Journal of Parapsychology

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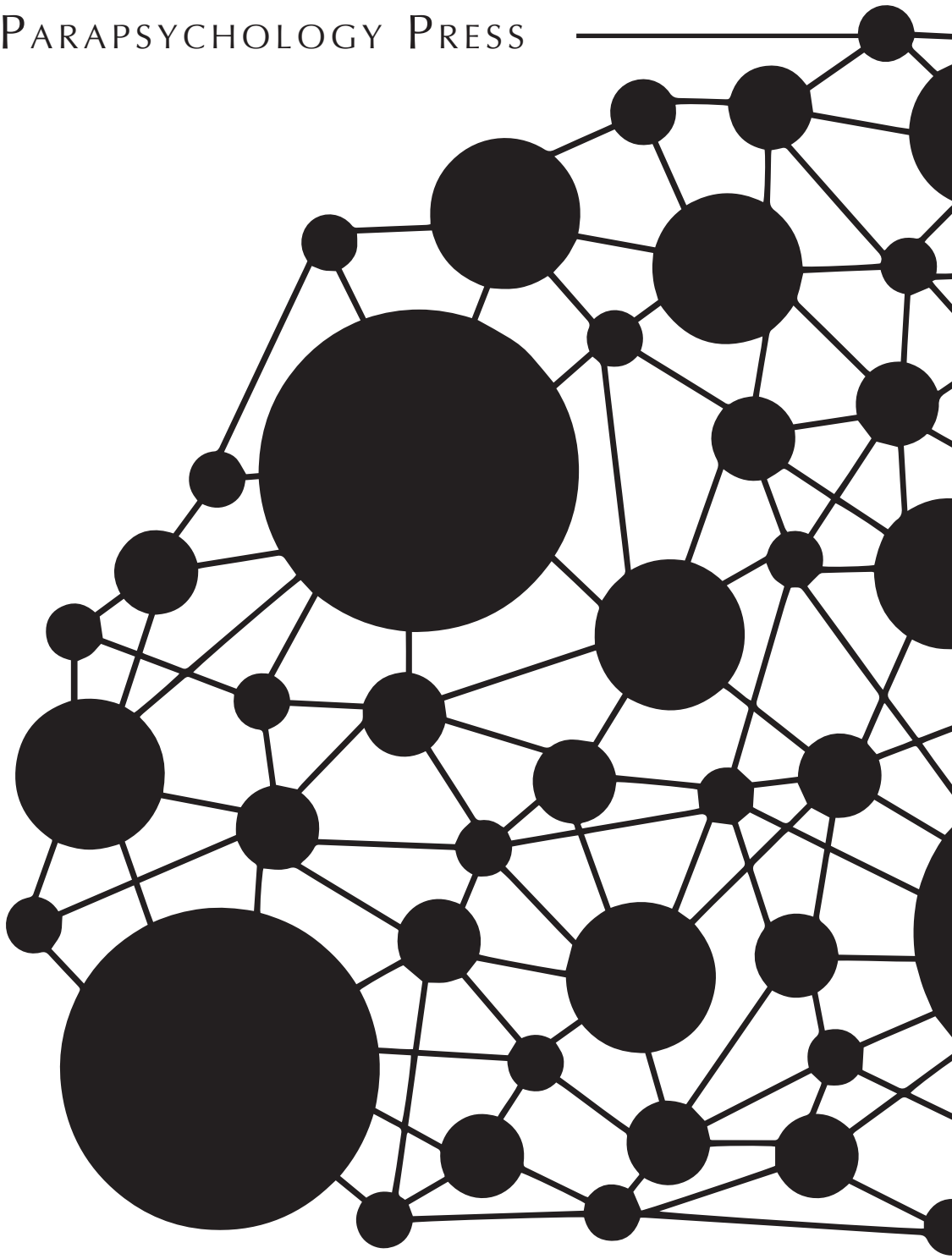

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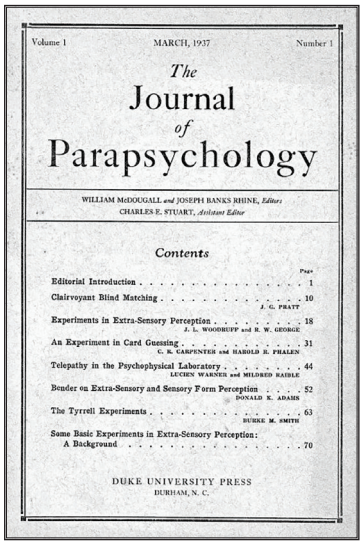

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WILLIAM McDOUGALL *and* JOSEPH BANKS RHINE, *Editors*
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Contents

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Table of Contents

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Journal of Parapsychology
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Editorial: Four Score (Plus) Years Ago

Etzel Cardeña

Among their many achievements, Joseph Banks Rhine and collaborators launched *The Journal of Parapsychology* (*JP*) in 1937, the foremost venue for parapsychology experimental research and surpassed in longevity only by the *Journal of the Society for Psychical Research.* There had been important controlled studies in parapsychology earlier on, but J. B. Rhine and his collaborators placed experimental parapsychology on a more secure footing. The eighty-plus years of *JP* issues would constitute an extraordinary achievement in any field, but is even more remarkable in such a contentious area as parapsychology. To avoid repeating mistakes one should be cognizant of the field's previous history (Cardeña, 2018a) and even a cursory look at the *JP* indexes shows how the field has developed throughout the years. My intention for this Supplementary E-issue was to give a bird's eye view of the coverage in the *JP*. My selection of articles will not make everyone completely happy but I trust that it will not make everybody surly either. My criteria to select articles for this issue were to: a) have an article per decade, 2) cover most of its main areas, 3) avoid having more than one article by the same author, and 4) include papers by important women and men, giving their right due to the former (Cardeña, 2018b).

I have left the original papers with very little reformatting and excluding addresses, which are now obsolete. This issue starts with two new papers I commissioned for the issue. In the first one, the previous editor, John Palmer, who very likely knows more about the *JP* than anyone, provides a general overview, focusing on some of the major controversies and debates covered in it, including how J. B. Rhine's original policy not to publish non-supportive papers was challenged from within the field.

The second one, by the most prolific parapsychology historian nowadays, Carlos Alvarado, sketches the diversity of topics and approaches covered by the *JP* throughout the years, changes in editorial emphases, and how the *JP* has contributed to the field overall.

