions and Previous Studies

<table>
<thead>
<tr>
<th>Sample</th>
<th>( N )</th>
<th>% Hit rate</th>
<th>( t )</th>
<th>( p ) (two-tailed)</th>
<th>( z )</th>
<th>ES ( t/\sqrt{N} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study 1 (Luke, Delanoy &amp; Sherwood, 2008)</td>
<td>General public</td>
<td>100</td>
<td>28.5</td>
<td>2.51</td>
<td>.01</td>
<td>2.41</td>
</tr>
<tr>
<td>Study 2 (Luke, Roe &amp; Davison, 2008)</td>
<td>General public</td>
<td>25</td>
<td>34.0</td>
<td>2.60</td>
<td>.02</td>
<td>2.39</td>
</tr>
<tr>
<td>Study 3 (Luke, Roe &amp; Davison, 2008)</td>
<td>Students (volunteer)</td>
<td>32</td>
<td>29.0</td>
<td>2.01</td>
<td>.05</td>
<td>1.91</td>
</tr>
<tr>
<td>Study 5 (Hitchman et al., 2012a)</td>
<td>General public</td>
<td>50</td>
<td>26.8</td>
<td>1.14</td>
<td>.26</td>
<td>1.11</td>
</tr>
<tr>
<td>Study 6 (Hitchman et al., 2012b)</td>
<td>General public</td>
<td>50</td>
<td>26.5*</td>
<td>1.62</td>
<td>.12</td>
<td>1.55</td>
</tr>
<tr>
<td>Study 7 - nonintentional</td>
<td>Students (required)</td>
<td>(20)</td>
<td>(26.0)</td>
<td>(0.35)</td>
<td>(.73)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>Study 7 – intentional</td>
<td>(20)</td>
<td>(20.0)</td>
<td>(-1.41)</td>
<td>(.18)</td>
<td>(-1.31)</td>
<td>(-.32)</td>
</tr>
<tr>
<td>Study 7 - overall</td>
<td>40</td>
<td>23.0</td>
<td>-0.87</td>
<td>.39</td>
<td>-0.84</td>
<td>-.14</td>
</tr>
<tr>
<td>Total</td>
<td>338</td>
<td>27.7**</td>
<td>-</td>
<td>.0005</td>
<td>3.51***</td>
<td>-</td>
</tr>
</tbody>
</table>

*Actually hit rate = 53.1 because MCE = 50% for this study as opposed to 25% in other studies. The hit rate has been converted to show the equivalent hit rate for comparison.

** All conditions. The hit rate for the 318 participants in the nonintentional conditions only is 28.2%

*** Stouffer \( Z \) for all studies and conditions. For nonintentional conditions only, Stouffer \( Z = 3.86, p = .0001 \), two-tailed.

Table 2
Correlations Between Psi Scores and Personality Scores

<table>
<thead>
<tr>
<th></th>
<th>( r ) (*( r_s ))</th>
<th>( p ) (two-tailed)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Emotional creativity</td>
<td>.16</td>
<td>.32</td>
</tr>
<tr>
<td>Openness to experience</td>
<td>.09</td>
<td>.59</td>
</tr>
<tr>
<td>Sheep-goat psi belief</td>
<td>-.09*</td>
<td>.58</td>
</tr>
</tbody>
</table>

Regarding possible intergroup personality differences (Table 3) there were no significant differences between the groups on the various individual differences measures, and the groups had equal variances as determined
by Levene’s tests, both of which indicate that group allocation was suitably randomised. A medium-sized positive
correlation was found (see Tables 4 and 5) between Emotional Creativity Inventory scores and openness to experi-
ence scores, as would be expected (Ivcevic et al., 2007).

### Table 3
**Comparisons of Mean Task Pleasantness (1 to 10 Scale) and Personality Measures for Different Intentional Conditions**

<table>
<thead>
<tr>
<th>Mean (SD in parentheses)</th>
<th>Intentional</th>
<th>Nonintentional</th>
<th>Overall</th>
</tr>
</thead>
<tbody>
<tr>
<td>Task pleasantness Penalty</td>
<td>2.92 (1.32)</td>
<td>3.22 (1.86)</td>
<td>3.04 (1.52)</td>
</tr>
<tr>
<td>Reward</td>
<td>5.00 (2.28)</td>
<td>6.73 (1.95)</td>
<td>6.12 (2.18)</td>
</tr>
<tr>
<td>Combined</td>
<td>3.55 (1.87)</td>
<td>5.15 (2.58)</td>
<td>4.35 (2.37)</td>
</tr>
<tr>
<td>Emotional creativity</td>
<td>94.70 (12.69)</td>
<td>99.60 (15.43)</td>
<td>97.15 (14.16)</td>
</tr>
<tr>
<td>Openness to experience</td>
<td>1.60 (1.39)</td>
<td>1.20 (1.32)</td>
<td>1.40 (1.35)</td>
</tr>
<tr>
<td>Sheep-goat psi belief</td>
<td>76.70 (8.29)</td>
<td>76.20 (7.95)</td>
<td>76.45 (8.02)</td>
</tr>
</tbody>
</table>

### Table 4
**Correlation Coefficients (p Values) for Intercorrelations of Personality Variables Across Conditions**

<table>
<thead>
<tr>
<th>Intentional condition (n = 20)</th>
<th>ECI</th>
<th>Psi belief</th>
</tr>
</thead>
<tbody>
<tr>
<td>Psi belief</td>
<td>.26 (.26)</td>
<td>-</td>
</tr>
<tr>
<td>OTE</td>
<td>.40* (.076)</td>
<td>-.18 (.46)</td>
</tr>
<tr>
<td>Nonintentional condition (n = 20)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Psi belief</td>
<td>.04 (.85)</td>
<td>-</td>
</tr>
<tr>
<td>OTE</td>
<td>.45* (.048)</td>
<td>-.08 (.72)</td>
</tr>
</tbody>
</table>

Correlation coefficient is $r_s$ (Spearman) unless indicated by * as $r$ (Pearson)

Using Fisher’s test none of the differences between the correlations were significant, because the greatest difference across conditions, between psi belief and ECI, was not significant $z = 0.66, p = .51$, two tailed. Therefore there are no apparent concerns with randomization of participants across conditions.

### Table 5
**Correlation Coefficient (and p Value) for Intercorrelations of Personality Variables for Both Conditions Combined**

<table>
<thead>
<tr>
<th>Both conditions (N = 40)</th>
<th>ECI</th>
<th>Psi belief</th>
</tr>
</thead>
<tbody>
<tr>
<td>Psi belief</td>
<td>.15 (.37)</td>
<td>-</td>
</tr>
<tr>
<td>OTE</td>
<td>.42* (.006)</td>
<td>-.04 (.79)</td>
</tr>
</tbody>
</table>

Correlation coefficient is $r_s$ (Spearman) unless indicated by * as $r$ (Pearson)
Comparison of Outcomes With Nonintentional and Intentional Precognition Tasks

Discussion

Unlike the previous six experiments using this test paradigm, and counter to prediction, the overall psi score was below chance; however, this finding was not significant. Nevertheless, as predicted, the psi scores for the nonintentional condition were higher than for the intentional condition, although the difference was not significant. Furthermore, upon comparing the two conditions it can be seen that—as with the previous six studies—the mean psi score for the nonintentional condition was above chance, although not significantly, and the intentional condition scores were well below mean chance expectation, although not significantly. This is the first time in the seven studies conducted so far that scores on this task have dropped below mean chance expectation, but only for this intentional psi condition, as would be partially predicted by the PMIR model. However, none of the scores or differences between them were significant in this study, so any positive interpretation must be treated with caution.

Concerning the individual difference measures, there was no significant correlation between psi scores and any of the three measures, thereby failing to replicate the previous findings in two of three studies of a relationship with belief in psi. Furthermore, the previous positive relationship between psi scores and openness to experience found in two of the four previous studies was not replicated, and neither was there evidence of a relationship between emotional creativity and psi scores. However, as with all previous studies, there was a positive relationship between subjective task pleasantness ratings and psi scores overall, indicating that the contingent tasks were valid. Further, these positive correlations remained significant for both the intentional and nonintentional conditions, and somewhat more so in the nonintentional condition, though not significantly so.

The discrepancy in overall score between this and previous studies appears, in part, due to the introduction of the intentional task, which was anticipated to have a negative effect on scoring. The mean in the nonintentional condition was in the same positive direction as in our previous studies. Our interpretation of the PMIR model partially predicted that performance on the psi task would be lower in the intentional condition than in the nonintentional one; however, the below chance scoring wasn’t necessarily predicted by the model, although this might occur in certain circumstances. In any case psi scoring in the intentional condition, although above chance expectation, is still just slightly so (26% hit rate) and nonsignificant. One possible explanation for the lack of significant difference in precognition scores between the intentional and nonintentional conditions could involve the assumption that intentional tasks necessarily consume more cognitive processing capacity than nonintentional tasks. This is not necessarily explicit from Stanford’s (1990) PMIR model and is the authors’ interpretation of his model.

One reason for the reduced psi scoring in this study relative to previous studies in this series may have to do with the sample. As can be seen from Table 1, all but one of the previous six studies (Study 2 of Luke, Roe, & Davison, 2008) either partly or wholly utilised members of the public, often attending psi- or psychology-related events, who volunteered without reward to take part out of curiosity and personal interest. By contrast the present study used university students who gained research participation points for taking part and may have been somewhat less motivated to achieve a high score than participants in previous studies using this test paradigm, who were all unrewarded volunteers and scored higher on the precognition task.

In any case, an inspection of the percentage hit rates across all seven studies (Table 1) shows that after an initial peak for Study 2, psi scores have declined slowly in a relatively linear fashion. Taking the overall scores for all seven studies, there is a significant decline in effect size as the studies progressed, $r_s = -.79, p = .04$, conforming to the supposed decline effect within parapsychology (e.g., for a review see Colborn, 2007). Given that the protocol has remained essentially the same across studies, and that the controls have not been increased (the paradigm was already tightly controlled), at least considerations of increased quality can be fairly safely ruled out, so the effect appears likely to be either psychological, such as the motivation of the researcher, or a physical property of psi, such as some kind of quantum-mechanical factor perhaps due to the violation of information transfer limitations with quantum systems (for an overview see Colborn, 2007).

Ultimately the large decline in scores for this series of studies can be seen to occur partly as a consequence of the destructive testing of the PMIR model with this test paradigm, as can be seen in the discrepancy of scores between the intentional and nonintentional conditions. However, in part, a steady decline effect across studies is also still evident even when the intentional component is considered (see Table 1).

In summary, this study failed to find clear evidence for a psi effect nor a superior effect of nonintentional
versus intentional psi task, despite finding a nonsignificant effect of intention in the predicted direction. Furthermore, none of the individual difference measures were found to be related to psi score either. Tentative explanations put forward for the lack of significant results include the destructive testing of the test paradigm, a nonmethodology-based decline effect (e.g., due to the experimenter motivation or quantum-based decline effects across studies), and motivational factors related to the use of a student sample with a motive other than to get a good psi score. Factors relating to statistical power might also be considered, because, even though the effects were in the opposite direction overall, power analysis using G*Power (Faul, Erdfelder, Buchner, & Lang, 2009) indicate that with a mean effect size of \( d = .23 \) a total sample size of 79 is recommended for power = .8 so, with 40 participants power = .55, this study is somewhat underpowered. Power issues aside, finally, however, this test paradigm for exploring the PMIR model of psi continues to demonstrate utility as the overall nonintentional psi task performance was Stouffer \( Z = 3.86 \) \((p = .0001)\) across seven studies, making this a somewhat robust measure of unintentional precognition, which deserves further exploration. Currently, however, although the previous studies have demonstrated, to some extent, the utility of the intrinsic reward (e.g., Luke & Morin, 2014) inherent within the PMIR model, the utility of the nonintentional psi task paradigm, explored here, needs further testing, ideally with a larger and more motivated sample.

References


Comparison of Outcomes With Nonintentional and Intentional Precognition Tasks


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Abstracts in Other Languages

Spanish

COMPARACIÓN DE RESULTADOS EN TAREAS DE PRECOGNICIÓN INTENCIONAL Y NO INTENCIONAL

RESUMEN: El modelo de Stanford de respuesta psi mediada instrumentalmente (PMIR) propone que psi es una función evolutiva adaptativa que trabaja en gran parte en servicio del organismo, pero que opera a un nivel casi completamente inconsciente. Una serie de experimentos exitosos de Luke y asociados han explorado el modelo PMIR con una tarea de precognición no intencional automatizada con tareas de resultado contingente postexperi- mental que varían en agradabilidad de acuerdo con el éxito en la tarea psi. Hasta ahora este paradigma sólo había explorado tareas no intencionales por lo que este estudio comparó condiciones de prueba intencional y no intencio- nal para evaluar la proposición de psi inconsciente del modelo PMIR. Una muestra de 40 estudiantes de psicología completaron 10 pruebas de la tarea automatizada de precognición, con 20 participantes asignados al azar a la condi- ción no intencional y 20 a la condición intencional en un diseño de grupos independientes. Contrariamente a los estudios previos los resultados psi en general estuvieron por debajo de la expectativa media de azar (MCE), aunque no significativamente. En línea con las predicciones basadas en el modelo PMIR, sin embargo, los participantes de tareas en la condición no intencional puntuaron más alto que la MCE y que el grupo con la prueba intencional, aunque estas diferencias no fueron significativas. Las medidas de creencia en psi, apertura a la experiencia, y cre- atividad emocional no correlacionaron con las puntuaciones de psi. Discutimos los resultados a la luz de estudios previos con sugerencias para futuras investigaciones.

French

COMPARAISON DE RESULTATS AVEC DES TACHES DE PRECOGNITION NON-INTENTIONNELLES ET INTENTIONNELLES

RESUME : Le modèle de Stanford de la réaction instrumentale médiatisée par le psi (PMIR) propose que le psi est une fonction adaptative et évolutive qui serait en grande partie au service de l’organisme, mais qui opérerait à un niveau d’inconscience presque totale. Une série d’expériences réussies conduites par Luke et ses associés ont exploré le modèle du PMIR avec une tâche de précognition non-intentionnelle automatisée avec des tâches post-ex- périmentales dépendantes des premiers résultats, tâches étant d’autant plus plaisantes que le succès à la tâche psi était élevé. Jusqu’à présent, ce paradigme avait été exploré seulement en utilisant des tâches non-intentionnelles ; c’est pourquoi la présente étude compare des conditions avec des tâches psi non-intentionnelles et intentionnelles pour explorer la proposition concernant le psi inconscient dans le modèle du PMIR. Un échantillon de 40 étudiants
en psychologie ont complété 10 essais chacun d’une tâche de précognition automatisée, avec 20 participants aléatoirement placés dans une condition non-intentionnelle et 20 placés dans la condition intentionnelle dans un protocole avec des groupes indépendants. Contrairement aux précédents résultats, les résultats pour le score psi global étaient inférieurs à ce qu’on pouvait attendre du hasard, bien que de manière non-significative. Toutefois, en accord avec les prédictions basées sur le modèle du PMIR, les participants placés dans la condition non-intentionnelle ont obtenu des scores supérieurs au hasard et également supérieurs à ceux placés dans la condition intentionnelle, quoique ces différences n’étaient pas significatives. Les mesures de la croyance au psi, de l’ouverture à l’expérience et de la créativité émotionnelle ne montrèrent pas de corrélations avec les scores psi. Les résultats sont discutés à la lumière des précédentes études et des suggestions sont faites pour de futures recherches.

German

EIN VERGLEICH DER ERGEBNISSE VON NICHTABSICHTLICHEN MIT ABSICHTLICHEN PRÄKOGNITIONSQAUFGABEN

THE RELATIONSHIP BETWEEN LOCAL GEOMAGNETIC ACTIVITY AND PSYCHIC AWARENESS

By Serena M. Roney-Dougal, Adrian Ryan,* and David Luke**

ABSTRACT: There is evidence that psychic experiences are related to geomagnetic activity (GMA). However, sometimes these are related with higher, and sometimes with lower, GMA. This anomaly may be clarified by studying local GMA. In order to explore this, a 4-year investigation was conducted. Unfortunately, solar activity and GMA were at their lowest for 100 years, so it was necessary to combine the preliminary and follow-up study data. Both studies used the same free-response design. The 26 participants, all of whom resided at, or near, Samye Ling Tibetan Centre, had practiced meditation for at least 10 years. Each participant completed a minimum of eight sessions each year. Three questionnaires and one psychological test were completed. Geomagnetic measurements were supplied by the British Geological Survey’s observatory located near Samye Ling. There was no difference between sessions conducted during high or low band 1, or high or low band 3 GMA, but post-hoc analyses suggested a band 3 GMA effect. Overall, males’ psi scores were significantly negative ($p = .02$, two-tailed), whereas females scored at chance; the difference was significant ($p = .03$). The participants with the highest temporal lobe questionnaire scores showed the strongest correlation of psi with GMA ($p = .06$).

Keywords: local geomagnetic activity, precognition, temporal lobe experiences questionnaire (TLEQ), season

The Link Between Psi and Geomagnetic Activity (GMA)

GMA is the aggregate of disturbances in the natural magnetic field surrounding the Earth, caused by the interaction of that field with plasma (electrically charged gas) ejected from the Sun during solar storms. For the past three decades research in parapsychology has found evidence that psychic experiences may be affected by fluctuations in the geomagnetic field, that is, geomagnetic activity (GMA). For a detailed review of the literature see Roney-Dougal, Ryan, and Luke (2013).

Receptive psi studies have found that, in general, psi scoring was greater during periods of low GMA as measured by the global indices (e.g., Krippner & Persinger, 1996; Persinger & Krippner, 1989). Occasionally, however, the opposite is reported (e.g., Radin, McAlpine, & Cunningham, 1994). Hubbard and May (1987) criticized research into the psi-GMA link for relying on global GMA measurements and urged that local measurements be made.

In a meta-analysis of 51 studies comprising 2,879 free-response trials, Spottiswoode (1997a) found that the correlation of psi with global GMA was much stronger in a 2-hr window centered close to the local sidereal time (LST) effect size peak. Dalkvist and Westerlund (2000) suggested that Spottiswoode’s findings could be explained if performance was affected by a factor that varied by an interaction of time of day and time of year. Ryan (2008) noted that as the large majority of ESP experiments in Spottiswoode’s database were carried out during the daytime, an influencing factor with seasonal variation would generate a systematic variation of psi effect by LST. The trials in Spottiswoode’s database did indeed exhibit a seasonal variation of psi effect (Sturrock & Spottiswoode, 2007). Ryan (2008) identified geomagnetic pulsations as a candidate for the underlying factor; these regular fluctuations in the geomagnetic field are the components that make up local GMA. They are classified into five bands according to frequency (i.e., the number of waves per second) and character (regular sinusoidal or irregular; Campbell, 2003; Jacobs, 1970), and each type exhibits distinct seasonal and/or interacting seasonal/daily variation (Jacobs, 1970). Pulsation strength and frequency tend to be related; in other words, pulsations at the lower end of the frequency spectrum tend to be the strongest.

Ryan (2008) studied the relationship between local geomagnetic pulsations and the results of 343 ganzfeld and remote viewing sessions and found that psi effect size did indeed vary by pulsation frequency. Ryan suggested
that these results may explain the inconsistent relationship between psi and GMA. He found that low frequency band 3 (0.025–0.1 Hz) geomagnetic pulsations were in general associated with poor psi performance (a finding further supported by Ryan and Subbotsky, 2010), whereas higher frequency band 1 (0.2–0.5 Hz) pulsations were associated with enhanced psi performance. The positive correlation between ESP and high frequency band 1 activity is a new finding. Local band 1 activity generally is not correlated with anp but in some years it is positively correlated with it. Low frequency band 3 disturbances correlate with the global GMA index and it is this aspect that relates to the correlations previous studies have found. However, there have been occasions where the general finding of negative correlation of psi with global GMA has been reversed and the high frequency band 1 component could well be implicated here, as Ryan (2008) found psi scoring related to enhanced band 1 activity. Ryan (2008) also suggested that an apparent association of specific frequencies of geomagnetic pulsations with psi may explain the relationships between LST, GMA, and psi performance reported by Spottiswoode (1997a, 1997b). Geomagnetic pulsations exhibit seasonal variation and will therefore be distributed unevenly across LST, due to the fact that psi trials have been mostly conducted in the daytime.

Research also suggests that there may be a lunar effect on psi (Etzold, 2005; Radin & Rebman, 1994, 1998; Sturrock & Spottiswoode, 2007). There is evidence from the geophysics literature that the moon affects GMA via tidal/gravitational effects (Fraser-Smith, 1982; Stenning, Carmody, & Du, 2002), although these small effects would seem to be insufficient to explain the relationships between lunar phase and psi that have been observed. A problem in this area is the potential for solar rotation to confound lunar analyses, as the lunar synodic month (29.5 days) is close to the synodic period of solar rotation (between 25 and 30 days, depending on solar latitude).

To look at individual differences related to GMA and psi scoring, we had our participants complete the Temporal Lobe Experience Questionnaire (TLEQ). This questionnaire addresses various different aspects of response to phenomena which may be related to disturbances in the temporal lobe. Neppe (1984) and Cook and Persinger (2001) suggested that GMA may affect the temporal lobes and that people who experience temporal lobe symptomatology may also have spontaneous and laboratory psi-type experiences. In other words, sensitivity to GMA may be correlated with temporal lobe symptomatology and increased psi experiences.

Because we were working with meditators we were interested in attempting to assess the degree to which meditation affected psi scoring. It is very tricky to assess the degree to which the practice of meditation alters a person. The Freiburg Mindfulness Inventory (FMI; Walach, Buchheld, Buttenmuller, Kleinknecht, & Schmidt, 2006) is a recent attempt to assess such changes, and so we administered it on a purely exploratory basis. Similarly the Necker cube test (Sauer et al., 2012) was designed to assess the effect of meditation on attention, and so this was used for the same purpose. The final questionnaire was the Meditation Attainment Questionnaire (MAQ) which had been used in previous research (Roney-Dougal & Solfvin, 2006, 2011; Roney-Dougal, Solfvin, & Fox, 2008) in which years of practice had been found to be related to increased psi scoring. Therefore, we once again asked participants to complete this questionnaire. Results from these questionnaires will not be presented in this paper.

Specific Environment

One of the key aims of the project was to replicate and extend the work of Ryan (2008) by exploring the features of local GMA that may modulate performance in receptive psi trials. Measurement of the geomagnetic pulsations that characterize the local geomagnetic environment requires an exceedingly sensitive magnetometer (0.1 nT or better). There are currently five suitable magnetometers in continuous operation within the U.K. One of these magnetometers is located in Eskdalemuir, 2 miles from Samye Ling Kagyu Tibetan monastery, the first Tibetan Buddhist center to be established in the West.

Many cycles are evident in records of GMA: diurnal, solar rotation, lunar, seasonal cycles, and an 11-year solar activity (sunspot) cycle. The latter is by far the most prominent, and so the psi trials were conducted over a 4-year period with the aim that the full range of GMA conditions would be represented. The investigation was split into two studies:

1. The preliminary study to initiate research into diurnal, solar rotation, lunar, and seasonal effects.
2. The formal follow-up study to verify any potential findings from the preliminary study and also to look at the effect of the sunspot cycle, and any other longitudinal findings.
In this paper, results are presented for the preliminary and follow-up studies combined to allow an overall assessment for the whole 4 years, because GMA was at an extremely low level for the entire 4 years.

**Hypotheses**

**Exploration of Psi-Conducive Factors in Local GMA**

**Formal hypotheses.**

1. Psi scoring for sessions conducted during “high” band 3 (band 3 = 0.025–0.1 Hz) GMA will be lower than psi scoring for sessions conducted during “low” band 3 GMA. The “low/high” cut-point (i.e., the threshold at which GMA is regarded as “high”) in this hypothesis is as observed in Ryan (2008). The use of this cut-point may be important because there is long-term (i.e., solar cycle) variation in the prevalence of pulsation activity. Thus the proportion of psi sessions in the “high” or “low” categories may depend on the year of experimentation.

2. Band 1 GMA (0.2–0.5 Hz) will be positively correlated with psi scoring. Spearman’s test will be used owing to the skewed distribution of GMA values. (Note: In the band 1 frequency range there are considerable variations between the response characteristics of magnetometers at different sites and during different periods. This presents difficulties in establishing the cut-point between “low” and “high” activity. For this reason, the bifurcation of sessions and subsequent t-test analysis planned at outset was not possible, and so the correlation hypothesis was used to assess the influence of this band of activity.)

**Exploratory hypotheses.** Participants’ total scores on the TLEQ questionnaire will be correlated: (a) negatively with the correlation between band 3 GMA and psi scoring; and (b) positively with the correlation between band 1 GMA and psi scoring.

Longitudinal analyses were:

1. the overall psi score over time
2. the correlation of GMA with psi score for individuals who had done at least two series of eight sessions. There were some individuals who participated for all four years.

**Exploration of Psi-Conducive Factors in Seasonality and Lunar Phase**

**Exploratory hypotheses.** (a) The daily maximum temperature and season of the year would be positively correlated with psi scoring; (b) psi scoring would be related to lunar phase.

**Method**

**Design**

A clairvoyance/precognition free-response design was used in which the participant attempted to correctly choose a picture selected at random by computer. The computer programme (PreCOG) chose a target set at random from a pool of 25 sets, and a 1 min video-clip at random from the selected four-video set. PreCOG also pseudo-randomly chose whether the session would be in clairvoyance mode (target chosen at beginning of session) or precognition mode (target chosen after participant had made their choice), such that half the sessions were in each mode. Each participant completed a minimum of eight sessions each year. PreCOG had already been run successfully with monastic communities in India; the sessions could be run without assistants, enabling the experimenter (the first author) to work with the participants at any time that was convenient for them and in the participant’s preferred meditation place. Most participants did one session a week at the same time of day, depending on their availability. This design has both a randomized double-blind and inbuilt fraud control, owing to PreCOG randomly choosing the target so that neither experimenter nor participant had any idea which target set was being used. This design
was used here for consistency with previous Indian meditation studies run by the experimenter (Roney-Dougal & Solfvin, 2011).

The preliminary study ran from December 6, 2008 until May 27, 2010. The follow-up study ran from September 14, 2010 until October 4, 2012.

Participants

The participants, who were either residents at Kagyu Samye Ling Tibetan monastery or the local Eskdalemuir village, had practiced meditation for at least 10 years and were practicing Tibetan Buddhist meditation regularly during the period of research. The oldest participant had been practicing for 53 years. Overall, in both the preliminary and follow-up studies, there were 25 participants who contributed 391 sessions. There were a total of 11 male and 14 female participants; one was a monk and two were nuns, the remainder being lay people. Ages ranged from 37–94 years.

A personal meeting with potential participants was arranged, the project described in detail, and an invitation to participate made. Any candidate who volunteered for a minimum of eight sessions was included in the study. Thus an informal consent procedure was used. The monks and nuns also obtained permission from the monastery abbot.

Eleven participants each completed one set of eight sessions, 8 participants completed two sets of eight sessions, 4 participants completed three sets, and 2 completed the maximum possible of four sets, making a total of 376 sessions. To eliminate bias due to optional stopping, extra sessions were excluded from the psi data analyses. In the GMA analyses the primary concern was not each individual’s performance, therefore optional stopping is irrelevant. For the purpose of the GMA and longitudinal analyses only, 7 participants completed extra sessions (12 in total) at the end of their eight-session set. Three extra sessions were provided by a participant in the preliminary study who sadly was unable to complete his set of eight sessions and so has been excluded from the psi data analyses, giving a total data set for the GMA analyses of 391 sessions.

Materials

The precognition computer programme (PreCOG) was developed for this field research by Jezz Fox for an Apple Macintosh MacBookPro running OS X. Custom-written software guided the participant through the procedure. A configuration file allowed specifics of the design to be set, including (a) the number of trials each participant would complete and (b) the point in the procedure at which the target was selected (randomly before the trial period for the clairvoyance protocol, after the trial period for the precognition protocol).

For each year there were 25 target sets, each containing four clips. Neither participant nor experimenter ever had prior knowledge of the potential target. In the preliminary study, the first series was run using the static targets developed for the Tibetan research (Roney-Dougal & Solfvin, 2011), and the second series used dynamic 1-min video clips developed by Dalton (Dalton, Steinkamp, & Sherwood, 1996). Dynamic video-clip targets developed for ganzfeld research by Northampton University (Roe & Holt, 2006) were used in the follow-up study. Target selection by the computer was a two-stage process: firstly a selection of the set was made, such that the participant never received the same set more than once, then a random selection of the target from within the set. All the randomizations were performed using pseudo-random algorithms.

For the choosing/rating stage of the procedure, PreCOG displayed the four items initially simultaneously at half size, and then the participant looked at them one at a time on the screen at their full size, in the order A, B, C, and D. When all four had been viewed they were again displayed simultaneously on the screen for rating on a scale of 1 to 100, with the restriction of each item having to be awarded a unique rating. Following the ratings, the data were recorded to disk before providing feedback to the participant by displaying the target for the session. The participant’s mentation was recorded throughout the session.

The MAQ (Roney-Dougal & Solfvin, 2011) assessed the number of years the participants had practiced different disciplines, such as breathing techniques (pranayama) and different types of meditation practice, and the number and duration of meditation retreats the person had completed. Each participant estimated the number of hours per day or week that they had practiced the various techniques.
The TLEQ, also known as the Iowa Interview for Partial Seizure-like Symptoms (IIPSS; Neppe, 1984; Roberts, 1999), contains 40 items encompassing sensory, cognitive, and affective symptoms, as well as nocturnal phenomena that Cook and Persinger (2001) suggest may be linked with sensitivity to magnetic disturbances. Of the 40 items, 39 are rated on a 7-point Likert scale (0–6), ranging from “never” (0) to “more than once a day” (6). One item is a simple yes-no question.

The FMI (Walach et al., 2006) comprises 14 items scored on 4-point scales from “rarely” to “almost always.” The questions are related to the generally accepted effects of mindfulness training.

The Necker cube test (Sauer et al., 2012) uses the bi-stable Necker cube image to assess ability to maintain focus of attention, which is associated with meditation attainment. The participant presses a key every time the image shifts from one to the other viewpoint. The measure used is the degree to which the person is able to hold to one or the other aspect of the bi-stable image.

**Procedure**

The procedure for each session was the same. It was recorded and presented in English on the computer and this guided the participant through the session. The same time of day and location were used, wherever possible, for each session with a given participant, and if possible, the participant always sat facing the same direction, which was recorded with a compass. Participants did only one session per day.

There was a 5-min relaxation period, a statement of intent to become aware of the target picture, followed by a 15-min meditation practice. At the end of this there was a 4-min awareness period during which participants were instructed to allow their mind to go blank and allow any target-related experience to occur. On completion of the awareness period, participants drew any mentation they had experienced that they thought might be related to the target on a pad of paper. The experimenter then joined them and they verbally described their experiences. This was recorded on the computer. The participants then saw all four video clips starting with video A and rated them on a 1–100 point rating scale according to their degree of confidence that the video was the target. Finally, the computer showed the actual target video.

After completing five sessions, each new participant completed the TLEQ; the next session they completed the FMI, and after the seventh session the Necker cube test. After eight sessions they completed the MAQ and were interviewed to obtain qualitative data concerning their previous experience of, and belief in, psychic abilities, as well as their experience of participating in the research. Participants completed the MAQ after each series of eight sessions.

**Geomagnetic Data**

Local geomagnetic field measurements, recorded each second, were collected from the British Geological Survey’s observatory at Eskdalemuir and converted into the appropriate frequency bands by fast Fourier transform (FFT). For each session GMA was calculated for the 2-hr period ending 20 min after the start of the session-awareness period. These measurements were analyzed only after all psi data had been collected and analyzed for psi effects, so as to maintain complete masking as to this variable.

**Analysis of Psi Scoring and GMA**

The target-rating analysis developed for the previous meditation studies (Roney-Dougal & Solfvin, 2011; Roney-Dougal et al., 2008) was again used here to assess the level of psi awareness shown by the participants. The basic unit of analysis for the psi scoring was the participant’s rating of the target for the session. This was normalized by a z score, TrDev, which was standardized relative to the mean and standard deviation of all ratings assigned in the session:

\[
\text{TrDev} = (\text{target rating} - M \text{ of session ratings}) / SD \text{ of session ratings}
\]
Target rating: the rating (1–100) assigned in the session to the actual target
Mean of session ratings: average of all four ratings assigned to session pool
SD of session ratings: standard deviation of all four ratings assigned to session pool
The TrDev score ranged from -1.5 to +1.5, with MCE = 0

This variable is essentially a standard normalization procedure, akin to a $z$ score, which can then be used for significance and effect size analyses. This method of analyzing free-response ratings was developed and used by Stanford and Sargent (1983). There are problems with all the methods for analyzing free-response data. It was decided to use this method because it is the most sensitive to the participant’s clarity of choice of the target.

**Results**

**Overall Psi Scoring**

Before analyzing the hypotheses, overall psi scoring was assessed for each eight-session series for each participant, and preliminary analyses were conducted to establish that the data could be pooled for the GMA analyses. Overall psi scoring is summarized in Table 1, and mean scoring for each participant overall in both studies is illustrated in Figure 1.

**Table 1**

<table>
<thead>
<tr>
<th></th>
<th>$N$</th>
<th>MeanTrDev</th>
<th>$t$</th>
<th>$df$</th>
<th>$p$ (two-tailed)</th>
<th>ES($r$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preliminary</td>
<td>152</td>
<td>-0.07</td>
<td>-1.13</td>
<td>151</td>
<td>.26</td>
<td>-.09</td>
</tr>
<tr>
<td>Follow-up</td>
<td>224</td>
<td>-0.03</td>
<td>-0.62</td>
<td>223</td>
<td>.53</td>
<td>-.04</td>
</tr>
<tr>
<td>Overall</td>
<td>376</td>
<td>-0.05</td>
<td>-1.20</td>
<td>375</td>
<td>.23</td>
<td>-.06</td>
</tr>
</tbody>
</table>

*Figure 1*. Mean psi score for each participant for both studies, with 95% confidence intervals.
This psi scoring is disappointing: research with long-term meditators was planned because significant psi scoring was anticipated as a result of previous findings. The absence of an overall psi effect requires an explanation, which only further research can determine. The individual psi scores are presented in the Appendix.