The first archival paper is by Joseph Banks Rhine, the most recognized figure in the experimental approach to psi phenomena in the middle of the 20th century. His article is characteristic of the approach of his group, a meticulous account of carefully designed very extensive sets of studies ("selected from our present stock of several hundred thousand trials") that thoroughly tested alternative non-psi explanations, improved methodology in answer to valid criticisms, and vigorously refuted invalid critiques. One of his observations, echoed by more recent research (e.g., the Nelson paper in this issue) is that in one series participants were more accurate in the first and last sections, reminiscent of the primacy and recency effects in recall of newly learned material. The fact that the paper had originally been published in the foremost journal on abnormal psychology then (and now), exemplifies how close he came to making parapsychology a mainstream accepted area of study.

Next is a paper by Gardner Murphy, who was not only the President of the (British) Society for Psychical (SPR) but of the American Psychological Association (APA). He authored a narrative, qualitative analysis of data relevant to possible survival, discussing how difficult it is to definitely rule out whether relevant data can be explained through psi by the living, while simultaneously pointing out to the potential motivation by discarnates as the explanation for some communications, a discussion very much alive in our days (e.g., Braude, 2003; Sudduth, 2016). By the way, one of the most important psychologists of all time, William James, was also president of the SPR and APA, and a founder of the American Society for Psychical Research. More recently Philip Zimbardo, another APA past-president, is on record for supporting an informed study of psi phenomena (Cardeña, 2014).

The next contribution is by the other great Rhine, Louisa, who focused on systematically collecting, organizing, and analyzing reports of spontaneous putative psi phenomena, following the tradition of the Society for Psychical Research's landmark *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886). Her paper has various examples of the four categories she found in most spontaneous reports and has a number of theoretical speculations that have not been fully mined. There are excellent reasons and bodies of knowledge (e.g., the research on CORT, or Cases Suggestive of the Reincarnation Type, see Kelly & Tucker, 2015) to justify the thorough investigation of putative psi phenomena occurring outside the lab. Louisa E. Rhine had also the rare ability of being an excellent popularizer of the field for the general audience (e.g., L. E. Rhine, 1967).

From qualitative emphases we move to the hard sciences, with physicist Helmut Schmidt and his landmark description of the new PK/precognition methodology he developed, involving the intentional anomalous deviation from randomness of random event generators he devised. In addition to this new method, he went on to publish other original and challenging works such as the one in which he described *retroactive* PK effects (Schmidt, 1976). Varvoglis and Bancel (2015) have described how using "selected subjects" (see also the Utts paper in this issue) and being procedurally flexible encourages better performance by participants.

And from physics we move to psychology and an area that deserves more attention, the sociocultural and neurocognitive variables potentially related to psi. Gertrude Schmeidler wrote many papers and books seeking to integrate mainstream psychology with parapsychology (e.g., Schmeidler, 1988) and we owe her, among other things, the finding that those who believe in the existence of psi ("sheep") tend to manifest slightly but significantly higher scores than those who do not hold such belief ("goats'). This effect has been replicated in meta-analyses (e.g., Storm & Tressoldi, 2017) and, of course, does not make any sense unless humans can somehow cognize stimuli temporally or spatially distant, otherwise there would not be any significant difference between the two groups. I do not think that currently any psi researcher is looking at brainwave biofeedback and psi, but some studies have found a relation between alterations of consciousness and performance in psi tasks (for a review see Cardeña & Marcusson-Clavertz, 2015).

One of the most celebrated papers in parapsychology is the collaboration between a psi-critic (Ray Hyman) and a psi-proponent (Charles Honorton). Rather than continuing to argue with each other, they collaborated on a landmark article citing their agreements and disagreements and proposing improvements to and standardization of ganzfeld methodology and reporting. Meta-analyses of research following such guidelines have continued to support a valid effect for ganzfeld research (for a review

see Cardeña, 2018c). They mention that "The critic can contribute to this need only if his criticisms are informed, relevant, and responsible," conditions that I have found to be rather rare in many if not most of the criticisms of psi research.

Another discipline that has been fundamental to psi is statistics. Parapsychology has made important contributions to research methodology and the use of statistics, including the first systematic use of meta-analysis (Pratt, Rhine, Smith, Stuart, & Greenwood, 1940). The paper included is by the 2016 President of the American Statistical Association, Jessica Utts. In it, she provides a thorough analysis of the psi program conducted at SRI (Stanford Research International) and SAIC (Science Applications International Corporation), taking into consideration methodological weaknesses. However, she failed to mention that *contra* a criticism of the SRI Price series, later research by Tart, Puthoff, and Targ (1980) found that extraneous cues in the transcripts did not explain away the significant results. She concludes that, although anomalous cognition cannot be demonstrated on demand, as so many other skills in life, "anomalous cognition is possible and has been demonstrated... No one who has examined all of the data across laboratories, taken as a collective whole, has been able to suggest methodological or statistical problems to explain the ever-increasing and consistent results to date." In the same issue she published her paper there was a long rebuttal by the critic Ray Hyman (1995). His comments are at times reasonable. For instance, I fully agree that it is a losing cause to define psi negatively, as phenomena lacking scientific explanation or not explained by conventional means, whereas a reasonable hypothesis is that consciousness (or mind, to include conscious and unconscious processes) is not fully bound by temporal and spatial constraints. Other criticisms are, in my view, untenable, and he gives an idealized version of science that is not only not true of the *soft* sciences, but even of the *hard* sciences. In a reply to his rebuttal, Utts (1995) belies Hyman's statements that parapsychology lacks a cumulative database, that psi phenomena are the only ones in science that claim ontological reality by rejecting the null hypothesis, and that parapsychology is the only field of science in which students cannot be assigned an exemplar in class in which they will be able to get original results. The last point is particularly obvious for anybody in academia. In my many years of advanced education not only in psychology but also in behavioral developmental genetics, psychophysiology, anthropology, and other disciplines, other than some easy experiments in psychophysics and learning with caged rats I was never exposed to what Hyman declares to be a characteristic of science pedagogy.

The final paper, by Roger Nelson, provides an excellent antidote to the often-voiced criticism that nothing has been found or developed in parapsychology in more than a century. In contrast, he gives a detailed list of what was developed and found by the Princeton Engineering Anomalous Research (PEAR) group, which was foundational for the later development of the Global Consciousness Project (the GCP). The GCP evaluates whether the output of random number generators distributed throughout the globe can be affected by changes in consciousness in the population following an attention-grabbing event such as the 9/11 attacks (Nelson updated this information in Nelson, 2015). The GCP is also an example of research with big and open data, which very few projects in any areas of science can boast of, and yet another example that psi does have a cumulative database.