Gender

Table 2 shows psi scoring by gender overall for both studies. When the preliminary and follow-up studies were combined, the males were found to score significantly below chance ($p = .02$). An ANOVA with gender and participant as factors indicates a significant difference: for gender, $F(1, 350) = 5.72, p = .03$. Therefore, in the GMA analyses it is necessary to do further analyses by gender.

Table 2

<table>
<thead>
<tr>
<th></th>
<th>$n$</th>
<th>MeanTrDev</th>
<th>$t$</th>
<th>$df$</th>
<th>$p$ (two-tailed)</th>
<th>ES($r$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Males</td>
<td>136</td>
<td>-0.16</td>
<td>-2.37</td>
<td>135</td>
<td>.02</td>
<td>-.20</td>
</tr>
<tr>
<td>Females</td>
<td>240</td>
<td>0.01</td>
<td>0.18</td>
<td>239</td>
<td>.86</td>
<td>.01</td>
</tr>
</tbody>
</table>

Clairvoyance vs. Precognition

Table 3 shows psi scoring by session type (clairvoyance or precognition) for both studies combined. The table shows that the scoring in clairvoyance and precognition sessions was close to chance levels with no significant difference between the two, $t(374) = 0.23, p = .82$. Neither was there a significant interaction between gender and session type for both studies combined, $F(1, 372) = 0.17, p = .68$.

This was predicted on the basis of two previous studies (Roney-Dougal & Solfvin, 2006; Roney-Dougal et al., 2008). Thus, three studies using PreCOG have found no difference between clairvoyance and precognition sessions. Therefore, in this respect the psi data can be considered homogenous for the GMA analyses.

Table 3

<table>
<thead>
<tr>
<th></th>
<th>$n$</th>
<th>MeanTrDev</th>
<th>$t$</th>
<th>$df$</th>
<th>$p$(two-tailed)</th>
<th>ES($r$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Clairvoyance</td>
<td>184</td>
<td>-0.04</td>
<td>-0.65</td>
<td>183</td>
<td>.52</td>
<td>-.05</td>
</tr>
<tr>
<td>Precognition</td>
<td>192</td>
<td>-0.06</td>
<td>-1.06</td>
<td>191</td>
<td>.29</td>
<td>-.08</td>
</tr>
</tbody>
</table>

Longitudinal Analysis: Variation of Psi Scoring Over Time

There was little trend of psi scoring across the period of experimentation (preliminary study and follow-up study combined); the correlation of date with psi scoring was $r(389) = .02$, and a correlogram confirmed that the psi scoring data was sufficiently stationary to proceed with GMA analyses without further adjustment. The traditional decline effect found in parapsychology was not present in these data.

GMA During the Period of Experimentation

Figure 2 illustrates the planetary GMA ($Ap$) from 1970 to 2012. The period of the preliminary study (December 6, 2008 until May 27, 2010) and follow-up study (September 14, 2010 until October 4, 2012) are marked. Contrary to solar activity forecasts, the recent minimum of solar activity extended over several years, resulting in
extreme lows of GMA not experienced since the end of the 19th century (Echer, Tsurutani, & Gonzalez, 2011). Thus, the timing of the study was, in retrospect, very unfortunate.

Figure 2. Smoothed index of planetary GMA (Ap) from 1970 to 2013. The period of the preliminary study is marked in black, and the follow-up study in grey.

Figure 3. Left: Band 3 GMA during the period studied by Ryan (2008). Right: Band 3 GMA during the period of preliminary and follow-up studies.

Figure 3 shows the dramatic change in GMA, from the 9-year period which informed the hypotheses (Ryan, 2008) to the 4 years of this study. Therefore, the findings are for a unique window in time and so could possibly be useful as baseline data for future research using local GMA.

Band 3 GMA: Formal Hypothesis

For the GMA-psi analyses, data from all 391 sessions were used. For the geomagnetic analysis, the differences between individuals are not relevant, only the differences in success between days, because it is important to know why some days seem to be good for psi experimentation and others not. Therefore, the between-participant variance (inter-participant differences) has been removed, not the (much larger) session by session differences, by adding to each psi score (TrDev), a constant, for each participant, such that the mean for each participant is equal to the mean for all sessions. Thus, each participant’s psi scores have been shifted up or down, but the variance has not been altered. The advantage of using TrDevAdj is that if we get a significant result, we know that this cannot simply be due to talented participants conducting their sessions during periods of particularly high or low GMA, because the differences in each individual’s overall scoring have been removed.
There was no significant difference between psi scoring during periods of low and high band 3 activity, as shown in Table 4. Table 5 shows a breakdown of the overall results by gender. It is noteworthy how few sessions are in high band 3, only one session in the preliminary study and only 11 in the follow-up. This lack of data severely weakened the power of the band 3 analyses. The males showed the hypothesised drop in psi scoring in high band 3, whereas the females showed an opposite trend, neither to a significant degree.

Table 4

*Adjusted Psi Scoring in Relation to Band 3 GMA for Preliminary, Follow-up, and Overall*

<table>
<thead>
<tr>
<th></th>
<th>High band 3</th>
<th>Low band 3</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
<td>TrDevAdj</td>
<td>N</td>
<td>TrDevAdj</td>
<td>t</td>
</tr>
<tr>
<td>Preliminary</td>
<td>1</td>
<td>0.40</td>
<td>159</td>
<td>-0.06</td>
<td>0.57</td>
</tr>
<tr>
<td>Follow-up</td>
<td>11</td>
<td>-0.06</td>
<td>220</td>
<td>-0.03</td>
<td>-0.12</td>
</tr>
<tr>
<td>Overall</td>
<td>12</td>
<td>-0.02</td>
<td>379</td>
<td>-0.04</td>
<td>0.09</td>
</tr>
</tbody>
</table>

Table 5

*Adjusted Psi Scoring in Relation to Band 3 GMA by Gender for Both Studies Combined*

<table>
<thead>
<tr>
<th></th>
<th>High band 3</th>
<th>Low band 3</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n</td>
<td>TrDevAdj</td>
<td>n</td>
<td>TrDevAdj</td>
</tr>
<tr>
<td>Male</td>
<td>4</td>
<td>-0.35</td>
<td>135</td>
<td>-0.03</td>
</tr>
<tr>
<td>Female</td>
<td>8</td>
<td>0.14</td>
<td>244</td>
<td>-0.05</td>
</tr>
</tbody>
</table>

Band 3 GMA: Post Hoc Analyses

An alternative way of assessing the relationship between psi scoring and band 3 activity is to test for a correlation between these two variables. Spearman’s rank order correlations between power in band 3 and mean psi scores are shown in Table 6. Overall there was a slight negative correlation between psi scoring and band 3 activity. For comparison, the table also shows the correlations for neighbouring frequency bands 2 (0.1–0.2 Hz) and 4 (7–22 mHz), and the 3-hr global index of GMA (ap). Note that in the follow-up study, the correlation of the global GMA index with psi scoring was significant, $r_s(229) = -.13, p = .04,$ two-tailed. However, one should not attach much importance to this result considering that there have been multiple analyses, the global correlation of -.13 is close to the local band 3 correlation of .10, and in the preliminary study the global correlation was in the opposite direction. The global correlation of -.13 could have been essentially due to the local band 3 correlation, but just a little stronger due to random fluctuation. This does, however, align with earlier research correlating global indices of GMA with psi scoring, in which receptive psi tends to be stronger with low intensity levels of GMA.

Table 6

*Correlations Between Psi Scores and Band 3, Band 2, Band 4, and Global GMA (ap)*

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>$r_s$ (Band 3, TrDevAdj)</th>
<th>$p$ (two-tailed)</th>
<th>$r_s$ (Band 2, TrDevAdj)</th>
<th>$r_s$ (Band 4, TrDevAdj)</th>
<th>$r_s$ (ap, TrDevAdj)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preliminary</td>
<td>160</td>
<td>-.04</td>
<td>.60</td>
<td>-.03</td>
<td>.12</td>
<td>.12</td>
</tr>
<tr>
<td>Follow-up</td>
<td>231</td>
<td>-.10</td>
<td>.13</td>
<td>-.07</td>
<td>-.07</td>
<td>-.13</td>
</tr>
<tr>
<td>Overall</td>
<td>391</td>
<td>-.07</td>
<td>.15</td>
<td>-.05</td>
<td>.02</td>
<td>-.03</td>
</tr>
</tbody>
</table>
A smoothed plot of the adjusted psi scores vs. band 3 GMA for all sessions in both studies (Figure 4, top panel) suggests that psi scores were reduced when band 3 GMA values exceeded a threshold, marked by the solid vertical line. The 140 sessions to the right of this line gave a mean psi score of -0.16, compared to 0.02 for the remaining 251 sessions, which is a significant difference, $t(389) = 2.16, p = .03$, two-tailed. However, this threshold at which scoring dropped is at a lower band 3 activity level than that observed by Ryan (2008); this threshold (which informed our formal hypothesis) is marked with a solid vertical line in the bottom panel of Figure 4. This 2008 threshold is at the highest GMA level for this study, which emphasises how low the GMA was and the impossibility of testing the planned hypotheses. Interestingly, in a parallel project at Lancaster University, Ryan and Subbotsky (2010) studied local GMA and psi in 100 remote viewing trials and found a significant negative correlation of band 3 with psi effect size. The drop in effect size occurred at a similar threshold to this study; this is marked with a solid vertical line in the middle panel of Figure 4. Both this study and the Lancaster study were conducted during a period of lower GMA than Ryan (2008; see Figure 3), which perhaps could account for this difference. Perhaps a participant’s physiological response to GMA is dependent on relative rather than absolute levels of GMA—earlier studies have indicated that psi scores are related to a drop or rise in intensity rather than the absolute level.

The 38 sessions conducted during periods of very low GMA (to the left of the dashed vertical line in the top panel of Figure 4) had a mean psi score of 0.28, significantly higher than the remaining 353 sessions, whose mean psi score was -0.08, $t(389) = 2.59, p = .01$, two-tailed. A similar pattern was found in the Lancaster RV study (Ryan & Subbotsky, 2010): here trials to left of the dashed vertical line in the middle panel of Figure 3 scored significantly higher than the remaining trials, $t(98) = 2.51, p = .01$, two-tailed. However, this pattern is not visible for Ryan (2008; lower panel of Figure 4).

*Figure 4.* Smoothed mean psi score (TrDevAdj) by band 3 activity (grey line) and 95% confidence interval (grey ribbon). Top panel: Samye Ling (both studies combined); middle panel: 100 remote viewing trials conducted at Lancaster University; bottom panel: 343 free-response trials examined by Ryan (2008), on which the formal hypothesis was based. Solid vertical lines indicate GMA threshold above which psi scoring falls; dashed vertical lines indicate GMA threshold below which psi scoring increases. In Ryan (2008) only one threshold point was used.
The finding that receptive psi scoring increased with very low levels of GMA is consistent with many articles in the literature. However, because these findings are post hoc, collected under most unusual conditions with very few data points for high GMA, they should be treated with extreme caution. The strongest individual participant correlations between band 3 GMA and psi are shown in Figure 8.

**Band 1 GMA: Formal Hypothesis**

It was hypothesised that band 1 GMA would be positively correlated with psi scoring. However, as shown in Tables 7 and 8, there was a small and nonsignificant negative correlation between band 1 activity and psi scoring. This is possibly entirely attributable to the extremely low levels of GMA. The males show positive scoring in line with the hypothesis, which, considering their overall significant negative psi scoring, does give some support for the band 1 hypothesis.

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>( r_s ) (Band 1, TrDevAdj)</th>
<th>( p ) (one-tailed)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preliminary</td>
<td>160</td>
<td>-.06</td>
<td>.77</td>
</tr>
<tr>
<td>Follow-up</td>
<td>231</td>
<td>-.04</td>
<td>.72</td>
</tr>
<tr>
<td>Overall</td>
<td>391</td>
<td>-.05</td>
<td>.83</td>
</tr>
</tbody>
</table>

**Table 8**

<table>
<thead>
<tr>
<th></th>
<th>( n )</th>
<th>( r_s ) (Band 1, TrDevAdj)</th>
<th>( p ) (one-tailed)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male</td>
<td>139</td>
<td>.01</td>
<td>.45</td>
</tr>
<tr>
<td>Female</td>
<td>252</td>
<td>-.07</td>
<td>.88</td>
</tr>
</tbody>
</table>

**TLEQ**

As hypothesised, participants’ scores on the TLEQ were negatively correlated with the correlation between band 3 GMA and psi scoring, \( r(20) = -.34, p = .06, \) one-tailed (see Figure 5). This result is marginally significant, and is due primarily to the females, \( r(11) = -.45, p = .06, \) one-tailed, the males showing no correlation, \( r(7) = .24, p = .73, \) one-tailed. In other words, response in psi scoring to Band 3 activity was more pronounced for participants who scored highly on the TLEQ. One participant (S22) scored very highly on the TLEQ, her individual correlation being significant, \( r(14) = -.46, p = .05, \) two-tailed, and this outlier data point makes a large contribution to the correlation. With the outlier removed, the overall (males and females combined) correlation becomes nonsignificant, \( r(19) = -.19, p = .21, \) one-tailed.
The correlation between participants’ total scores on the TLEQ and the correlation between band 1 GMA and psi scoring was also nonsignificant, $r(20) = -.13$, $p = .72$, one-tailed. This is entirely to be expected considering the extremely low GMA and hence the absence of a correlation between band 1 activity and psi scoring.

**Longitudinal Analyses**

**Seasonality.** As shown in Table 9, the significant correlation between daily maximum temperature and psi scoring found in the preliminary study was not found in the follow-up study, or both studies combined.

<table>
<thead>
<tr>
<th></th>
<th>$r$ (MaxTemp, TrDev/Adj)</th>
<th>$N$</th>
<th>$p$ (one-tailed)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preliminary</td>
<td>.14</td>
<td>160</td>
<td>.04</td>
</tr>
<tr>
<td>Follow-up</td>
<td>.02</td>
<td>231</td>
<td>.41</td>
</tr>
<tr>
<td>Overall</td>
<td>.07</td>
<td>391</td>
<td>.09</td>
</tr>
</tbody>
</table>

Figure 6 shows psi scores by month for the whole 4 years of the study. Highest scores were achieved in May, but an ANOVA did not indicate a significant effect for month: $F(11, 380) = 1.07$, $p = .39$. The high scoring in May was entirely attributable to the preliminary study; scoring in May in the follow-up study was negative. Scoring from May until September was nonsignificant in the psi-hitting direction, whereas from October until April it was primarily in the psi-missing direction, most particularly in October and January. Thus, although not significant, there was a seasonal component as hypothesised, directionally supporting the Sturrock and Spottiswoode (2007) finding.
Local Geomagnetic Activity and Psychic Awareness

Figure 6. Adjusted Psi score by month, with 95% confidence interval (overall).

Lunar Phase. Figure 7 shows psi score by lunar phase for the overall study. No patterning by lunar phase is apparent, except for a slight tendency for scoring to rise after dark and full moons and to drop before them.

Figure 7. 12-point moving average of psi score by lunar phase (overall).

Discussion

This study was supposed to be a longitudinal study starting with GMA at the solar cycle minimum and continuing until the sunspot cycle was approaching its maximum. Instead, the sunspot activity was at its lowest in 100 years! Even at the time of completion of data collection the GMA had only just returned to the normal minimum
level. So the GMA data are completely unrepresentative of what would be expected during a normal solar cycle, and everything that follows should be taken with this consideration.

Although the formal hypotheses were not significantly supported, suggestive evidence was found that psi scoring was correlated with band 3 geomagnetic pulsations. This relationship comprises two parts: a drop in psi scoring at high levels of band 3 activity, and, in a second independent analysis, an increase in psi scoring at very low levels of band 3 activity. However, these findings should be treated with caution for the reason that the significant scoring was only found post hoc by changing the threshold levels for high and low band 3 from the hypothesised levels. To mitigate this concern, the threshold levels for the cut-off points for high and low levels of band 3 GMA were similar to those found in a parallel study at Lancaster University during the period of this study and so could be related to the unusually low levels of GMA. Also, the overall correlation between psi scoring and band 3 GMA strengthened slightly from \( r = .04 \) in the preliminary study to \( r = -.10 \) in the follow-up study, in line with the slight increase in GMA across this period. This suggests that with normal levels of GMA there could have been the expected significant correlations. If the hypothesis put forward by Ryan (2008) is correct, and the higher frequency band 1 geomagnetic fluctuations enhance psi, then it’s possible that the psi scoring in the psi-missing direction found in the preliminary study is related to the almost total lack of band 1 GMA. The psi scoring did improve slightly in the follow-up study as the GMA started to increase. In the dataset studied by Ryan (2008), for the 47 trials with low GMA (across all frequencies) psi was at chance level.

The variation in psi scoring with high or low band 3 GMA was positively correlated with TLEQ scores to a marginally significant degree. Neppe (1984), Persinger (1989), and Cook and Persinger (2001) have all suggested a relationship between psi type experiences and temporal lobe symptoms. Roll (1977) in his investigation of poltergeist cases found that many of the focal people suffered from epilepsy. The significant correlation in the present study was due mainly to one participant (S22) who had very high levels of scoring on the TLEQ (see Figure 5) and who had the third highest psi score overall. Her scoring was very variable with eight direct hits out of 19 sessions, where five would be expected by chance, and six rank-four misses. This variation in scoring may have been associated with GMA: as mentioned in the results section she showed a significant negative correlation of band 3 GMA and psi scores, \( p = .05 \), two-tailed, as shown in Figure 8. Five of the seven high band 3 activity sessions were psi-misses, whilst the two very low band 3 GMA sessions were psi-hits. Several other participants also showed this correlation with varying degrees of significance, as shown in Figure 8. The strongest correlations are shown here and three of these are from females (S8, S22, and S24). S8 and S24 also scored highly on the TLEQ.

With respect to the higher frequency band 1 pulsations, the hypothesis of a positive correlation between psi scoring and pulsation activity was not supported, although this could be due to the very low levels of GMA throughout the period of investigation. In general (i.e., considering all experimentation, not just this study), considering (a) the persistent negative correlation of psi effect size with band 3 activity, (b) the fact that band 3 is strongly correlated with the global index of GMA \( ap \), and (c) Spottiswoode’s (1997a) finding of a near zero correlation between GMA \( ap \) and psi effect size in a meta-analysis of 2,879 free response trials, an effect of another factor, such as band 1 activity, still seems likely. In other words, if there is, in general, a negative correlation between GMA and receptive psi, then this should be evident in a large database of trials such as Spottiswoode’s. But this is not the case, suggesting that there is a second factor that has a counterbalancing effect.

An important consideration regarding GMA and psi scoring is the unduly low levels of GMA shown in Figures 2 and 3. This is unprecedented in modern times and possibly could underlie the overall chance level psi scoring. Persinger (1989) remarked on how some decades are associated with very high levels of spontaneous psi reports and suggested that this may be related to decreased levels of GMA. It would, therefore, have been expected that this data would show high levels of psi, but this was not the case. More research is required!

It is possible that the effect of GMA on psi is due to changes in intensity. Many of the parapsychological studies of the effect of GMA on psi indicate that a decrease from an otherwise high level is related to increased receptive psi scoring; the days before and after the reported psi experience show higher GMA than the day of the experience. In this study, GMA was so low that there was very little change in either direction.
A seasonal component was once again evident though not at a significant level. Overall, psi scoring was highest in May. This was entirely attributable to the preliminary study; scoring in May in the follow-up study was negative. Scoring from May until September was nonsignificant in the psi-hitting direction, whereas from October until April it was primarily in the psi-missing direction, most particularly in October and January. Thus whilst not significant there was a seasonal component, supporting Sturrock and Spottiswoode’s (2007) finding. The weather at Samye Ling was dire throughout the summer of 2012 when most of the summer data were collected, as it was for most of Britain that year. It rained nearly every day! The weather at Samye Ling is amongst the most severe in Scotland, renowned for extreme rainfall and cold. In 2010 there were only 3 months which were frost free. In November 2009 the rainfall resulted in devastating floods in southwest Scotland. The river rose by 9 feet at Samye Ling and flooded the surrounding fields. Therefore, any sunshine and warmth that does occur at Samye Ling changes people’s mood in a noticeable way! Carpenter (2012) strongly advocates taking notice of mood variables, as research suggests that these affect psi scoring. Therefore, despite the lack of significance, this is a factor that could be considered in this study. It is recommended that researchers take note of seasonal and weather conditions pertaining to their experimental sessions. This might be a possible explanation for the low psi scoring during the summer months and for the overall negative psi scoring, or this could possibly be an experimenter effect.

Overall psi scoring was significantly different between the genders: males scored significantly below chance whilst females scored at chance. This difference between the genders was also found in research with yogis in an ashram setting in India (Roney-Dougal & Solfvin, 2006). Whether this is an experimenter effect, something to do with male meditators, or some other unspecified cause is open to discussion. One possible cause is that of culture. In unanalysed qualitative data from interviews at the end of sessions, some of the male participants mentioned negative

Figure 8. Individual participants’ correlation between band 3 GMA and psi score.
attitudes towards psi, partly from Western cultural belief systems and partly from Buddhist teachings at Samye Ling. The correlation, found in all four Indian studies, between years of practice of meditation and increasing psi scoring did not occur in this study. Instead, throughout the 4 years of data collection, the overall scoring was at chance and in the psi-missing direction for many of the participants, though there was an incline with slightly better scoring in the follow-up study. There are several possibilities as to why this is. One of the problems in parapsychology is that like other social sciences it is a very “soft” science with multiple variables affecting every session. One of these is culture. The attitude towards psi differs between Indian yogis, Tibetan Buddhist monks, and Western meditation practitioners. The negative attitude towards psi manifested by some people at Samye Ling was initially unexpected. Apparently, the Rinpoche who founded Samye Ling discouraged people from relating meditation practice to psi effects, encouraging them to focus on the psychological and emotional benefits, such as mindfulness and compassion. Most Indians and Tibetans accept psi as part and parcel of life, whereas in the West there are strongly conflicting belief systems. In Eastern tradition, one aims for enlightenment, compassion, and wisdom. Psi is not to be aimed for but neither is it something to be shunned, and most Tibetan monasteries have a Lama who practices divination (for more details see Roney-Dougal, 2006). Conversely, many Westerners have deep and complicated fears and resistance towards exhibiting psi at a conscious level (Tart, 1984). Qualitative data for this factor is present in the interviews conducted after the completion of the sessions, and some quantitative data will be extracted from these in the future.

The psi scoring in clairvoyance and precognition sessions was almost identical. Three studies using the PreCOG methodology have found no difference between clairvoyance and precognition sessions, which suggests that receptive psi is a unitary phenomenon, the differences found between the two in previous parapsychological research being a psychological one. Whilst in this study the overall psi was non-significant, the males did score significantly albeit in a negative direction. In the Tibetan research (Roney-Dougal & Solfvin, 2011), again overall the scoring was non-significant, but one group (the Lamas) did score significantly and again there was no difference in scoring between the two types of session. In no case has there been a formal hypothesis; this is merely an interesting outcome from the PreCOG programme, and is mentioned here because it could lead to an interesting line of research. This suggestion of receptive psi as a unitary phenomenon is a reasonable one because neither participant nor experimenter knew whether the session was running in clairvoyance mode or precognition mode; that information was only accessed during the analysis. And despite the overall gender difference there was no difference for males or females in this respect.

Conclusion

These data form a unique set of measurements covering as it does a 4-year period when GMA was at an unprecedentedly low level of activity, rather than following the normal 11-year solar cycle. By the preplanned analysis, there was little difference between sessions conducted during periods with high and low band 3 activity, but post hoc, using a lower high/low threshold, a significant difference was observed. As these analyses are post hoc, they should be treated with caution. However, the GMA threshold at which scores dropped was close to that observed in a recent study of local GMA conducted at Lancaster University (Ryan & Subbotsky, 2010). Because the inverse relationship between band 3 activity and psi corroborates previous research it can be cautiously concluded that there was a band 3 effect in this data. The negative correlation found in the 2008 data which informed the hypotheses was similar to that found in this study.

There was no indication of an enhancement of psi scoring during periods of high band 1 activity. This may be attributable to the overall low levels of GMA during the study.

Overall, males scored significantly negatively, whereas females scored at chance levels; the difference was significant but this is a post hoc analysis and so is only suggestive. Researchers can learn from studies with negative results and it is important that negative results are reported. The prime consideration in this study was the effect of a moderating environmental variable on psi, so it was to be expected that sometimes there would be psi hitting, and sometimes no psi, and sometimes psi missing.

The exploratory hypothesis that participants with high scores on the TLEQ questionnaire would respond most strongly to GMA was marginally significant. However this result was due mainly to one outlier, and so needs further corroboration.
A second exploratory hypothesis concerning the effect of season on psi did show some correlation with that found by Sturrock and Spottiswoode (2007), but not to a significant degree. This could be attributable to the overall lower level of psi scoring and to the unseasonably bad weather at Samye Ling.

Despite the lack of significance, further work with local GMA measurement would seem warranted as an avenue for future research once GMA levels have returned to normal. Additionally, further work on individual differences in responses to GMA would be useful. It is recommended that researchers consider and report the phase in the solar/GMA cycle at which their study was conducted, and we would further urge researchers to record the date, time, and location of trials, and to make this data publicly available so that retrospective studies of the effect of environmental variables can be performed.

References


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Local Geomagnetic Activity and Psychic Awareness

Abstracts in Other Languages

Spanish

LA RELACIÓN ENTRE LA ACTIVIDAD GEOMAGN´ETICA LOCAL Y LA CONSCIENCIA PS´IQUICA

RESUMEN: Hay evidencia de que las experiencias psíquicas están relacionados con la actividad geomagnética (GMA). Sin embargo, a veces éstas se relacionan con mayor y a veces con menor GMA. Esta anomalía puede ser aclarada estudiando la GMA local. Para explorar esto, llevamos a cabo una investigación durante 4 años. Por desgracia, la actividad solar y la GMA estuvieron en su nivel más bajo desde hace 100 años, por lo que fue necesario combinar datos preliminares y de seguimiento en el estudio. Ambos estudios utilizaron el mismo diseño de respuestas libres. Los 26 participantes, todos ellos residían en o cerca del Centro Tibetano Samye Ling, habían practicado meditación cuando menos 10 años. Todos los participante completaron un mínimo de ocho sesiones cada año, así como tres cuestionarios y un examen psicológico. Las mediciones geomagnéticas fueron facilitadas por el observatorio del Servicio Geológico Británico ubicado cerca de Samye Ling. No hubo diferencias entre las sesiones llevadas a cabo durante la banda 1 alta o baja, o la banda 3 alta o baja pero análisis post-hoc sugieren un efecto de la banda 3 de GMA. En general, las puntuaciones psi de los hombres fueron significativamente negativas ($p = 0.02$, dos colas), mientras que las mujeres puntuaron al azar, con una diferencia significativa ($p = 0.03$). Los participantes con las puntuaciones más altas en el cuestionario de lóbulo temporal mostraron la correlación más fuerte de psi con GMA ($p = 0.06$).

French

LA RELATION ENTRE L’ACTIVITE GEOMAGNETIQUE LOCALE ET LA PERCEPTION PARANORMALE

RESUME : Il y a des preuves que les expériences paranormales sont liées à l’activité géomagnétique (GMA). Toutefois, elles sont parfois associées avec une GMA plus élevée, et parfois avec une GMA plus basse. Cette anomalie pourrait être clarifiée par l’étude de la GMA locale. Afin d’explorer cet aspect, une étude fut conduite sur 4 années. Malheureusement, l’activité solaire et le GMA furent à leur plus bas niveau depuis 100 ans durant cette période, si bien qu’il fut nécessaire de combiner les données préliminaires et postérieures à l’étude. Ces deux études ont utilisé le même protocole à réaction libre. Les 26 participants, résidant tous près du Centre tibétain Samye Ling, ont pratiqué la méditation depuis au moins 10 ans. Chaque participant a effectué un minimum de huit sessions chaque année. Trois questionnaires et un test psychologique furent complétés. Les mesures géomagnétiques furent effectuées par l’observatoire du British Geological Survey localisé aux environs de Samye Ling. Il n’y eut pas de différences entre les sessions conduites durant la GMA de bande 1 haute ou basse, ou la GMA de bande 3 haute ou basse ; mais des analyses post-hoc suggèrent un effet du GMA de bande 3. Globalement, les scores psi des mâles étaient significativement négatifs ($p = .02$, two-tailed), tandis que les femelles eurent des scores au niveau du hasard ; la différence fut significative ($p = .03$). Les participants avec les scores les plus élevés au questionnaire sur le lobe temporal montrèrent la plus forte corrélation entre score psi et GMA ($p = .06$).

German

DIE BEZIEHUNG ZWISCHEN LOKALER GEOMAGNETISCHER AKTIVITÄT UND PARAPSYCHISCHER ERFahrung

ZUSAMMENFASSUNG: Es gibt Hinweise, dass parapsychische Erfahrungen mit geomagnetischer Aktivität (GMA) zusammenhängen. Diese treten jedoch manchmal mit höheren, manchmal mit niedrigen GMA-Frequenzen auf. Diese Anomalie könnte durch die Untersuchung der lokalen GMA geklärt werden. Um dies herauszufinden, wurde eine vierjährige Untersuchung durchgeführt. Unglücklicherweise befanden sich die Sonnenaktivität und die GMA auf ihrem tiefsten Punkt seit hundert Jahren, so dass es unumgänglich wurde, die Daten der vorläufigen und

Appendix

Table of Participants’ Psi Scores

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BOOK REVIEWS


Advances in Parapsychological Research 9 is a timely collection of cutting-edge thought and research in the field of parapsychology. This is the first Advances published since 1997, and the editors assert that there have been a number of advances since the previous edition. This is the case despite a lack of mainstream acceptance of the subject matter, and sometimes accusations of fraud and shoddy methodology (e.g., in the responses to Bem’s publication of results supportive of the psi hypothesis in a mainstream journal).

This collection is of value to the esteemed parapsychologist as much as the newcomer considering the current state of play of this field. The book includes an introductory chapter on science and psi by the editors, reflections on the contributions of Jule Eisenbud by Stephen Braude, a discussion of the global consciousness project by Roger Nelson, the value of research on ESP in dreams by Simon Sherwood and Chris Roe, a discussion of Thalbourne’s theory of psychopraxia by Lance Storm, a consideration of various statistical means of interpreting the outcomes of the ganzfeld database by Patrizio Tressoldi, further understanding the relationship between psi and altered states of consciousness by Adam Rock and colleagues, the value of unconscious measures of psi by Ed Modestino, how understanding from energetic traditions might impact this field by David Feinstein, and William Braud’s thoughts on how parapsychology might develop as it moves forward.

In their introductory chapter, the editors examine the state of play of parapsychology as a science and note that the findings in the field represent a threat to the dominant paradigm—potentially a Kuhnian crisis state in terms of scientific revolution. The contents of this volume certainly attest to progression in the field in terms of findings, theory, and approaches toward the subject matter. The lack of mainstream acceptance is a common but valid lament of the parapsychologist. Throughout parapsychology’s history there are frequent suggestions of “error some place,” whereas, in contrast, the history of parapsychology actually reflects a strong dedication to research methods (and statistics). It is of note that many methods that are commonplace in psychology were pioneered in parapsychology. The question still seems to be what constitutes sufficient evidence to change the mainstream perspective. The editors note that there are a number of other problems within parapsychology that hinder its acceptability. These include the so-called source of psi problem, the problems associated with the term “psi” (its negative definition as what remains when we rule out all normal explanations), the small effect sizes, and what they term the “slippery” nature of psi; these render it difficult to fully understand which factors truly correlate with psi. The editors suggest that only when these problems are overcome will there be a transition from a Kuhnian crisis to a solution stage. This author considers that some of these issues are more problematic than are others. For example, psi may be better considered as a property of a system (e.g., von Lucadou’s models as applied to RSPK) or recipe; according to one recent model of psi (Jim Carpenter’s first sight model), we would expect the source of psi to be associated with the person for whom it has the most meaning. Equally, the effect sizes may well be larger under certain circumstances. Many of these observations refer to parapsychology as defined according to the natural sciences. In this volume, William Braud considers that “science” should be expanded beyond the restrictive approaches that have perhaps hindered this field throughout its history.

The collection begins by alerting the reader to the contribution of Jule Eisenbud to this field. His story is intriguing and made this author want to go and read some of his early work on psi and psychoanalysis. Eisenbud is a key name in psychoanalysis, psychiatry, and parapsychology. He is also very well known for his research with Ted Serios, a psychic claimant who was tested by Eisenbud for his claims of thoughtography (thoughts being psychically represented on photographic film). Braude describes Eisenbud as a great thinker, pioneer, and friend. He notes the intriguing differences between the public story of the interchange between Randi and Eisenbud and how Randi’s claims were never actually executed in terms of being able to replicate the thoughtography experiments using normal means. These experiments have often been written off as the work of clever sleight-of-hand
(for example, Serios often required the presence of a “gizmo,” a hollow tube to enable his psychic performance). However, Braude notes Eisenbud’s commitment to engaging in attempts to replicate Serios’s performances and various controls applied to this case (including the use of a Polaroid camera, which would prevent any interference using sleight-of-hand, as the photographs would be immediately produced without the possibility of interference). Many of these controls and replications render a completely normal explanation very difficult. Braude writes that the letters between Eisenbud and Randi and many of the thoughtography photographs are currently on display at the library of the University of Maryland, Baltimore County.