Nelson's optimistic forecast of the future of parapsychology is also a good note to finish this celebration of the 80 years of the *Journal of Parapsychology.* May the reader be at least as inspired and impressed by reading these past contribution as I have been. I want to thank a number of people for

their assistance with this issue. I sent a call for nominations of the most important *JP* articles from its inception, and the following provided useful suggestions, even if I did not always heed them: Carlos Alvarado, Roger Nelson, and Nancy Zingrone. Thank you also to John G. Kruth for arranging the scanning of the original articles and to Daryl Junk for doing the scanning and initial editing.

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80 Years of the *Journal of Parapsychology*: An Historical Overview

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Abstract: In this invited article, the author reviews the history of the *Journal of Parapsychology* from its inception in 1937 to 2017. The focus is on published controversies and debates with critics outside the field of parapsychology, *JP* publication policy, and the changes in editorship.
Keywords: Journal of Parapsychology, parapsychology, publication policy

When Etzel Cardeña invited me to write a history of the *Journal of Parapsychology* (JP), I immediately knew that I had to make some difficult decisions about how to cover journal content. If I ignored the content and stuck to process issues such as changes in editorship and publication policy, the finished product would be but a few pages. On the other hand, if I tried to cover all the content, the paper would be way too long, even if I said so little about each contribution that the summaries would not be particularly useful. So it quickly became evident that I had to be selective about the content I covered. I decided to focus primarily on two classes of contributions: (a) articles about the *JP* per se, including summaries of content over a circumscribed period of time and publication policies, and (b) special sections of a particular issue or successive issues comprised of series of papers on a given topic or theme. Most of these represented controversies about the status of the evidence for psi. I tried to describe these sections in a matter-of-fact way and avoid injecting my own opinions and prejudices. As the piece is historical, I will order the paragraphs more-or-less chronologically.

As noted by Mauskopf and McVaugh (1980) in their definitive review of the early years of the Parapsychology Laboratory at Duke, J. B. Rhine was eager to establish a scientific journal for parapsychology that he felt would provide legitimacy and mainstream recognition of the research. It began in 1937, with Rhine and the famous psychologist William McDougall, chairman of the new Duke psychology department, as editors. The journal was enabled by funding from Alice Crunden and then from Charles Ozanne, both of whom also gave considerable money to fund research at the laboratory. The editor was originally intended to be the noted psychologist Gardner Murphy, but he withdrew as he felt that the research papers scheduled for the next issue were of insufficient quality. Although some recognition was achieved in that articles in the journal were referenced in *Psychological Abstracts* from the outset, psychologists at the time could not be enticed to publish in it (Mauskopf & McVaugh, 1980).

In January 1939 Murphy reconsidered and assumed co-editorship of the journal with Bernard

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Riess, primarily to free up Rhine to devote more of his energy to research. He also was encouraged by a policy suggested initially by Rhine, based on a symposium at an American Psychological Association convention, to establish a Board of Review, consisting of 11 psychologists, 5 from New York (including Murphy), 3 from New England, and 3 from the West Coast to review and critique papers published in and submitted to the *JP*. The January 1939 issue indeed contained articles critical of the research (Mauskopf & McVaugh, 1980). However, this initial relation between parapsychology and psychology was not sustained.

The December 1938 issue featured a summary of six papers presented at a symposium entitled "Methods in ESP Research" at the recent APA convention (Journal of Parapsychology, 1938). Louis Goodfellow presented a critique of the Zenith Foundation's radio-telepathy experiments. Thomas Greville discussed mathematical and statistical methods in ESP research. Harold Gullickson argued that the cards used in J. B. Rhine's ESP experiments gave off sensory cues; Rhine rebutted Gullickson's criticism. John Kennedy raised the possibility of recording errors in the ESP tests, and Murphy discussed the limitations of this hypothesis.

The September 1939 issue was devoted to a series of papers of a symposium on parapsychology at the 34th Annual Meeting of the Southern Society for Philosophy and Psychology. There were five papers. The first two were discussions of parapsychological statistics by Joseph Greenwood (1939) and Greville (1939), the main statistical consultants of the Parapsychology Laboratory. The second two papers were discussions of parapsychological experimental methods by staff members Pratt (1939) and Charles Stuart (1939). The final paper was by a critic, Vernon Lemmon (1939), who focused on optional stopping artifacts. A panel discussion ensued.

In support of the plan to promote external criticisms of the research, the December 1940 issue featured statistical critiques by Willy Feller (1940) of Brown University, and Douglas Ellison (1940) of Stanford University, with replies by Greenwood and Stuart (1940) and by Pratt (1940) respectively.

In the March 1942 issue, J. B. Rhine (1942) formally announced the resignation of Murphy and Riess as co-editors of the journal, which he attributed to "the complications which [World War II] and parallel developments have introduced for them" (p. 1) without going into further detail. Rhine continued as (primary) editor until 1960, with Pratt as a co-editor. Other co-editors at various times during this period were Stuart, Betty Humphrey, Louisa Rhine, and William Roll.

In the editorial, Rhine also announced the discontinuation of the Board of Review, noting several resignations from the Board and the delays in publication that the reviews caused. He also noted that the *JP* would begin publishing more "general" articles that less technically sophisticated readers could better comprehend. He also heralded the publication of research reports reflecting the introduction of free-response tests conducted by staff member Stuart and the analysis of card-guessing data for position effects.

The editorial was followed by a review of the first 5 years (volumes) of the *JP* by Dorothy Pope and Pratt (1942). The most prominent content was original research reports, of which there were 39, but there was an absence of "speculative and theoretical discussion" (p. 5). Across the 5 years there was

a tendency for the research reports to be shorter, with increasing emphasis on "safeguarding features of methodology" (p. 8), and "fewer cases of an outstanding level of performance" (p. 14). The statistical discussion focused on unjustified data selection and optional stopping. There was also an increasing emphasis of process-oriented issues such as position effects and an introduction of research on precognition. Critics were reluctant to publish their criticisms in the *JP*, so much of what appeared in the journal in this regard were surveys of criticisms published or presented elsewhere.

In the December 1946 issue J. B. Rhine (1946) wrote an editorial reflecting on the first 10 years of the *JP*. He bemoaned the facts that the journal operated at a deficit and was not widely read, implying that this was because even with the general articles the journal was a heavy read for the nontechnical reader. On the other hand, he acknowledged that scientific considerations needed to have priority. He also bemoaned that there had been "no overt recognition of the [*JP*s] contributions to science" (p. 222). In a more positive vein, he noted the expansion of the published research to cover precognition, psychokinesis, "pure telepathy," and personality correlates of ESP, and he reiterated the importance of the research on position effects.

The first two issues of the 1948 *JP* (Journal of Parapsychology, 1948) were devoted largely to a "symposium" consisting of 19 invited papers presenting the authors' views on "an ideal program for this field of research for the next decade" (p. 1). The authors included seven associated or previously associated with the Parapsychology Laboratory (Edmond Gibson, 1948; Pratt, 1948; Murphy, 1948; Margaret Reeves, 1948; Betty Humphrey, 1948; J. B. Rhine, 1948; J. L. Woodruff, 1948), four from elsewhere in the U.S. (Jan Ehrenwald, 1948; Jule Eisenbud, 1948; Hornell Hart, 1948; Schmeidler, 1948), and eight from Europe, mostly Britain (C. D. Broad, 1948; J. Hettinger, 1948; H. H. Price, 1948; S. G. Soal, 1948; Robert Thouless,1948; G. N. M. Tyrrell, 1948; Rene Warcollier, 1948; D. J. West, 1948). From 1949 through 1951 the *JP* featured multiple research reports by Soal and associates, and in the March 1954 issue West (1954) published a survey of parapsychological research in Britain.

The first issue of the 1949 *JP* was devoted to papers and discussion presented at a genuine symposium held by the Society for Parapsychology in Washington, DC. First were four formal papers presented in the morning session on statistical methods by Greville (1949), position effects in ESP and PK data by Pratt (1949), personality correlates of ESP by Schmeidler (1949), and ESP drawing tests by Humphrey (1949). The afternoon session consisted of a roundtable entitled "The Nature of Psi Processes"; the panelists were J. B. Rhine, R. A. McConnell, R. B. Roberts, Greenwood, C. B. Nash, Louisa Rhine, Jan Ehrenwald, and Montague Ullman. The symposium concluded with a paper by Murphy (1949) entitled "The Place of Parapsychology Among the Sciences."

In the March 1950 issue, J. B. Rhine (1950) published an editorial outlining various aspects of the *JP*'s publication policy. He again noted the tension between the need to make sure that research meets the technical scientific requirements of reporting and the need to make the articles readable. He argued that the editor had to give priority to the former requirement while doing as much as possible to meet the second. As for critical articles, Rhine said it was "dangerous" to reject them (p. 6). The editor will make critical comments on the manuscript, but it is up to the author to incorporate these points in a revision. Considerable attention was devoted to the thorny of issue of publishing chance results, and for the most part he justified rejecting them: "If the author has no illuminating observations or conclusions

to relate and no novel method to describe, the editors feel that there is not space or reader interest enough to justify the publication of a report unless there is the possibility of learning from such a failure" (p. 6). He went on to say that the conclusion that "failure to report a completely fruitless research somehow invalidates the conclusions drawn from significant studies that have been reported" is a "wrong impression" (p. 6). However, he insisted that all the results should be reported from individual studies. Finally, he noted that the results of minor studies cannot always be reported due to space limitations, even if the results are significant.

The December 1955 issue was devoted largely to discussion of an article in the prestigious journal *Science* by George Price (1955a), who attributed the significant card guessing results in Rhine's and Soal's experiments that could not be accounted for by conventional criticisms to, as Rhine put it in his introductory remarks, "fraud, mental abnormality or incompetence against the research personnel in parapsychology" (p. 238). Following were abstracts of Price's article and replies by P. W. Bridgman (1955), Paul Meehl and Michael Scriven (1955), J. B. Rhine (1955a, 1955b), and Soal (1955), followed by Letters to *Science* or the *JP* by James Crumbaugh (1955), Ralph Erickson (1955), Waldron Gardiner (1955), Gibson (1955), Jack Kapchan (1955), McConnell (1955), Ozanne (1955), and Kendon Smith (1955), as well as a brief reply by George Price (1955b) to the original critiques. The exchange ended with summary remarks by J. B. Rhine (1955c).

In the December 1956 issue, J. B. Rhine (1956) published an editorial reviewing the first 20 years of the *JP*. He began by revealing that a major original purpose of the journal had been "to keep the literature of experimental parapsychology as far as possible assembled in one central location" (p. 263). The rest of the editorial summarized how the scope of the journal had gradually expanded over the years. Whereas at first it was devoted exclusively to publishing evidence for the existence of ESP, it expanded to cover the different types of ESP (telepathy and precognition in addition to clairvoyance), followed in turn by theoretical articles, articles dealing with the personality correlates of ESP, reports of spontaneous cases (to help guide the experimental research), articles on the psychological factors that impact the psi process, and, finally, a "reopening of the question of sprit survival" (p. 264) although not as a primary issue.

The survival theme was reinforced in the March 1960 issue with a section on a symposium held at Duke in 1959 on how to deal with the problem of "incorporeal personal agency." The section began with a listing of the program, but only J. B. Rhine's (1960a) "Opening Remarks" and Louisa Rhine's (1960) paper were reproduced in the issue. The "Opening Remarks" provided a history of the involvement of J. B. Rhine and the Parapsychology Laboratory with the issue. As a justification both for the symposium and for research on the survival question, he noted that since the research on psi had [in his view] established the existence of an "extraphysical" component of personality, it is a logical next step to examine to what degree it may be independent of the physical component. J. B. Rhine (1960b) published a more extensive paper on this topic in the December 1960 issue, where he presented a critical review of past efforts to address the problem and offered suggestions for the future.

At the end of 1957, J. B. Rhine gave up the editorship of the *JP*. For the next 6 years the editors were Pratt, Louisa Rhine, and William Roll. This was followed by a 13-year period (1964 to 1976) in which the editors were Louisa Rhina and Dorothy Pope.

The survival theme was in a sense continued in the March 1961 issue with a section discussing a "symposium" on the question of the "physicality of psi." In the introduction section, J. B. Rhine (1961c) reported that he had invited readers to submit 800-1600 word articles addressing specific questions related to this issue. The introduction was followed by "Discussion" pieces by Broad, Cyril Burt, Pratt, and Scriven. In the September 1961 issue "Responses to the Forum" were published by the original contributor Scriven (1961), with replies by Pratt (1961a) and Wolfgang Ehrenberg (1961).

The June 1961 issue featured articles by C. E. M. Hansel explaining how he believed that what he considered to be the two best controlled and potentially evidential card-guessing experiments in the early days of the Parapsychology Laboratory could be explained by fraud—by the subject Pearce in the Pearce-Pratt experiment (Hansel, 1961a) and by one of the experimenters, Woodruff, in the Pratt-Woodruff experiment (Hansel, 1961b). The critiques were followed by a rebuttal by J. B. Rhine and Pratt (1961) in the former case and by Pratt and Woodruff (1961) in the latter. These were followed by an article on position effects in the Pratt-Woodruff series (Pratt, 1961b), which was a crucial component of the defense of that study. In his introduction to the exchange, J. B. Rhine (1961a) noted that he offered Hansel the option of resolving the dispute in private correspondence but Hansel insisted on publication.