The global consciousness project is an intriguing project which is in its 14th year of operation. The project consists of globally placed “EGGs” or electrogaiaigrams (named for an earlier incarnation of this project). These consist of random event generators (RNGs), which emit truly random streams of numbers. These are placed around the world to explore correlations between global events and deviations from normality in the structure of the randomness of the data of the RNGs. The data indicate that there is structure in the data that correlates with a series of major world events. Indeed, the patterns reflect “structure … in the random data more than it should” (p. 32) and the overall effect size has odds against chance of less than one in a billion. This nonrandomness cannot be attributed to electrical grid stresses, mobile phone activity, or electromagnetic fields. These effects also behave differently than selected moments of nonrandomness within a random string of numbers from a random system. Spatial and temporal analyses support meaningful structure in the data that is not consistent with these effects resulting from an experimenter effect. Interestingly, there also seems to be a relationship with shared perceptions and emotional states, in particular when there is a high level of compassion.

Nelson argues in favor of a consciousness-related explanation for the structure in the data collected to date and is sympathetic to field-like models of consciousness. The chapter ends with discussion of the implications of these data, which Nelson argues are suggestive of a universal mind, the idea that there are subtle linkages between people. It is difficult to know what is really going on. These data are certainly very intriguing, but one always needs to exert caution when proposing causality with correlational data.

This volume also includes an updated version of Sherwood and Roe’s excellent evaluation of the dream ESP databases. This version includes articles published since the time of the publication of the original manuscript. The chapter begins by noting how the ganzfeld became the successor to the dream studies, but it is not clear to what extent the ganzfeld constitutes a change of state per se. The authors argue that the nocturnal dream state is a better-defined altered state of consciousness, which has a clear and strong association with subjective paranormal experiences throughout the literature. The work presents a critical evaluation of the Maimonides dream studies, including a summary of the specific nature of the research methodologies across the series, and the aspects of the methodology that may be worthy of further thought for future study in parapsychology. The authors cite Markwick and Beloff, who note that dreams may be the “royal road to psi” and that this state is a fruitful one for researchers to explore for further understanding psi. Sherwood and Roe present a comparison between the Maimonides dream studies and the post-Maimonides dream studies and note that both databases result in a deviation from chance supportive of the psi hypothesis. However, they note various criticisms applied to the original series, including the lack of replications of the exact Maimonides methodology. The post-Maimonides studies are characterized by a significantly lower effect size overall (albeit supportive of a nonchance effect). These studies have been, for the most part, carried out in the homes of the participants, partially from a need for ecological validity and partially due to economy. The costs associated with these benefits appear to be a reduction in the psi-conduciveness. For example, the authors suggest that one of the key elements may be that there is a reduction in rich dream imagery in the home environment, as it is less likely that people have access to as much of their dream imagery when they awaken naturally as when they are awoken in the middle of a REM period. They note that dream studies are a neglected but promising paradigm that warrants future replications. They also offer a few suggestions for optimizing psi performance and balancing ecological validity with economy, and maximizing what can be learned from the original Maimonides studies.

An interesting inclusion in this volume is Lance Storm’s chapter, in which he compares Thalbourne’s theory of psychopraxia to two similar theories: Jung’s theory of synchronicity and Stanford’s psi-mediated instrumental response (PMIR) theory. He notes that psychopraxia has received little consideration within academic parapsychology, perhaps because the 2004 monograph has yet to be read by a sufficient number of scholars. The inclusion of this chapter thus serves to draw attention to this model, and encourages the reader to explore its theoretical predictions. “Psychopraxia” derives from the terms “psyche” (meaning soul, mind, or self) and “praxis” (meaning to accomplish...
or bring about). It is a valid attempt to unify mainstream psychology with parapsychology. In addition, the theory fuses the concepts of ESP and PK and argues that they are a unitary phenomenon. In the model, action occurs either endosomatically (in the body) or exosomatically (outside the body). Within the model, it is understood that the self is the common denominator of all experience and the source of action. Action will occur when there is a pro attitude, which can be a goal, wish, desire, intention, preference, or need, and it can be conscious or unconscious. There is a hierarchy of pro attitudes such that the one with dominance is the one that wins, and then the self adopts this attitude. The action will emerge. Storm discusses the similarities and differences between the model of psychopraxia and synchronicity and PMIR, and he suggests that psychopraxia is a valuable addition to parapsychology. He notes that the theory is ontologically neutral in terms of a model of the mind, it may provide a simplified understanding of psi, it is a more accurate description of psi, and it provides testable hypotheses for future research in this field. The existence of good theories with testable hypotheses may also help to strengthen the acceptability of the field to the mainstream.

The ganzfeld has long been touted as the flagship paradigm in parapsychology. This is intriguingly the case despite a lack of recent research using this methodology. The vast database which is in existence is a good resource for the application of statistical techniques and conceptual thinking concerning psi (in terms of both proof and process orientations). Tressoldi’s chapter presents a clear summary of various approaches toward understanding the experimental outcomes. It appears to be the case that there is an effect (different from chance) irrespective of the way in which one ascertains experimental outcomes. For example, there is an effect that is different than what one might expect by chance when one applies a mainstream hypothesis-testing approach. Similarly, Bayesian analysis suggests greater evidence in favor of the experimental hypothesis (that there is an anomaly) than the alternative model of the null hypothesis. Tressoldi notes that Bayesian approaches include some subjectivity in determining the prior probability and cites Utts’s intriguing work whereby four different “priors” are compared (no prior information, an open-minded type of prior, a psi-believer prior, and a skeptical prior). Other Bayesian analyses suggest that there is a psi outcome that is not zero, even with an extremely conservative prior probability level. Additionally, the outcome is not just a substantial effect, but rather it is “extreme,” because the value vastly exceeds the cutoff value for this level of effect. A third means of analyzing this database, via a new method of quantum modeling, also indicates a pattern supportive of the psi hypothesis for this database. Tressoldi concludes his chapter by noting the disparity between statistical findings and the lack of human acceptance of those findings. He also reminds the reader that these studies (and other work which finds that these studies are far superior to studies in the waking state and with forced-choice designs) are intriguing, and that altered states of consciousness are conducive to psi phenomena.

This theme is continued in the next chapter, in which Rock, Friedman, and Jamieson discuss the nature of the relationship between psi and altered states of consciousness (or altered states of phenomenology, as an alternative suggestion by the lead author with Krippner), with a focus on the ganzfeld state. Their chapter focuses on the possibility of applying Pekala’s Phenomenology of Consciousness Inventory (PCI) to the ganzfeld state to further understand which dimensions of consciousness may best predict psi performance. This would constitute a timely advance in understanding the specific aspects of “the ganzfeld state” that are more closely associated with psi.

A very promising research area in parapsychology is that psi may be better detected by unconscious measures. Edward Justin Modestino discusses physiological responses as unconscious psi. The idea of unconscious psi is not a new one in parapsychology; indeed, it is present in the writings of James, Myers, and Huxley. Here, psi information is present in the unconscious (the body) but is filtered from, or not accessible to, conscious awareness. Interestingly, this is also a major tenet of Carpenter’s first sight model, that is, that psi processes are inherently unconscious. In Huxley’s terminology, the brain serves as a reducing valve that prevents an overload of information from the larger mind (information held collectively). Modestino presents a review of research that has explored psi using a range of physiological measures. The review includes discussion of the presentiment effect (the observation that the body and brain respond prior to exposure to a stimulus), correlates of precognitive priming, and unconscious measurements of remote stimulation (i.e., studies which explore the physiology of a person who is in a different location than the target stimulus). Research findings indicate that there is clear evidence for psi using an array of measures of both the autonomic nervous system and the central nervous system. Such anomalies cannot be accounted for by response biases or artifacts and are another challenge for mainstream neuroscientists. Modestino also notes that this problem is compounded by the lack of a theory that might explain these anomalies.

In the penultimate chapter, David Feinstein discusses energy psychology—the study of physical and psychological changes that appear to occur following the application of procedures that are claimed to influence the
body’s “energies” or “energy fields.” These include tapping, thought field therapy, and acupuncture, among others. In this chapter, Feinstein focuses on “surrogate tapping” in particular. This is a method whereby the therapist acts as a surrogate for a client and engages in tapping certain acupoints while holding an intention for change (physical or emotional) in a client who may or may not be physically present. Intriguingly, this appears to facilitate changes in clients, irrespective of whether they are physically present or consciously know about the treatment. These findings are difficult to accommodate within normal models of mind-body interactions. Such models include the idea that acupoint pressure may be associated with reductions in levels of the stress hormone cortisol, which in turn influences brainwaves, the production of serotonin, and opioids, which have somatic effects. Feinstein himself has been an advocate of an amygdala deactivation model, which proposes that stimulating acupoints leads to deactivation of the activity of the amygdala and results in a reduction of threat arousal. The nonlocal observations in surrogate tapping are consistent with data on distant healing and DMILS in parapsychology and provide support for an anomalous process of information transfer, or psi.

In the final chapter, William Braud posthumously challenges the current discipline of parapsychology to expand its scope beyond the natural-science-inherited restrictions it has placed on itself. He notes the difference between two forms of parapsychology: parapsychologia versus parapsycholosophia: parapsychologia refers to a parapsychology of knowledge, or parapsychology as driven by natural-science-informed approaches, whereas parapsycholosophia refers to a complementary, more holistic approach toward knowledge, or a parapsychology of wisdom. Braud is a clear advocate for the inclusion of the latter. The chapter is a clearly written wakeup call for those working in this area, drawing attention to many ways in which certain ways of knowing and ways of doing research have been privileged throughout the history of the field. Braud proposes a conscious naming of eight such guiding assumptions that have (unconsciously) guided mainstream parapsychology to date. These are (a) the privileging of physical and material measures and domains, (b) nomothetic approaches toward knowledge, (c) so-called masculine approaches, (d) reductionist perspectives, (e) the need for psi to have some application, (f) the need for researchers to be objective and uninvolved, (g) restrictions in terms of what qualifies as science and the subject matter of that science and privilege in terms of what constitutes authority, and (h) the neglect of those people and approaches that do not have such “authority” in contributing knowledge and deciding what constitutes knowledge. By naming these assumptions, Braud proposes to stimulate some self-reflection with regards to how parapsychology wants to consciously proceed from now. Braud proposes a shift toward a more humane and experiential way of doing research in this area, which would include related phenomena of mystical and spiritual experiences, a shift toward more qualitative ways of doing research, and so forth. He proposes that there be inclusion of subjective experiences, idiographic or case study approaches, naturalistic and spontaneous events, and deep description and other forms of explanation (both applications and appreciations); the involvement of the investigator’s perspective and experiences within the field of study (thus embracing subjectivity); exploration of the “dark places” in parapsychology—where psi is not, including the nature of psi-missing and the nature of subjective experiences, including the meaningfulness of these experiences, and the patterns surrounding psi performance and experiences that are not predicted and that do not necessarily impact the specified target event itself. Finally, Braud advocates a democratizing of research, thus succeeding in de-emphasizing “authoritarian and hierarchical structures in our field and in general” (p. 207). This includes research using approaches and materials that are available to members of the public and experiencers. By expanding parapsychology in this way, there may be wider scope for discovery and deeper understanding of these fascinating phenomena.

I would thoroughly recommend this book. As a selection of the current state of play in this field, I think these chapters are both informative and inspiring. It reminded me that this field is necessarily self-reflexive, self-critical, and constantly evolving.

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Experimental psychologist Douglas Stokes tells us in his preface that his book “explores conceptions of the soul and the afterlife that are consistent with the findings of modern science.” He discusses many different conceptions of the afterlife, with the goal of determining which may be actually true.

I found much in this book that is admirable. The author demonstrates vast erudition, discussing relevant topics as diverse as cosmology, quantum mechanics, parapsychology, philosophy of mind, and the world’s religions. Stokes makes it clear from the outset that he rejects the doctrine of eliminative materialism:

Modern scientists and philosophers appear to be finally abandoning the untenable position of radical, eliminative materialism like proverbial rats leaving a sinking ship…. However, some modern scientists and philosophers, such as Daniel Dennett, Susan Blackmore, Thomas Metzinger, Paul Churchland, his wife Patricia Churchland, and Bruce Hood, deny the very existence of continuing selves, or Cartesian theaters, as these self-proclaimed “skeptics” disparagingly call them. (Perhaps these writers are not conscious beings, which might explain the quality of their “thought” processes.) . . .

To most people the existence of a continuing self is immediately given and cannot be doubted. Any theory that denies the existence of any centers of consciousness is quite simply wrong. (pp. 5, 20)

Stokes also displays a good grasp of modern physics, noting that: “Since the emergence of quantum mechanics early in the last century, the brain is no longer viewed as a deterministic system” and seemingly agrees with the implication that “only indeterministic systems are associated with consciousness, as conscious minds would be of no use to a mechanistic system” (p. 63).

He correctly describes how modern physics has had little impact on the thinking of materialistic philosophers such as those mentioned above, and he shows historical insight, noting that “The tenacity with which some scientists resist the idea of an autonomous realm of mind is perhaps understandable in the light of history…. Any mention of an immaterial soul may give rise to fears in many scientists of a descent back into religious irrationalism (and a consequent lack of funding)” (p. 47).

Stokes states in his introduction that the central arguments of his book “will not depend on parapsychological evidence such as hauntings, claimed memories of previous lives, and ostensible messages from the dead provided by mediums or in dreams, as these findings are not accepted by mainstream scientists” (p. 5). Claims similar to this—“most scientists are skeptical,” “rejected by mainstream science,” etc.—are made repeatedly throughout this book (I counted eight times) as an excuse to dismiss what seems to me as the most relevant evidence that could be brought to bear on the question of survival, and they lend a curious conformist timidity to the author’s writing. I will have more to say about this later.

Instead, Stokes reviews materialism, idealism, and dualism, considers them briefly in terms of physics and neuroscience, and settles on the doctrine of panpsychism—the idea that consciousness pervades the universe and may be possessed by entities as tiny as an electron:

Panpsychism finesse the intractable philosophical problem of accounting for how consciousness could arise from insensate matter. It didn’t. It was there all along. It was there at the Creation (i.e. Big Bang) and perhaps even before that (as part of whatever collective mind or agent set up the current laws of physics and then somehow caused an explosion to make them so). (p. 64)

But Stokes rejects the idea of personal survival:

A mountain of evidence amassed by neuroscientists over the past few decades demonstrates the fundamental dependence of memories and personality traits on the state of the physical brain. In view of these findings, it is unlikely that the mind would be able to survive death with its memories and personality intact. It is more likely that the mind survives death as a center of pure consciousness. (p. 6)
As mentioned, this book covers a wide variety of topics, discussing physics one moment, biology, philosophy or religion the next. And therein lies the problem with this book: Many of the subjects—including those which in my opinion are most central to the issues—are given only a brief, superficial examination. Evidence is dismissed without a fair hearing, and conclusions are arrived at prematurely.

Memories, Personality, and the Brain

At six points in the book Stokes repeats claims to the effect that memory and personality are intimately associated with brain activity, and so the conclusion is drawn that they cannot exist without such activity. No one doubts that the activity of mind is intimately associated with the activity of the brain, but correlation is not causation, and so the real question is the nature of such association—that is, whether the relationship is one of production or of transmission-reception, and of filtering.

Stokes also states in several places that the association between states of mind and brain is due to recent advances in the field of neuroscience. But in fact, except for the appeals of modern writers to the terminology of neuroscience, the arguments advanced in favor of the dependence of the mental on the physical—such as the effects of disease and brain damage on mental activity, and that memories seem to be stored in the brain—are essentially the same as those advanced 20 centuries ago by the Roman poet Lucretius.

But does the evidence really indicate that memories are stored in the brain? Neuroscientists have tried for decades to locate the sites of memory traces within the brain. The usual process has been to train animals to perform some task and then cut out parts of their brains to find out where the memories are stored. But even after large chunks of their brains have been removed—in some experiments up to 60%—the unfortunate animals can often remember what they were trained to do. Even experiments on invertebrates such as the octopus have failed to locate specific memory traces, leading one researcher to conclude that “memory seems to be both everywhere and nowhere in particular” (Boycott, 1965, p. 44).

There is, however, much evidence that changes can occur in the brains of animals as a consequence of learning. This consideration has been used in an experiment with chicks in an attempt to localize memory traces in the brain laid down during the learning process. Nerve cells in a particular region of the brain showed greater growth and development in chicks that had learned to perform a simple task, but when the brain region associated with the learning process was removed a day after they were trained, the chicks could still remember what they had learned. The cells that had experienced greater growth and development during the learning process were not necessary for the memory retention. The hypothetical memory traces remain elusive. (Sheldrake, 1988, p. 165)

The utter failure to find the memory traces in the brain has led to the untestable hypothesis that memory is stored “both everywhere and nowhere in particular.” Indeed, this hypothesis was invented to explain the failure, as under the conventional assumption memories must be stored in the brain. But in the absence of direct evidence, it remains more faith than fact.