For the December 1961 issue, J. B. Rhine (1961b) wrote a 10-page "review" of the first quarter century of the *JP*. I will refer only to those points that had not been made in earlier reviews. First he praised the major contributions of McDougall in the founding of the journal and the defining of its mission. We also learn that it was McDougall who suggested that the name "parapsychology" appear in the journal's title. Rhine noted that starting in 1942 specialty editors had been added on the editorial page, most notably statistical editors. Greenwood was the first of these, and in 1945 Greville was added. Also begun in 1942 was a book review section for "books of distinctly parapsychological character and of special significance to the field" (J. B. Rhine, 1961, p. 240). In 1946, a kind of newsletter called *Parapsychology Bulletin* was established. For 10 years it was officially an adjunct of the *JP*, but after a 1-year discontinuation it resumed in 1957 as a separate publication of the Parapsychology Laboratory until 1965. In 1958, publication of *Parapsychological Abstracts* commenced, intended "to cover all parapsychological publications—foreign as well as domestic—that would be of value to students and that were not reported in full in the JP" (J. B. Rhine, 1961, p. 241). A major event was the *JP* becoming affiliated with the recently formed Parapsychological Association (PA). Starting in 1958, the *JP* began publishing PA "news, programs and abstracts of convention papers" (J. B. Rhine, 1961, p. 241). Finally, Rhine noted that although the *JP* went out of its way to publish critical articles, this policy did not extend to a critic who in the judgment of the editor was "unqualified or not well enough informed to weigh and argue his criticism" (J. B. Rhine, 1961, p. 244) and he couldn't resist a dig at the bane of all journal editors, the "petty annoyances" (J. B. Rhine, 1961, p. 243) expressed by authors who had their papers rejected.

Beginning in 1969, reports of highly successful psi experiments on rodents by Walter J. Levy began to appear in the *JP*. Levy was incredibly successful in getting positive results in psi experiments during his short tenure at what was now the Foundation for Research on the Nature of Man (FRNM). The entire 1973 volume included no less than seven reports of Levy's experiments. But then in the June 1974 issue, J. B. Rhine (1974) had to report that members of Levy's research team had uncovered conclusive

evidence that Levy had created significant results by faking the data in at least one experiment. Levy confessed to Rhine and quickly resigned from FRNM. Although Levy claimed that only this one study was fraudulent, Rhine concluded that judgment on all Levy's research needed to be suspended and that the final verdict would depend on whether the research could be successfully replicated (which it generally was not).

Controversies about important psi experiments never seem to die, and in the June 1974 issue R. G. Medhurst and Scott (1974) published a reanalysis of the data in the Pratt-Woodruff experiment that they claimed supported Hansel's skeptical interpretation. Pratt (1974a) followed with an article challenging Medhurst and Scott's interpretation of the new findings. Responses by Scott (1974) and Pratt (1974b) ensued.

In the June 1975 issue, J. B. Rhine (1975) revisited the question of the *JP*'s policy of not publishing nonsignificant results. His first point was that that a nonsignificant study has no bearing on the interpretation of other, significant studies, so long as the studies are "independent." The second is that nonsignificant results are only informative if the author had previously produced significant results to which the nonsignificant results could be compared. He did, however, leave himself the out that a nonsignificant study might be published if the author could show exactly how the study produced useful information, although the implication was that this had never occurred in the past or was expected in the future.

The article prompted three Letters to the Editor in the March 1976 issue, one by John Beloff, Richard Broughton, and Brian Millar (1976), and the other by Dick Bierman (1976). The key argument in both papers was that Rhine ignored the fact that the significant result could be a type 1 error (de facto chance) and failed replications of the study is how this can be established. Thus, suppression of negative results can lead to an invalid interpretation of the significant studies, even if all the studies are independent. Beloff et al. further suggested that the policy would encourage authors to submit fraudulent data on the grounds that they would not fear publication of a failed replication. A third Letter, by Morgan Eades (1976), an academic psychologist, was also critical of Rhine's article, but made no substantive argument. J. B. Rhine (1976) replied to the Letters in the same issue. First, he reiterated the point that he would publish abstracts of the nonsignificant studies and send full copies to readers upon request. Rhine did not directly address the statistical argument, instead complaining that the critics wanted him to conform to the standards of "hard" sciences such as physics. This is not an argument the letter writers made. He also responded to a fraud argument that is not the one Beloff et al. made: that the motive for faking data would be that a study would have to produce significant results for it to be published. Rhine simply speculated in effect that it is unlikely the author would have this motivation.

In the June 1977 issue, J. B. Rhine (1977) published a paper announcing his retirement as editor of the *JP* (although, as noted above, he had not been listed as an editor since 1957). Beginning in that year and until 1982, the editor was listed as Dorothy Pope, with K. Ramakrishna Rao listed as co-editor. Pope was the only editor in the entire history of the *JP* who was not otherwise a researcher or academic and thus could devote full time to her editorial duties.

Rhine's 1977 article was largely an acknowledgement and appreciation of the role of McDougall in establishing both the Parapsychology Laboratory and the *JP* in a university, and in defining their re-

spective missions. McDougall's writings were extensively quoted. Toward the end of the article, Rhine speculated about what McDougall would think about how the journal had evolved during the years following his death in 1938. Among the speculations was that McDougall would have liked to see more contributions from authors of stature outside the normal parapsychological sphere and more attention to the relation of psi and parapsychology to other scientific fields and, especially, broader issues such as the mind-body problem. On a more nuts-and-bolts level, Rhine noted that a major problem faced by the field at the time of the *JP*'s founding was that researchers had difficulty getting their psi research published in psychology journals. Conversely, he bemoaned the long-term difficulty he had in keeping the journal filled with quality material, largely because authors outside the field were reluctant to publish in the *JP* either for political reasons (e.g., job security) or because a publication in a psychology journal would be more prestigious. However, he concluded that the *JP,* along with the other parapsychology journals, provided enough space to accommodate all publishable parapsychological investigations.

J. B. Rhine died in 1980, and the March 1981 *JP* was wholly devoted to articles describing his contributions to parapsychology. The first article was by his wife giving a personal perspective on his life and career, focusing on "the personality of J. B. Rhine and his scientific work" (L. E. Rhine, 1981, p. 5). It was followed by an article by Schmeidler (1981) discussing "Rhine's contributions to experimental methodology and his specific innovative methods, and show[ing] how he continuously maintained the highest possible research standards" (p. 11), and an article by John Palmer (1981) on the empirical evidence for ESP accumulated by Rhine and his colleagues. Palmer stated that his purpose was to "highlight the trends which seem to define the evolution of Rhine's approach to ESP research" (p. 25). This was followed by an article by Beloff (1981), the purpose of which was to "expound Rhine's teachings on the nature of psi" (p. 41). The next article, by James Hall (1981) addressed the implications of Rhine's work "for the study of theology and the practice of religion" (p. 55). In the final article, Brian Mackenzie (1981) discussed Rhine's place in the history of parapsychology, "the personal and professional factors involved in J. B. Rhine's major contributions to parapsychology" (p. 81).

In 1983, Rao became the editor of the *JP*, and in the March 1987 issue he published a series of articles devoted explicitly to the journal. The first article was an editorial in which he briefly reviewed the first 50 years of the *JP* and set an agenda for the next 50 years (Rao, 1987). The historical review did not say much that had not been said before, but he did note the multiple changes in the editorship of the *JP* over this period. The prospective section gave a very clear summary of his publication policy, which differed somewhat form Rhine's. Although the main focus would continue to be "on experimental research and empirical findings and their interpretation … due attention will go to field investigations, case studies, and speculative and scholarly papers of theoretical purport …." (p. 5). There would also be recognition of the interdisciplinary nature of psi research and inclusion of cross-cultural studies.

Following the editorial was publication of a lecture by Mauskopf (1987). Although the title of the published version referred to the origins of the *JP*, it was devoted in large part to the founding of the Parapsychology Laboratory and most of the points about the journal had been made in the previous papers about the *JP* that I describe above. He noted that at first Rhine wanted parapsychology research reports to be published in psychology journals, but that he changed his mind in part due to failure to get his own papers published in such journals. He then decided to establish the *JP* as "an ongoing publica-

tion agency that would be immune from the vagaries of outside agencies and priorities" (p. 15). An article by Broughton (1987) on *JP* publication policy took an historical approach and expanded on points made in previous *JP* articles on the topic and reviewed above. Not surprisingly, he devoted considerable space to reviewing in a matter-of-fact way the debate on Rhine's policy of not publishing nonsignificant results. He also noted that there was backlash to Rhine's policy, most notably a denunciation of the policy (although not mentioning the *JP* by name) in a statement by the Parapsychological Association. Finally, Broughton noted that Rao had expanded the use of external referees. Broughton's paper was followed by an exhaustive review by Palmer (1987) of the controversies that had appeared in the *JP* over the years. It included elaborations of the exchanges that I touch upon briefly earlier in this paper.

The March 1985 issued featured the first of a series of debates about the evidential status of free-response ESP research using the ganzfeld procedure. Following an introduction by editor Rao (1985), Ray Hyman (1985) published a critique of the ganzfeld experiments conducted up to that time, citing specifically reporting biases, multiple analysis artifact, and various procedural flaws, two of which correlated significantly with study outcomes. He concluded with a meta-analysis demonstrating that the successful replication rate is about what would be expected by chance. Charles Honorton (1985) published a meta-analysis of a subsample of ganzfeld experiments to which the multiple analysis artifact did not apply. He found that the results of these studies were highly significant. He challenged Hyman's flaw assignments and none of his own assignments correlated significantly with study outcomes. In the December 1986 issue, Hyman and Honorton (1986) co-authored a "joint communiqué" in which they agreed that the results could not be attributed to chance but disagreed about whether the flaws could account for the significance. Finally, they agreed that the final verdict would depend on the results of future independent replications. The communiqué was sandwiched between 10 other articles commenting on the controversy, by James Alcock (1986), Irvin Child (1986), Gerd Hövelmann (1986), James McClenon (1986), Palmer (1986), Robert Rosenthal (1986), Scott (1986), Rex Stanford (1986), Douglas Stokes (1986), and Jessica Utts (1986).

In 1992, Honorton died suddenly at a young age. His prominence is attested to by the fact that the March and June 1993 issues of the *JP* were devoted to a review of his career and achievements. Following an introductory paper by Rao (1993) and a personal appreciation and reminiscences by one of Honorton's closest friends, the statistician Donald McCarthy (1993), several articles gave a historical overview of various stages of Honorton's career. James Carpenter (1993) wrote about the time Honorton, Carpenter, and other younger parapsychologists were together at FRNM. Stanley Krippner (1993) reviewed his contributions to the famous Maimonides dream experiments. Mario Varvoglis (1993) reviewed psi research on the ganzfeld procedure and random number generators that Honorton had conducted as Principal Investigator at Maimonides. Ephraim Schechter (1993) then reviewed the more advanced and groundbreaking autoganzfeld research that Honorton conducted at his own laboratory, Psychophysical Research Laboratories. Finally, Robert Morris (1993) discussed Honorton's last days at the University of Edinburgh, where he had enrolled to finally obtain an academic degree.

Other articles, although still historical, were somewhat more topic-focused. Broughton's (1993) article focused on Honorton's methodological contributions, especially the innovative computer games he developed to test psi. Daryl Bem (1993), who coauthored the important autoganzfeld meta-analysis

that was published in the prestigious *Psychological Bulletin*, reviewed Honorton's contributions to ganzfeld research and the controversy surrounding it. Statistician Utts (1993) discussed Honorton's contributions to meta-analysis in parapsychology, and Stanford (1993) reviewed Honorton's theoretical and empirical contributions to process-oriented psi research. Palmer (1993) wrote a paper on Honorton's responses to critics of psi research, most notably Hansel's allegations of fraud in prominent psi experiments, as well as mostly statistical critiques by James Kennedy, and, of course, the ganzfeld debate with Hyman. The final two papers were the republication of a broadside against critics of parapsychology by Honorton (1993) himself and a bibliography of his published work by Carlos Alvarado, Rhea White, and Nancy Zingrone (1993).

The December 1989 and March 1990 issues featured a debate among statistician Bernard Gilmore, Utts, and Palmer on problems concerning target randomness in psi experiments, especially RNG experiments. Gilmore (1989, 1990) argued that the target sequences used in psi experiments are never truly random: "It is the nature of the concept of randomness that no guarantees of randomness are possible" (p. 329). Thus, participants who receive trial-by-trial feedback could detect the nonrandomness and use it to inflate their scores. He also questioned the validity of standard statistical tests to determine the significance levels and suggested Monte Carlo procedures be used instead. Utts (1989) and Palmer (1989, 1990) argued, among other things, that Gilmore never gave a plausible explanation of how the levels of nonrandomness he claimed could influence the results of actual psi experiments. Even if Gilmore were right, any nonrandomness would be so subtle that one could not reasonably expect a participant to detect it.