But there is another possibility: Memories may not be stored in the brain, any more than TV shows are stored in the components of your TV. Biologist Rupert Sheldrake writes:

But what about the fact that memories can be lost as a result of brain damage? Some types of damage in specific areas of the brain can result in specific kinds of impairment: for example, the loss of the ability to recognize faces after damage to the secondary visual cortex of the right hemisphere. A sufferer may fail to recognize the faces even of his wife and children, even though he can still recognize them by their voices and in other ways. Does this not prove that the relevant memories were stored inside the damaged tissues? By no means. Think again of the TV analogy. Damage to some parts of the circuitry can lead to loss or distortion of picture; damage to other parts can make the set lose the ability to produce sound; damage to the tuning circuit can lead to loss of the ability to receive one or more channels. But this does not prove that the pictures, sounds, and entire programs are stored inside the damaged components. (Sheldrake, 1991, p. 116)

This interpretation makes it much easier to understand the fact that lost abilities often return; patients often recover partially or completely from brain damage even though the damaged regions of the brain do not regenerate.
The appropriate patterns of activity come into operation somewhere else in the brain. This is almost impossible to understand if programs are “hard-wired” into the nervous system; but memories may be stored in fields, which can move their regions of activity and reorganize themselves in a way that fixed material structures cannot.

Hans-Lukas Teuber, a cognitive psychologist who has extensively analyzed brain damage in war veterans, writes that “one is struck, before anything else, with the enormous resiliency of cerebral functions in the majority of instances. This far-reaching restitution of function remains, in my view, essentially unexplained” (Teuber, 1975, p. 160).

This view is echoed by neuroscientist E. R. John, who writes that in general, after traumatic head injury, “memories and skills return at a rapid rate during the first six months, with recovery sustained at a lower rate for up to four months. Defects in cognitive functions caused by brain injury due to penetrating wounds are characterized by an enormous resiliency of function in the great majority of cases, ultimately leading to little or no detectable defect” (John, 1982, p. 251).

So, contrary to what Stokes would have us believe, it is by no means clear from the evidence that memories are stored in the brain and so must be permanently lost in the absence of a properly functioning brain. It should be clear from the above that a consideration of only the neurophysiological evidence leaves us at an impasse with regard to the question of whether consciousness, complete with memories and personality, continues to exist after the death of the material brain. The question can only be resolved in a rational manner by a consideration of other forms of evidence.

Other Lines of Evidence

Stokes would no doubt dismiss the possibility that memories are not stored in the brain on the grounds that such a possibility is “not accepted by mainstream scientists.” He provides an almost infuriatingly shallow treatment of the evidence for personal survival from near-death experiences (NDEs) and mediumship and then uses this same excuse to dismiss such evidence.

The NDE provides evidence that the mind, complete with memories, may exist in the absence of a properly functioning brain. Experiencers frequently report not only normal, but also enhanced consciousness and perception, often seemingly occurring at times during which there is every medical reason to believe that their brains were either severely impaired or entirely nonfunctioning. Yet Stokes devotes a mere 2 ½ pages to the NDE, casually concluding that “Various neurophysiological causes for such hallucinations have been proposed, including seizures to the temporal lobes of the brain, lack of oxygen to the brain, the release of endorphins in the brain, and the random firing of cells in the visual cortex” (pp. 120–121). Yet I wrote a 300-page book on the NDE (Carter, 2010b), in which I examined all proposed counterexplanations and concluded that not a single one stood up to detailed and critical examination.

With lack of evidence for any other theories for NDE, the thus far assumed, but never proven, concept that consciousness and memories are localised in the brain should be discussed. How could a clear consciousness outside one’s body be experienced at the moment that the brain no longer functions during a period of clinical death with flat EEG? Also, in cardiac arrest the EEG usually becomes flat in most cases within about 10 s from onset of syncope [fainting]. Furthermore, blind people have described veridical perception during out-of-body experiences at the time of this experience. NDE pushes at the limits of medical ideas about the range of human consciousness and the mind-brain relation. (van Lommel, van Wees, Meyers, & Elfferich, 2001, p. 2044)

Stokes devotes three pages to mental mediumship, before dismissing it all on the grounds that “No mainstream scientists and even very few parapsychologists take mental mediumship seriously… due to the counterexplanations of sensory cues, fraud, ‘cold reading’ techniques, and the possible ability of mediums to gather information
about deceased persons through psi” (p. 129). However, I wrote a 369-page book on this topic (Carter, 2010a) and discovered that this is simply not true: Many “mainstream scientists” past and present have taken mental mediumship very seriously, from the renowned physicist Sir Oliver Lodge to the contemporary Nobel laureate Brian Josephson—unless of course Stokes simply classifies any scientist who takes mediumship seriously as “not mainstream” by definition! And, in my book I carefully considered all of the above counterexplanations, and I found that in the best cases not a single one stood up to detailed and critical examination.

Again, this is not the proper forum for a detailed review of the evidence, and so I will only briefly mention one case. It was investigated by the ruthless skeptic Richard Hodgson and concerned the mental medium Leonora Piper of Boston. Hodgson had her trailed by detectives and found nothing suspicious, and she was brought to London, where she knew no one, to be examined, and she performed as well as in Boston.

The case that convinced Hodgson of communication between the living and deceased concerned his friend George Pellew, a friend of Hodgson who died suddenly at age 32. Pellew gave every indication of communicating directly through Mrs. Piper’s voice, and sometimes through automatic writing. Out of 150 sitters who attended séances with Piper during that time, he recognized by name 29 of the 30 that Pellew had known in life (the sole exception was a young woman who had been a child when the living Pellew had last seen her). He conversed with each of these individuals in the appropriate manner and showed an intimate knowledge of his supposed past relationships with them.

Hodgson concluded:

> The continual manifestation of this personality—so different from Phinuit or other communicators—with its own reservoir of memories, with its swift appreciation of any reference to friends of GP, with its “give and take” in little incidental conversations with myself, has helped largely in producing a conviction of the actual presence of the GP personality, which it would be quite impossible to impart by any mere enumeration of verifiable statements. (Hodgson, 1897–1898, p. 328)

There is simply no rational justification to dismiss evidence such as this, merely because it is not accepted by “mainstream science” —whatever that means. The history of science is full of phenomena that were rejected by mainstream scientists out of prejudice or ignorance, including meteorites, continental drift, and the prevention of infection by the washing of hands before surgery. Indeed, science has on many occasions progressed precisely due to the questioning of mainstream beliefs. If science is to be a rational enterprise, then arguments should stand or fall on their own merits, and considerations of fashion and ideology should play no role in the evaluation of evidence.

### The Transmission Model

After briefly discussing then summarily dismissing the empirical evidence in support of personal survival, Stokes devotes a single page to the transmission model of mind-brain interaction—the idea that the brain works as a two-way receiver-transmitter between mind and body. This idea has been supported and endorsed by a variety of scientists and philosophers, including William James, Karl Popper, physicists Evan Harris Walker and Henry Stapp, neuroscientists Gary Schwartz and Mario Beauregard, and Nobel Laureate Sir John Eccles, the latter describing the two-way interaction between brain and mind with “brain receiving from conscious mind in a willed action and in turn transmitting to mind in a conscious experience” (Eccles, 1970, p. 58). Stokes, however, will have none of this, and writes:

> While this “transmission” model may be entertained through philosophical gymnastics, so too can the theory that the brain does not even exist. However, when one objectively (or even intuitively) considers the preponderance of the empirical evidence amassed during the last quarter century, it is very difficult to believe in the transmission theory. If this were a prizefight and I the transmission theory, I would certainly echo the sagacious words made famous long ago by the great boxer Robert Duran: No mas. (p. 158)

Duran was widely condemned by boxing fans and experts for throwing in the towel too early, and so if this were a prizefight and I the transmission theory, I would certainly not repeat Duran’s mistake. And certainly not with
the impressive group of neuroscientists, physicists, physicians, and philosophers who would be in my corner.

What makes Stokes’ defeatism even more remarkable is the utter timidity with which opponents of the transmission model defend their views. Consider this startling admission from philosopher of mind William Lycan:

Being a philosopher, of course I would like to think that my stance is rational, held not just instinctively and scientistically…. But I do not think that, though I used to. The standard objections to dualism are not very convincing; if one really manages to be a dualist in the first place, one should not be much impressed by them. My purpose in this paper is to hold my own feet to the fire and admit that I do not proportion my belief to the evidence. (Lycan, 2009, p. 551)

Philosopher of mind John Searle has also candidly suggested that the motivation behind acceptance of the production model is more emotional than rational:

Acceptance of the current views is motivated not so much by an independent conviction of their truth as by a terror of what are apparently the only alternatives. That is, the choice we are tacitly presented with is between a “scientific” approach, as represented by one or another of the current versions of “materialism,” and an “antiscientific” approach, as represented by Cartesianism or some other traditional religious conception of the mind. (Searle, 1994, pp. 3-4)

Personal survival is both a theoretical and an empirical possibility. The statement that consciousness, along with memories and personality, may survive the death of the brain is not self-contradictory, nor is it in conflict with any of the laws or facts of science as currently understood. Concerning the compatibility of modern science with personal survival, physicist Henry Stapp has written:

Strong doubts about personality survival based solely on the belief that postmortem survival is incompatible with the laws of physics are unfounded. Rational science-based opinion on this question must be based on the content and quality of the empirical data, not on a presumed incompatibility of such phenomena with our contemporary understanding of the workings of nature. (Stapp, 2010).

The issue of whether personal survival is a fact can only be decided by conceiving of the various possible relationships between mind and body (what Stokes presumably means by “philosophical gymnastics”), by determining what sorts of evidence would tend to corroborate the various possibilities, and then by critically examining the evidence without prejudice one way or the other to decide which of the possibilities provides the best fit with all of the evidence. Shallow treatment followed by cavalier dismissal of evidence indicating personal survival is not enough to put the transmission theory on the ropes.

Stokes seems to realize this, for he then delivers what he clearly considers a knock-out punch:

By the end of the second millennium, it had been amply demonstrated that one’s cognitive and affective life is intimately dependent on brain activity. A twist of a scalpel in one’s hippocampus, and one loses the ability to store new episodic memories. How then, with their hippocampi long since decomposed, can the dead regale us with tales of their adventures in the afterlife? Remove his amygdala, and a violent maniac is turned into a docile creature. How then can a restless spirit, torn not only from its amygdala but its entire brain, terrorize us from beyond the grave to avenge some past injury? It is simply no longer possible to maintain that the personality is independent of the brain or that the brain is simply the conduit through which the soul speaks, rather than the generator of the personality. How, if a mind cannot maintain its memories once the brain has entered the ravages of Alzheimer’s, could it remember its adventures on earth when the entire cerebrum has been reabsorbed into the dust? (p. 158)

But this is not the KO Stokes thinks it is, when we consider Alzheimer’s cases such as the following:
An elderly woman never speaks, no longer recognizes her loved ones when they come to visit, and shows no expression. By the looks of her, she is a human vegetable. And she’s been this way for over a year. Her brain’s cerebral cortex and hippocampus—necessary for memory, thought, language, and normal consciousness—are severely shrunk. Her brain bears little resemblance to a healthy one.

Yet something utterly astonishing is about to happen. As reported by both the nursing staff of her care unit and her family members: “Unexpectedly, she calls her daughter and thanks her for everything. She has a phone conversation with her grandchildren, exchanges kindness and warmth. She says farewell and shortly thereafter dies.” (Betty, 2014)

This case comes from the database of Professor Alexander Batthyany, a professor of cognitive science at the University of Vienna. Batthyany is currently running a large-scale study on the phenomenon known as terminal lucidity, in which severely brain-damaged patients who have been incoherent for years suddenly seem to return to normal functioning, with memories and personality fully intact, usually in the last minutes or hours of their earthly lives.

Batthyany’s preliminary results, presented at the recent IANDS Congress, suggests that normal cognition, or lucidity, does return in spite of a severely-damaged brain, in about 5–10% of Alzheimer’s cases. And only when death is very near.

This percentage may although be higher, as Batthyany’s research shows that episodes are brief — between 30 minutes and 2 hours—and so are easy to miss. These reports give Batthyany the impression that the mind is hidden behind and constrained by a damaged brain, remarking “Much like the moon eclipses the sun, the brain eclipses the self” (Maclsaac, 2014).

How the mind manages to communicate in an unimpaired manner once free from the restrictions of a damaged brain is of course mysterious—but no more so, Batthyany says, than communication through it (Betty, 2014).

Neuro-psychiatrists Michael Nahm and Bruce Greyson describe the devastating effects of Alzheimer’s on the brain:

Several forms of dementia, notably Alzheimer’s disease, are largely caused by degeneration and irreversible degradation of the cerebral cortex and the hippocampus, resulting among other symptoms in confusion, disorientation, and memory loss. It is unclear how severely demented patients can sometimes recognize their family members and remember their lives again shortly before death, suggesting that the memories in these cases had been rendered inaccessible but not entirely deleted. (Nahm & Greyson, 2009, p. 944)

Reports of terminal lucidity date back to ancient times, but there is no shortage of such cases from the modern era. Nahm and Greyson describe how:

A woman aged 92 who had been diagnosed with Alzheimer’s disease for 9 years and did not recognize close family members, including her son, recognized them again 24 hours before she died. Moreover, she knew how old she was and where she was, which she had not known for many years (Nahm & Greyson, 2009, p. 943)

Nahm, Greyson, Kelly, and Haraldsson describe two more cases of Alzheimer’s:

An elderly woman who suffered from the illness for 15 years and was cared for by her daughter. The woman was unresponsive for years and showed no sign of recognizing her daughter or anybody else. However, a few minutes before she died, she started a normal conversation with her daughter, an experience for which the daughter was unprepared and which left her utterly confused.

The second Alzheimer’s case was remarkably similar. In this case it was a woman’s grandmother who had neither talked nor reacted to family members for a number of years until the week before she died, when she suddenly started chatting with the granddaughter, asking about the status of various family members and
giving her granddaughter advice. Her granddaughter reported that “it was like talking to Rip Van Winkle.” (Nahm, Greyson, Kelly, & Haraldsson, 2012, p. 3)

And cases of terminal lucidity are not limited to the late stages of Alzheimer’s disease but also occur in brain cancer (Nahm et al., 2011, p. 2) and meningitis (Osis & Haraldsson, 1977, p. 131)

Comments on Terminal Lucidity

These cases, only a fraction of those found in the literature, clearly indicate that memories and personality may indeed be recovered and accessed after severe, incapacitating brain damage. What reason could there be for Stokes to devote only a single short paragraph to this phenomenon? Merely because it contradicts his conclusions? As noted above, near death experiencers have often reported that their entire life histories flashed before their eyes in incredible detail, suggesting that one purpose of the brain is to filter out memories not necessary for day-to-day existence. If so, then brains clogged with the plaque of Alzheimer’s and brains with other forms of damage may in fact be damaged filters, blocking access even to memories that are necessary for day-to-day existence. Experiences such as those described above make no sense according to “mainstream” neuroscientific opinion, but they make perfect sense if the purpose of the brain is to selectively inhibit consciousness and memory to those thoughts and memories of utilitarian value to the organism. These experiences can be interpreted as the activity of mind disengaged, or in the process of disengaging, from the restrictions of a material brain.

The transmission model can explain everything the production model explains, such as the effect of drugs and brain injury on the mind. For any change in brain functioning, such as that resulting from intoxication or a stroke, should be expected to affect its capacity as a receiver-transmitter just as certainly as its capacity as a producer.

In my opinion, the author’s conclusions are entirely unsupported by an in-depth critical evaluation of all the evidence. What Stokes refers to as “the fundamental dependence of memories and personality traits on the state of the physical brain” seems more accurately to be the fundamental dependence of the expression of memories and personality traits on the state of the physical brain. Given the extensive evidence from biology, medicine, near death experiences, mediumship, and terminal lucidity, which side then should declare No mas?

Conclusion

The main flaw in this book would seem to be the author’s persistently stated reluctance to take certain lines of evidence seriously, merely because “the mainstream scientific community has largely rejected (and more often simply ignored) research suggesting that one might survive death with some portion of one’s personality intact.” It would therefore seem fitting to end this review with a quote from physicist-philosopher Nick Herbert:

In this materialistic age, dualists are often accused of smuggling outmoded religious beliefs back into science, of introducing superfluous spiritual forces into biology, and of venerating an invisible “ghost in the machine.” However, our utter ignorance concerning the real origins of human consciousness marks such criticism more a matter of taste than of logical thinking. At this stage of mind science, dualism is not irrational, merely somewhat unfashionable (Herbert, 1993, p. 10).

References


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In my review of the Braude’s first book, I indicated that he would make a significant contribution to parapsychology. Although we expect continuing contributions from the now retired Braude, this collection of articles, mostly published previously, presents a natural time to confirm my prediction almost four decades ago.

This book brings together articles that span much of Braude’s broad range of interests. In the excellent Preface, he indicates that the title of the book has a two-fold meaning. It refers to the inadequacy of mechanistic approaches adopted in the behavioral sciences, but also in some cases by parapsychologists. On the other hand, it points to the charges by Braude’s critics, who have accused him of engaging in pseudoscience or in other forms of irrationality. The book offers a defense of this thinking in both areas.

The book falls naturally into several sections. In the first three chapters (and merging into the fourth), he argues for four themes that are basic to his approach within philosophy and parapsychology. These are: (a) the poverty of mechanistic theories to explain behavior, (b) an adequate psychological explanation, (c) the importance of and nature of dissociation, and (d) an understanding of human abilities. In Chapters 4 and 5, Braude discusses dissociation and multiple personality, whereas in the final three chapters, he discusses parapsychological topics directly. I will focus on the first section; perhaps, as a fellow philosopher, I see Braude setting up the assumptions leading to the conclusions in most of these later chapters.