The December 1999 issue was devoted almost exclusively to an extension and expansion of the ganzfeld debate. A target article by Julie Milton (1999) drew upon an update of a recent ganzfeld meta-analysis by Milton and Wiseman that concluded that the studies aimed at replicating Honorton's successful autoganzfeld studies failed to yield significant overall evidence of psi. She went on to point out methodological flaws that could have accounted for successful meta-analyses of ganzfeld and other methodological paradigms, with particular reference to what she considered to be deficiencies in the quality codes that had been correlated with study outcomes, such that they overestimated the quality of the studies coded. The article was followed by a running commentary on Milton's paper, consisting of 90 messages from 22 commentators (Schmeidler & Edge, 1999). The commenters were not identified by name until after the 3-week "debate." The subtopics were "Critiques and Defenses of Milton's Conclusion," "Criticisms and Defenses of the [Milton] Presentation and Its Omissions," "Definition of Ganzfeld," "Future Research on the Ganzfeld," "Is Psi Real?" and "The Value of the Debate."

In 1994, Palmer became editor of the *JP*. Starting in 2000, following a requirement from the PA, he began publishing written versions of Invited Addresses (including Presidential and Banquet Addresses) that were submitted by their authors. Through 2002, with rare exceptions, there continued to be four issues of the journal published each year, listed as March, June, September, and December. To facilitate reducing the almost 1-year publication lag that had developed over the years, in 2003 Palmer shifted to two fatter issues per year, Spring and Fall. To further bring in line the listed and actual publication dates, Palmer published two single issues (Spring/Fall 2008 and Spring/Fall 2009) that were of approximately the same length as single issues; after this the *JP* reverted to two issues

per year. This reduced the publication lag to about 3 months and by the time Palmer retired in 2016 it had been reduced to 1 month.

In the 2009 issue, Palmer resumed the practice of publishing editorials in some but not all issues. Some of the editorials were "guest editorials" written by other parapsychologists. In the introduction to the first editorial, Palmer noted that "I envision them as thought-provoking opinion pieces addressing fundamental issues facing contemporary parapsychology" (Palmer, 2009, p. 3).

To commemorate the 75th anniversary of the journal, a special issue was published in December, 2012 (Journal of Parapsychology, 2012). A diverse sample of leading parapsychologists was invited to submit short essays addressing the question "Where will parapsychology be in the next 25 years?" This resulted in 28 essays by 32 authors and co-authors. These were followed by a contribution from Palmer that summarized the essays and gave his own answer to the question. Most of the essays focused on recommendations, more like where parapsychology *should* be in 25 years.

Finally, in the Fall 2016 issue there appeared a special book review section occasioned by the nearly simultaneous publication of two anthologies taking opposite positions on the mind-body problem: *Beyond Physicalism (BP)*, edited by Edward Kelly et al., and *The Myth of An Afterlife (MoA),* edited by Michael Martin and Keith Augustine. The idea was to create a debate focused on the issue of post-mortem survival by having Stokes (2016a) write a critical review of *BP* and James Matlock (2016a) write a critical review of *MoA*. Kelly (2016) and Augustine (2016), the latter supplemented by Claus Larsen (2016) and Ingrid Smythe (2016), then wrote replies to the reviews, followed by replies to the replies by Stokes (2016b) and Matlock (2016b). Palmer (2016) then wrote a discussion section giving his perspective on the points made by the other contributors. The content of the debate was largely philosophical, specifically referencing the competing ontologies aimed at solving the mind-body problem (e.g., materialism, dualism) with secondary reference to the empirical evidence for survival.

In early 2017, I retired as editor of the *JP* and was replaced by Etzel Cardeña, who described the changes he envisions for the journal in an editorial in the Spring 2017 issue (Cardeña, 2017). The major substantive change will be an expansion in the scope and coverage of the journal. The most important of these changes is an increased emphasis on papers addressing anomalous experiences per se and states of consciousness, studied by non-experimental (presumably qualitative/phenomenological) methods. This adjustment is necessary if for no other reason than to reflect a rapidly evolving change in the primary research objectives (I would go so far as to call it a paradigm shift) within the field of parapsychology, a development I personally find disturbing.

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80 Années du *Journal of Parapsychology:* Un Tour d'horizon Historique

Résumé : Dans cet article invité, l'auteur passe en revue l'histoire du *Journal of Parapsychology* depuis son point de départ en 1937 jusqu'en 2017. L'accent est mis sur les controverses et les débats avec les critiques extérieures au champ de la parapsychologie qui y furent publiés, ainsi que sur la politique du publication du *JP* et les changements de comité éditorial.

80 Jahre *Journal of Parapsychology*: Ein historischer Überblick

Zusammenfassung: In diesem eingeladenen Artikel beleuchtet der Autor die Geschichte des *Journal of Parapsychology* von seiner Gründung 1937 bis 2017. Der Schwerpunkt liegt auf veröffentlichten Kontroversen und Debatten mit Kritikern von außerhalb der Parapsychologie, der Publikationspolitik des *JP* und den Veränderungen in der Herausgeberschaft.

80 Años del *Journal of Parapsychology*: Un Panorama Histórico

Resumen: En este artículo, el autor pormenoriza la historia del *Journal of Parapsychology* desde su inicio en 1937 hasta 2017. El foco son las controversias publicadas y debates con los críticos externos a la parapsicología, la política de publicación del *JP*, y los cambios de editores.

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Eight Decades of Psi Research: Highlights in the *Journal of Parapsychology*

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Abstract: This is a short review of the 80 years of existence of the *Journal of Parapsychology*. Founded in 1937, the journal articulated the experimental research program of J. B. Rhine and his associates at Duke University. Highlights of the journal are discussed, starting with examples of articles reporting experiments of extrasensory perception and psychokinesis. Also discussed are articles about spontaneous cases, the presentation of novel and creative approaches, critiques and discussions, overviews of the field, J. B. Rhine's use of the *Journal of Parapsychology* to prescribe for the field, and concepts and theories. The *Journal of Parapsychology* is seen as an important influence in the development of parapsychology.