Chapter 1 discusses memory trace theory. Yet, it also introduces and argues for what turns out to be a foundational issue for Braude, the inadequacy of mechanistic theory. As this position is used often in the following chapters, either explicitly or implicitly, I will spend a little time on this argument.

Braude argues against the memory trace theory of explanation, which is used widely in philosophy, psychology, and neurophysiology. This kind of mechanistic explanation has risen almost to the level of being taken as heuristic in these fields, but this is because its assumptions have never been deeply examined. It is part of a larger
explanatory strategy that proposes that human behavior is a result of processes occurring in the human body, typically the brain. So, one merely has to analyze the causes of human behavior in terms of these (e.g., brain events) to produce an adequate explanation. Typically, this kind of explanation will assert that the reason I have a certain mental image of Larry LeShan now, let’s say, is because decades ago I met Larry and the experience produced a trace in my brain, some sort of physiological engram that I can point to in order to produce an adequate causal explanation of the mental image I have today. It is that memory trace that produces my present mental image. This sort of explanatory strategy is basic to psychological explanation in the behavioral sciences, but Braude argues that such an engram is an impossible object.

Braude is careful to point out what he is against. He is not arguing against memory traces because they are often conceived of as physical, as located in the brain. His argument is not against materialism but against mechanistic explanation of behavior. It doesn’t matter what the hardware is, whether these traces are physical objects or holographic objects; rather, the problem is that the objects demanded by this kind of mechanistic theory cannot exist. Braude is not objecting to the view that the brain plays some function in human behavior, but merely to the view that the brain stores memories. Braude seems comfortable with the view that the brain plays a role, perhaps an important one, in mediating behavior. Using an example from an old graduate school buddy of mine, John Heil, Braude describes a person who throws a party and wants to remember all the people at the party, so he asks each of them to leave an object, a trace, by which he can remember them. Let’s say it was a photocopy of their driver’s license. But if your driver’s license photo is like mine, it at best resembles what I’d look like in a mug shot. And maybe it was an old license, so it was my decade-old self. Yes, you can still (barely) recognize me, but it looks as if you need another memory (or more) to be able to interpret the photo as being of me to connect the trace with me, but then we need another trace to connect those traces, and we end in a regression. What is needed to avoid this regression is an unambiguous representational calling card, as it were, a representation that is not interpreted. But such an object does not exist. All representational objects can be interpreted. As Braude says, “representing is not something objects can do all by themselves; and representation can’t be an intrinsic or inherent relation between the thing represented and the thing that represents it” (p. 5).

Why is this rejection of memory traces important for parapsychology? Braude points out that William Roll explicitly developed a view of post-mortem survival based on memory traces. And, it turns out that this view is reflected implicitly in Rupert Sheldrake’s theory, which Braude turns to in Chapter 2.

Braude says that he was surprised to find that Sheldrake’s theory, proposed as an alternative to mechanistic theories, actually employs some of the same standard mechanistic errors. Sheldrake proposed his theory of morphogenetic fields as the only viable alternative to mechanistic and vitalistic theories to explain biological morphogenesis (in general, the issue of biological development of organisms from simple structures, and even the continued development of organisms when a part of the organism is removed). Sheldrake proposes that each morphic unit has its own morphogenetic field, and these fields affect morphic units through morphic resonance across time and space.

Braude argues that Sheldrake seems to take morphic units and morphogenetic fields as natural kinds, as things that exist outside of context, as a “context-independent set of natural furniture” in the world. But, as Braude argued in the previous chapter, there are no such natural kinds. Objects are interpreted by humans to be objects of such and such sort, and not, for example, simply parts of larger objects. Objects are distinguished as objects within a certain context, for a certain purpose, and as related to certain concerns. Braude argues that to think there are natural kinds is to commit the Platonic error of essentialism, to think that nature is parsed inherently in certain ways as eternally separate objects with essential qualities. Sheldrake’s assertion of morphic units being natural kinds is such a retreat to mechanistic thinking. There is no inherent similarity in nature, but rather things are taken by us to be similar within certain contexts and for some purposes. Thus, even if Sheldrake’s theory can adequately predict occurrences that other theories do not predict, it still cannot be used as an adequate explanation of these occurrences, according to Braude.

Chapter 3 is a defense of folk psychology. Most explanation in the behavioral sciences depends on the view that one needs to trace back a behavior to an initiating causal state inside the agent. One can readily see, however, based on the previous chapters, that such an approach is mechanistic and therefore flawed for the reasons given before. The interesting tack that Braude takes in this chapter is to apply that rationale to argue that such a mechanistic approach is inadequate to explain a person’s character. We normally explain a behavior by saying that a person is
honest, or compassionate, or tactless, and so on. But mechanistic explanation fails to explain such character traits and dispositions. Further, in explaining a person’s actions, we need to take into account what the person intended, but mechanistic explanations fail to account for such patterns of actions. Braude argues that if we say that a person is ungrateful, we can say this only against a background of knowing what alternative behaviors and intentions were possible; only in having knowledge of such “action spaces” can we characterize an action as ungrateful. Folk psychology can take into account such action spaces, a more adequate psychological explanation, whereas mechanistic explanation excludes them.

In Chapters 4 and 5, Braude turns from a discussion of the inadequacies of mechanistic explanation to a discussion of what we can learn from dissociation and from multiple personality. The point he wants to make about dissociation is how creative it is. Think, for instance, of a person being hypnotized and being told that an elephant is not in the room (while it is). The person must fill in the space that the elephant occupies to make it a “normal” perception; subjects actively deal with their world in creative ways. Further, if we extend the analysis to dissociation, in which a person must cope with expressing and maintaining an alter or alters, we see that a person not only expresses an ability that a particular alter may have, but also the character traits of the alter. One can think of Braude’s conclusion as having implications for such parapsychological issues as an analysis of reincarnation, which is made more explicit in Chapter 6. I see no direct implication for parapsychology coming from Braude’s Chapter 5 discussion of multiple personality and moral responsibility, so I will merely commend this chapter to you for its insight into explanation of multiple personality, which also sets the stage for a discussion of human abilities.

The final three chapters directly concern parapsychology, and they may be most interesting to the readers of this journal, but Braude uses the analyses he has given in earlier chapters as the foundation for the moves he makes in these chapters. Chapter 6 is titled “Parapsychology and the Nature of Abilities.” We often think of psi as a human ability, but Braude argues that we are not clear on what the term means. Braude distinguishes three things that one might mean when using the term “ability”: (a) a capacity that is universal and rudimentary, like the ability to digest food or hear; (b) an ability (as he wants to use the term) that is higher order and refers to dispositions, for example, to be able to cook but also to become a competent musician or athlete; and (c) a skill, which refers to a mastery over more organic endowments, such as sculpting a human figure or controlling one’s heart rate. Because we don’t know which of the three categories to place psi functioning in, we don’t know what methodologies are appropriate to use to investigate it, or how we should view its occurrences. Braude makes a number of important points, and I will mention only a couple. The first is that the highest-level abilities are context dependent, so we should not expect to elicit psi on demand, nor should we expect a person who displays psychic functioning in one area to be able to display it in another; just as a musician may not be a great chef, so someone who can affect an RNG may not be able to remote view. Additionally, given the evidence from dissociation and savants, we need to be careful in assuming that special skills, such as the sudden ability to speak a foreign language, prove post-mortem survival. He concludes that “since no conventional experimental controls can prevent people from using whatever psi abilities they can muster to achieve whatever result they want … the real trailblazers of parapsychological research will be those who recognize that conventional experimental methods and controls may be practically useless” (p. 175), and that the best experimenters will be those who simply are good observers, displaying perceptivity and sensitivity.

In Chapter 7, Braude offers “Some Thoughts on Parapsychology and Religion,” although he is more concerned with nondeist (and nonreligious, it seems to me) explanations. If we really take seriously the nonexperimental evidence from psi (as he believes it is better), we find evidence for super-psi, which means we don’t know the limits of PK and ESP functioning but it seems to be widespread and connect people and the environment in fundamental ways. What is most interesting to me in this chapter is what he calls naughty and sneaky psi. Many want to think that psi is used only in the service of good purposes, but if it is a human ability, we know that there are “naughty” people, as well as naughty and sneaky sides of all of us. Then we should not be surprised if we use psi negatively, even to the point of employing it unconsciously to wreak revenge.

The final chapter is more personal, as Braude apt to consider his work in marginal areas (those discussed in this book). The insights and advice he offers are important for us to hear as we continue to work in this area and, presumably, continue to bear the brunt of criticism.

Braude writes with a clarity and subtlety that cannot be captured in this review. His examples are great (and copious and often funny), and he’s not one to hold punches. He is subtle in his analysis but blunt in his views.
Braude’s conclusions often don’t conform to those of many in parapsychology, but he offers his arguments and conclusions straightforwardly and, like any good academic, urges us all to engage in the discussion. I assume that Braude will offer additional work in the field, but this book offers a nice overview of some of his important work over the past several decades; it’s a fitting dessert for a grand meal. Now we’ll wait and see if he offers us sherry and cigars.

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This is a complex and informative book and it is impossible to do it justice in just a few pages. Suffice to say it is worth reading by anyone who is interested in exploring consciousness, if only for the final four chapters by Ramakrishna Rao who outlines the Yogic, Vedic and Buddhist viewpoints and then summarises the East-West correspondences and differences.

This is of great importance because in the West we often muddle our use of the terms mind and consciousness and make a divide between mind and matter. In the East mind is a different concept from consciousness and in some philosophies mind is material. This leads to a completely different approach to consciousness. For example, in the West the term unconscious can sometimes mean a complete lack of consciousness and sometimes mean mental information of which one is not aware. In the East the unconscious is one aspect of consciousness, with differing meanings depending on the philosophy.

This book is a revised and expanded edition of the original which was published in 1992. The expanded part is the Eastern perspective written by Rao as Part II of the book, and which are revised versions of chapters from his book Consciousness Studies, which was published in 2002, and the final chapter is a revised version of a paper published in the Journal of Consciousness Studies. The chapters in Part I are from a conference on “Cultivating Consciousness,” held in Durham, NC, in 1991, where the various articles were first presented. Thus we need to be aware that these papers were written more than 20 years ago and so the concepts and information are no longer quite so new! This, of course, is especially pertinent for the bibliography.

The introduction is by Ramakrishna Rao, in which he outlines the work by Louisa Rhine and emphasizes how any study of consciousness has to incorporate findings from parapsychology and spontaneous psychic experiences.

Part I: Western Models

Western Philosophical Models

Amongst others, the Institute of Noetic Science provided a grant for this conference, and Willis Harman contributes the first chapter, discussing the need for a reassessment of the metaphysical foundations of Western science. He considers these foundations to be objectivism, positivism, and reductionism, which are the underlying assumptions of logical empiricism and are based on the assumption of separateness. He considers this needs to be rectified by a more holistic science, which he calls a “Wholeness Science,” with interdependence as its foundation.

Stephen Braude responds to Harman’s talk. He considers that Harman fails to identify the most serious errors of mechanism. Instead of “wholism” Braude advocates a scientific pluralism, which recognizes that different scientific disciplines require different methodologies and perspectives, not one single theory to cover everything, but a “community of equals” (p. 42).

Thomas Hurley continues the theme of the metaphysical foundations of modern science and the problems,
such as reductionism, associated with them. He then identifies several themes that he considers to be emerging and that may help shift our worldview. These include the study of complex systems, purpose and self-determination, holistic concepts, and qualitative approaches.

David Griffin is the first to specifically address the western philosophical view of consciousness and the problems surrounding these western concepts. The first problem he identifies is that some Western philosophers even question the existence of consciousness! Their reasoning results from the familiar Western mind-body problem stemming from Descartes. Griffin takes Whitehead’s definition “that consciousness is the subjective form of an intellectual feeling, which arises, if at all, only in the late phase of a moment of experience” (p. 57). Next he discusses the Western difficulty in ascribing downward causation or any power to consciousness, and he brings the concept of pan-experientialism as the philosophy that enables this. Throughout all of this he conflates mind with consciousness and states that consciousness is a “virtually non-efficacious by-product of the mind” (p.66). For Griffin, mind is the most extensive, and his definition of consciousness is that of awareness, since he considers the unconscious to be nonconscious. This is diametrically opposed to the Eastern perspective.

Jean Burns brings parapsychology more specifically into focus, though it has been mentioned in previous chapters. She discusses characteristics associated with the mind-brain interface that incorporate psi into the theorizing, and models of consciousness in which psi is discussed. Many of these incorporate quantum mechanics in some form or other into their hypotheses, whilst her model is a thermodynamic one.

Neil Rossman defines consciousness as varieties of awareness that are displayed by various creatures in a developmental manner that expands as consciousness becomes reflective self-consciousness, and humans develop a sense of self.

Western Psychological Models

Eugene Taylor addresses the problems that Western physical concepts of consciousness have with altered and psychic states of consciousness and with Eastern spiritual concepts of consciousness. He discusses various states of awareness experienced whilst in a sensory deprivation chamber, and suggests that the split between Western and Eastern concepts is due to the West always dealing with the external world whilst the East is more concerned with inner states.

The next chapter is by Ramakrishna Rao, who looks at conceptual and methodological issues. His definition of consciousness is that it is both a state of awareness as well as awareness of something. Added to this are varying levels of subliminality vs. liminality and explicit vs. implicit. He then mentions various Western philosophers and their concepts of consciousness.

Beverley Rubik discusses consciousness in relation to “subtle realms,” such as bioelectromagnetics, and argues for greater gender balance in future research, a softer yin-based approach.

Robert Jahn introduces the idea of the complementarity of consciousness, as in Neils Bohr’s concept of complementarity, a sort of both/and dimension of consciousness.

Charles Tart explicates his model for transpersonal psychology based on computer generated virtual reality. Dreams are our normal every night virtual reality, and he suggests that our everyday experience is a virtual reality.

Some Research Topics

Rather than talking about consciousness per se, Brenda Dunne gives us a brief insight into some of the PEAR REG work and from this suggests support for a complementarity principle in consciousness previously outlined by Jahn.

In the context of the role of wholistic healing within western philosophy, Michael Grosso considers the power of imagination in healing, such as cultural psychosomatic disease and healing forms of consciousness.

Alfred Alschuler considers the experience of inner voices in people, such as saints, political leaders, clairvoyants, and the role they have played in human culture. Commonly they are transcendent experiences that people relate to union with the divine or an inner teacher.

The chapter by Srinivasan presents the first Eastern perspective on the topic and discusses the nature of re-
ality from the worldview of an ever-changing universe that is coexistent with a background of unchanging reality. In this model evolution proceeds from the changeless to everything, including mind, in the present time-space material universe. Some Indian philosophers equate the unchanging reality background with pure Consciousness.

The final chapter in Part I is by United States Senator Claiborne Pell. He makes a plea for more research into survival of bodily death.

As can be seen from these extremely brief reviews of highly complex topics, the Western views of mind and consciousness span a huge range with no two people addressing either the same issues or having the same understanding of mind and consciousness.

Part II: Eastern Perspectives

In Part II, which comprises one third of the book, Ramakrishna Rao first presents the Yogic philosophy of consciousness. Although a Yogic scholar may well find his brief explanation inadequate, for me, as a Westerner who is unfamiliar with the finer details of this philosophy, I found it very clearly written and a most interesting view contrasting with the various Western concepts discussed in Part I. Yoga philosophy is linked with the Samkhya philosophy, considered to be the oldest philosophy in India. It essentially espouses two basic principles in the universe, prakrti (matter) and purusa (pure consciousness, which is the foundation for awareness and is different from mind, although there are no direct translations of the Sanskrit words). Both are primary and irreducible principles, all-pervading and ubiquitous. Evolution is the actualization of these potential principles. When the two become entangled, then the conscious mind is formed. Mind is the interface partaking in consciousness and in the material world. Yogic philosophy distinguishes three aspects of mind. The central processor (manas) aspect of mind assimilates the sense perceptions, which are then related to the ego (ahamkara). This is the aspect of mind being researched by neuroscience. This is then transformed into awareness by the psyche (buddhi), which enables consciousness of the object by virtue of its association with purusa. The consciousness of purusa is reflected on buddhi. When this final stage does not occur, we have unconscious cognitive states (samskaras). This philosophy enables psi to have an essential place within the worldview. “Time and space are categories created by the mind to organize and understand sensory information. Buddhi itself exists beyond the constraints of space and time” (p. 243). Thus, awareness is of two sorts —transcendental (mystical, intuitive) and phenomenal (the material world), which enables eight different states of consciousness, one of which (the anomalous) is related to psi awareness.

Even more interesting is the next chapter, in which Rao compares and contrasts the Yogic philosophy with that of Advaita Vedanta, the philosophy of Shankara. This philosophy brings in the concept of Atman, or pure consciousness, which is self-manifesting and self-illuminating, contentless, formless, nonintentional, not limited by time and space, both subject and object, undifferentiated, knowledge itself, and “rests in no other” (p. 261). Atman becomes the personal consciousness in the form of jiva which is consciousness limited by the mind and body. As in Yogic philosophy, mind is considered to be the subtlest form of matter, bridging consciousness and matter. Within personal consciousness there are four cognitive states: waking, dream, deep sleep, and samadhi. Rao discusses the ramifications of this philosophy and starts to build a much bigger picture of the sophistication and understanding of the Eastern traditions. It is commonplace to state that in the West we have explored outer knowledge whilst in the East they have explored inner space, and this is brought out clearly in this chapter. Reading these Indian philosophies makes me feel that we Westerners really are in nursery school insofar as the concept of consciousness is concerned.

Rao then brings Buddhist philosophy into the pot. He explains consciousness from the viewpoint of Theravadan Buddhism, which has a complex phenomenological psychology of consciousness, very different from the Tibetan traditions with which I am more familiar. In Buddhism, the mind is composed of momentary states of consciousness that are constantly arising and dissolving, much as a flame or a river is constantly changing in a ceaseless becoming of dependent origination. Theravadan Buddhism is more of a psychology than a philosophy, aiming at an understanding of the nature of consciousness, which is a relationship between subject and object. From these relationships, Buddhism has identified 89 states of consciousness, such as the sense domain, the domain of thinking, reflection, concentration, and the transpersonal plane. Our consciousness is a dynamic process with both subliminal and supraliminal components; the subliminal component is called bhavanga and is a key concept, similar in many
ways to buddhi in Yogic philosophy. Further, consciousness is considered to contain 52 basic elements, such as feeling, volition, perception, attention, which combine to create variations in consciousness. Our perceptions are coloured by our conditioning and occur as a process involving six steps, which can vary, thus leading to changes in our experience. Rao then briefly mentions the later Buddhist Mahayana philosophies and compares Buddhist philosophy with Vedanta and Yoga.

And finally Rao compares Western and Eastern concepts in his summing-up chapter. He notes the wide variety of concepts covered by the Western authors and suggests that the one commonality is that all conceive of consciousness in some way connected with awareness. He then gives a brief review of the history of Western philosophy and psychology of consciousness, and a review of the Indian philosophies. He concludes by describing the two approaches as complementary, the West emphasizing the phenomenal and the East the transcendental. Both are important.

I do not necessarily think that in the West we should adopt any of the Eastern philosophies, but I think it is really useful to understand their perspectives, because I think that it helps to clarify the dreadful muddle we have in the West and, from this clarity, perhaps advances can be made in our understanding of consciousness.

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To the Editor:

The recent article by Dalkvist, Mossbridge, and Westerlund (2014) on expectation bias in presentiment studies discussed an important methodological problem but included a controversial recommendation and two key comments that are not correct. Presentiment studies investigate whether physiological measures indicate that a person can unconsciously and precognitively anticipate a random stimulus. The most common strategy for analyzing the data has been to compare the average values of the observed physiological measures preceding the different types of random stimuli.

This analysis strategy reverses the traditional analysis for a typical ESP experiment, such as a participant pushing a button to predict which light will be randomly selected. The traditional analysis uses the button press or response to predict the random light or target. As described by Burdick and Kelly (1977, p. 93):

The response array is taken as fixed (in fact, it is immaterial where it came from, and this underlies the great generality of the method). The statistical problem is to evaluate the probability of obtaining a number of hits as large or larger than that observed, given the response array.

The random targets are the outcomes that are assumed to be variable and to follow a probability distribution.

This strategy was developed in the 1940s when it was realized that the responses cannot be assumed to be independent as is required for the outcome variable for standard statistical analysis. One of the most well-known examples is the stacking effect that occurs when a single target sequence is used with multiple participants. The responses are not considered independent because “the respondents may tend to possess shared guessing habits” (Burdick & Kelly, 1977, p. 92). The basic principle is that habits and associated nonindependence may occur within any sequence of responses generated by a human, and among any such sequences, including when feedback is not given. As noted above, any habits are immaterial when the traditional analysis strategy is used. The random targets are the outcome variable and are statistically independent (if generated with replacement).

The reversed strategy used in presentiment studies treats the random stimuli (targets) as fixed and uses the physiological measures (responses) as the outcome variable. This strategy results in concerns about dependence among responses. Given the immediate feedback on each trial, the analysis can have biases resulting from the physiological measures reflecting the properties of the particular random target sequence for a participant (Dalkvist, Mossbridge, & Westerlund, 2014; Kennedy, 2013).

Expectation bias as discussed by Dalkvist, Mossbridge, and Westerlund is a type of dependence that can occur in cases with immediate feedback. The traditional assumption for ESP research has been that human responses can also have other forms of dependence that the experimenters may not anticipate and that may or may not involve feedback.

One option for presentiment studies is to apply the traditional analysis strategy by using the physiological measures to predict the random stimuli. When properly done, this eliminates problems of dependence. A previous article recommended this option for confirmatory research and discussed the requirements for proper application (Kennedy, 2013).

Another option is to use the physiological measure as the outcome variable and to build statistical models that attempt to handle the potential dependence. However, the nature, amount, and effects of dependence are difficult to establish, and it is difficult to show that a statistical model adequately handles potentially complicated dependencies among responses (Kennedy, 2013). Dalkvist, Mossbridge, and Westerlund recommended that a simple model be used, but they provided little discussion about methods to evaluate whether a model adequately corrects the means and error variance for all pertinent dependencies.

From my perspective, this modeling approach cannot be expected to provide convincing evidence for a controversial effect (Kennedy, 2013). In addition to making this debatable recommendation, Dalkvist, Mossbridge, and Westerlund made two comments that are not correct.
Dalkvist, Mossbridge, and Westerlund inaccurately stated that bootstrap methods are “free from any statistical assumption” (p. 93) and can be applied in cases with dependencies among observations. Contrary to their comment, standard bootstrap methods are based on the assumption that the original observations are independent (Efron & Tibshirani, 1993, pp. 27, 31, 45, 396; Good, 2005, p. 23). Bradley Efron, the initial developer and promoter of bootstrap methods, commented: “There is no easy solution to problems of dependence … problems of dependence do not appear to be well understood and are an important area for further research” (Efron & Tibshirani, 1993, p. 396). As implied by this comment, simple models cannot be assumed to solve dependence problems.

Dalkvist, Mossbridge, and Westerlund also inaccurately said that my recommendation to use the physiological measure to predict the random stimuli is flawed by very low power because it does not adjust for the effects of the previous stimulus on the physiological measure. However, adjustments for the previous stimulus can be done with this analysis. The physiological values used in the final analysis for presentiment studies typically are derived by relatively complex processing after all data have been collected. The processing is usually described as baseline adjustment, normalization, data reduction, and/or artifact rejection.

The optimal strategy for confirmatory research is to have all decisions, derivations, adjustments, and criteria for the physiological data for a trial use only data collected prior to feedback for the trial (Kennedy, 2013). Any incorporation of data after feedback introduces potential for bias. When the physiological measures are used to predict the random stimuli, the trials can be stepped through in the sequence that they occurred, with the prediction criteria developed from previous studies or from previous trials in the current experiment. The prediction criteria can include adjustments for the stimulus on the previous trial. Models similar to those discussed by Dalkvist, Mossbridge, and Westerlund may be useful in developing the prediction criteria, but a confirmatory hypothesis test will be based on applying the criteria to other data that were not used in developing the criteria. This strategy avoids contamination by data after feedback for a trial—and also avoids other dependence problems.

The current situation with presentiment research reminds me of free-response research in the late 1970s. Many free-response studies had been done with methodology that was not optimal. The initial discussions of the methodological issues (e.g., Kennedy, 1979a, 1979b) resulted in arguments that some of the methodological concerns were unlikely to have significant effects and/or could be easily counteracted. However, after a few years of debates (e.g., Hyman, 1985), it became apparent to virtually everyone that the controversial methods should be avoided in future research (e.g., Hyman & Honorton, 1986).

References


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To the Editor:

We agree with Kennedy that great care should be taken to avoid use of uncertain methods in future presen
timent and other precognition experiments. In particular, in our above-mentioned paper (Dalkvist, Mossbridge, & Westerlund, 2014), we have argued forcefully that expectation bias—a counterintuitive statistical bias caused by
the erroneous belief, on the part of a participant, that the likelihood of a forthcoming sequential random event, such
as a particular outcome in a sequence of coin flips, is dependent on previous outcomes—must be totally avoided or
controlled for. This is particularly important because expectation bias causes systematic—in contrast to unsystem-
atic—errors, thereby leading to invalid rather than unreliable results, the latter being in general less serious than
the former, because they do not distort data in any fundamental way. We have also argued that ANOVA is a suitable
method (although not the only one) for dealing with expectation bias.

However, as pointed out by Kennedy in the above Letter to the Editor and elsewhere (e.g., Kennedy, 2013),
there are also other dependencies among responses that can compromise results from presentiment and other pre-
cognition experiments, for example, particular response patterns. However, in contrast to the effects of expectation
bias, provided that stimuli are properly randomized, such effects are not systematic and can therefore not by them-
selves give rise to invalid results, but only affect the alpha level (in an unknown way). Nevertheless, the effects
should be addressed. In our 2014 paper, two alternative solutions were discussed. One was to follow Kennedy’s
recommendation to reverse the statistical roles of the physiological variable and the previous stimulus, letting the
former serve as the independent variable and the latter as the dependent variable. Specifically, we recommended
doing this within the framework of ANOVA, with the previous stimulus and the physiological variable serving as
independent variables and the forthcoming stimulus as the dependent variable—not separately, as recommended by
Kennedy. Unfortunately, though, the need to categorize the physiological variable when it serves as an independent
instead of dependent variable leads to an expected reduction in statistical power (potentially useful quantitative
information is lost). For this reason, the optimal strategy may be to perform two tests—one with the physiological
variable serving as the dependent variable (without categorization) and the other with the physiological variable
taking the role of an independent variable (with categorization)—to investigate whether they converge, in which
case the more powerful test could be accepted.

Our second suggested approach to the problem of dependent responses was to use a statistical computer
simulation—a so-called Monte Carlo method instead of a traditional statistical, so-called parametric method—owing
to the Monte Carlo method’s freedom from underlying assumptions. Specifically, we recommended using “bootstrapping” because of its general availability. Regrettably, however, it had escaped us that this particular method, despite
its freedom from the two other assumptions of traditional parametric statistical methods—equal variances and normal
distributions—still requires satisfying the assumption of independent responses if the alpha level is to be reliable. We
are grateful to Kennedy for noticing and reporting this oversight (which occurs easily considering the simplicity of the
bootstrap method). It should be kept in mind, however, that other Monte Carlo methods exist that are free not only from
the assumptions of equal variances and normality, but also from the assumption of independent responses. This holds,
for example, for methods in which the stimuli, not the responses, are recursively randomly redistributed to create an
“empirical” sampling distribution of a particular response statistic (e.g., the mean) associated with randomly chosen
stimulus orders, even though the number of different stimuli limits the statistical power of such methods (for an exam-
ple, see Dalkvist & Westerlund, 1998). Unfortunately, however, at present these stimulus sampling methods require
specifically tailored programming to be done, and they are thus not equally readily available as bootstrapping methods.

Despite the problem of dependent responses in testing presentiment and other precognition results discussed
above, we insist on recommending that ANOVA be used. We do this mainly because, by using ANOVA, the expected
(huge) effect of the previous stimulus becomes isolated from the possible effect of the forthcoming stimulus in a
direct and natural way. It is true, as pointed out by Kennedy, that the effect of the previous stimulus can be controlled
for separately before the main test is performed—for example, by extracting residuals from regression analysis in a
preparatory phase of the analysis, although such a method seems unnecessarily complicated. Exactly how effects of
the previous stimulus (and possibly also some earlier stimulus) are controlled for is not a major concern, however.

Admittedly, from a purely methodological point of view, Kennedy’s alternative strategy would seem to be
a good one. There may be a psychological problem involved, however: How can we get researchers to accept and
do the relatively laborious work the strategy requires?
To the Editor:

Professor Caroline Watt’s research, supported by the Perrott-Warrick fund, has recently focused on precognitive dreams. Some of her research has basically been anomalistic psychology (Valášek, et al., 2014; Watt et al., 2014), but in one thought-provoking study the psi hypothesis was also tested (i.e., Watt, 2014). The dream ESP studies have recently been reviewed (Sherwood & Roe, 2013), but since Watt has not cited any specific study as being inspirational I refrain from comparing it to prior studies. Some comments do however seem warranted.

Parapsychologists often expect participants in their studies to do things that they have never claimed to be able to do on command. Nevertheless, the researchers seem surprised when their results do not turn out to be significant. Watt argued that her sample was appropriate, since the majority of the participants “believed in precognitive dreaming, had experienced an evidential (according to Bender’s criteria) precognitive dream personally at least once in their lifetime, and were able to recall their dreams at least once per week” (p. 121). Nevertheless, evidential precognitive dreams were quite rare experiences for most participants, and only two believed that they experienced this about once a week.

Precognitive dreams range from trivial to profound unforgettable experiences, but often concern deaths or accidents. The 50 participants in Watt’s study were expected, during five nights at home, to dream about a randomly selected short video clip (around 1 min) that they would get to watch at some unspecified time in the future. Access to the clip was not given immediately. The task can thus be compared to trying to predict what one will briefly watch on television at some randomly determined time.

In addition, the participants were limited to 300 words to summarize their dreams (this letter is 632 words). The literature suggests that precognitive dreams are in some ways distinctive, but five nights can generate quite extensive dream reports. Unless the participants had learned to distinguish precognitive dreams they had little chance of excluding irrelevant dreams in their summaries. Signs of psi may have disappeared during the transfer to the summary, drowned by irrelevant dreams. Understandably, most participants doubted that their dream summaries would relate to the target clips.

Correspondence to the target clip was rated on a 1–100 scale by a judge and then by the participants themselves. In general, both the judges and the participants thought that there was very little resemblance. None of the two judges had any prior experience of evaluating this kind of material. Surprisingly, there were still 64 direct hits (32%) despite that “there was nothing qualitatively special about the hits compared to the misses” (p. 123). Similarity ratings had no significant relationship to hit rate. A possible explanation of the strange results is that Watt unintentionally tested the judges’ ability to predict the correct targets. Some would however argue that their unexpected
success was likely more due to an experimenter psi effect rather than due to their own ability.

References


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To the Editor:

I am grateful to Nemo Mörck for his comments but would like to correct a couple of inaccurate statements. First, Mörck notes the relevance of Sherwood and Roe (2013), but states that “Watt has not cited any specific study as being inspirational.” Sherwood and Roe (2013) had not been published at the time our study was conducted. However we do cite Sherwood and Roe’s (2003) review and call for further research as motivating our study. Second, Mörck states that our participants were asked to dream about a video clip “they would get to watch at some unspecified time in the future.” In fact, our participants were informed: “After we have received your dream summary you will be sent a ‘target’ video clip to view.” Because the study aimed to have each participant conduct one trial per week, participants knew that they would quickly receive feedback on the target after they submitted their dreams, usually within hours. Finally, Mörck correctly notes that there is some ambiguity over the source of extra-chance scoring in Watt (2014). Due to the way that psi is defined, it is not possible to be absolutely certain about the source of any putative psi effect. I hope that, in time, improved theoretical understanding of psi will help to minimise such ambiguities.

References


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</tr>
<tr>
<td>f. Total Distribution ([Sum of 15e])</td>
<td></td>
<td></td>
</tr>
<tr>
<td>g. Copies not Distributed ([See Instructions to Publishers #4 (page #3)])</td>
<td>3</td>
<td>4</td>
</tr>
<tr>
<td>h. Total ([Sum of 15f and g])</td>
<td>192</td>
<td>181</td>
</tr>
<tr>
<td>i. Percent Paid ([15c divided by 15f times 100])</td>
<td>100</td>
<td>100</td>
</tr>
</tbody>
</table>

*If you are claiming electronic copies, go to line 16 on page 3. If you are not claiming electronic copies, skip to line 17 on page 3.
16. Electronic Copy Circulation

<table>
<thead>
<tr>
<th>Description</th>
<th>Average No. Copies Each Issue During Preceding 12 Months</th>
<th>No. Copies of Single Issue Published Nearest to Filing Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>a. Paid Electronic Copies</td>
<td>468</td>
<td>468</td>
</tr>
<tr>
<td>b. Total Paid Print Copies (Line 15c) + Paid Electronic Copies (Line 16a)</td>
<td>657</td>
<td>645</td>
</tr>
<tr>
<td>c. Total Print Distribution (Line 15f) + Paid Electronic Copies (Line 16a)</td>
<td>657</td>
<td>645</td>
</tr>
<tr>
<td>d. Percent Paid (Both Print &amp; Electronic Copies) (16b divided by 16c × 100)</td>
<td>100</td>
<td>100</td>
</tr>
</tbody>
</table>

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☐ If the publication is a general publication, publication of this statement is required. Will be printed in the FALL 2014 issue of this publication.

☐ Publication not required.

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Robert S. Goldein  BUSINESS MANAGER

Date: 9/29/14

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