Keywords: Journal of Parapsychology; J. B. Rhine; experimental parapsychology; spontaneous case studies; influence of journals

This anniversary of the publication of the *Journal of Parapsychology* brings to mind the works of many authors over the years who have contributed to the development of modern parapsychology. In this paper I present a few examples of various topics discussed throughout the life of the *JP* hoping to illustrate the importance of the journal for parapsychology.[2]

Experimental Research

Following the appearance of J.B. Rhine's *Extra-Sensory Perception* (1934), the *JP* was founded in 1937 mainly to publish experiments conducted with ESP cards. The opening editorial published in the first issue of the journal stated:

> Parapsychology is a word that comes to us from Germany . . . We think it may well be adopted into the English language to designate the more strictly experimental part of the whole field implied by psychical research . . . We do not claim that any sharp line can be drawn marking off the field of parapsychology within the larger vaguer province of psychical research. Rather, we anticipate that the stricter experimental methods will gradually invade other parts of the province annexing them to their own more special field, until possibly the two shall coin-

1 I am grateful to the journal's editor for inviting me to submit this paper I wish to thank Nancy L. Zingrone for editorial suggestions. James Carpenter kindly gave me information about some of his papers. Address correspondence to: Carlos S. Alvarado, Ph. D., Parapsychology Foundation, P.O. Box 1562, New York, NY, 10021, carlos@theazire.org.

2 The discussion below is but a brief overview of selected topics from the *JP* for the 1937–2017 period. To keep the discussion short, I focus on articles, to the neglect of correspondence and book reviews. More details on the topic appear in an article I wrote for the *Psi Encyclopedia* (Alvarado, in press).

> cide. But we regard the differentiation of the two terms as useful at the present time; and it is our intention to admit to this journal only contributions that properly fall within the narrower sphere implied by its title; that is to say, reports of experimental studies in the stricter sense and discussions of methods and interpretations of such work. (McDougall, 1934, p. 7)[3]

Illustrating this agenda, during the first ten years of the *JP* 52% of the articles published were reports of experiments. In contrast, only 11% of the total number of papers that appeared in the other important American journal, the *Journal of the American Society for Psychical Research,* were experiments (Zingrone, 1988, p. 332). These were highly exploratory days in which many topics were investigated to learn more about ESP functioning. This included such varied things as comparison of different techniques to test for ESP (Gibson, 1937), and of various distances from the targets (J. B. Rhine, 1937a). Regarding distance, Rhine (1937) concluded:

> The work already reported seems to show that distance does not limit ESP as it does sensory perception. Particularly those Duke experiments known as the Pearce-Pratt and the Turner-Ownbey series demonstrate this by reason of the control series needed for comparison. In a total of 101,450 trials in which distance was a condition and which gave as a whole a significant deviation, there was shown in the two series in which there were score averages at different distances that there was no proportionate falling off of average with distance, such as a mechanical analogy would require. In fact, it would appear that no falling off whatever directly occurs with spatial separation. (p. 184)

These initial explorations include different types of participants, such as the mentally retarded (Bond, 1937), twins (Kubis & Rouke, 1937), and the blind (Price & Pegram, 1937), as well as the impact of psychological variables on ESP scoring. Among these were intelligence (Humphrey, 1945) and the relationship between participants and experimenters (Pratt & Price, 1938). Explorations of psychological correlates of ESP are still conducted, having been recorded in the *JP* more recently as can be seen in Honorton's (1997) ganzfeld report and in other work (Haraldsson, Houtkooper, Schneider, & Bäckstrom, 2002) as well.

Starting in the 1940s, the *JP* also became the repository of various reports about psychokinesis (PK) using dice, many of which had remained unpublished from the 1930s.[4] The abstract of the first report on the subject, authored by J. B. and Louisa E. Rhine (1943), reads:

> The test procedure consisted of dice-throwing, in which a pair of common dice was thrown either by hand or by a semi-mechanical method. The objective was to cause them to come up as "high dice," i.e., with faces totaling 8 or above. A "run" consisted of 12 throws of the pair and the expectation for each run was 5 successes (8 or above); the average score obtained for the 562 runs that were made was actually 5.53. This represents a total score that is 300 hits above the total expectation from chance . . . , which represents extremely high odds against the likelihood of such results occurring by chance. (p. 20)

3 This editorial, which was not signed, was attributed to William McDougall (Mauskopf & McVaugh, 1980, p. 147).
4 For details about the development of the early PK dice work, see L. E. Rhine (1970).

Like the ESP work, soon there were studies published to try to understand PK via the explorations of the effects of other variables on PK scoring. Among these studies were those that investigated the effects of alcohol on PK test performance (Averrill & Rhine, 1945), and the impact on scoring of different sizes of dice (Hilton, Baer, & Rhine, 1943).

The topic of PK was less popular in the *JP* than ESP, but it continued to appear in later work. Edward Cox's (e.g., 1965) work with various devices, Helmut Schmidt's (1970) use of random number generators, and more recent tests exploring mental strategies and feedback (e.g., Roe & Holt, 2006) are examples of these type of experiments. The modern period also included work with biological targets (e.g., Braud & Schlitz, 1983). All these explorations, and particularly the early ones published during the 1930s and the 1940s, were fundamental for the development of modern experimental parapsychology.

Work with Spontaneous Cases

The articles of Louisa E. Rhine about ESP cases are perhaps the best-known publications about spontaneous cases in the history of the *JP*. Even though she did not consider that cases could provide evidence for the existence of ESP, she explored her cases — mainly obtained through correspondence to Duke University's Parapsychology Laboratory — for the purpose of identifying testable hypotheses for experiments.[5]

In an early paper about the types of ESP experiences, L.E. Rhine (1953) wrote:

> Four main types were found: (1) Intuitive, in which the subject's experience was a simple, unreasoned impression or hunch. (2) Hallucinatory, in which the experience was projected as if it were a sensation. (3) Unrealistic dreaming, in which the experience was characterized by fantasy. (4) Realistic dreaming, in which the imagery was almost photographically realistic. (p. 77)

Rhine continued her analyses over the years. She considered such topics as different types of precognition (L. E. Rhine, 1954), veridical hallucinations (L. E. Rhine, 1956), and completeness of information in dream and waking experiences (L. E. Rhine, 1962a, 1962b).

Over the years, several other papers about spontaneous experiences appeared. A particularly influential paper, important for the modern history of poltergeist research, was Pratt and Roll's (1958) report of a case in Seaford, Long Island. In this paper the authors introduced the term recurrent spontaneous psychokinesis (RSPK) to refer to poltergeists. This was followed in later years by other reports of poltergeists and hauntings (Kruth & Joines, 2016; Maher, 2000).

More recently questionnaire surveys about a variety of psychic experiences in relation to experimental ESP scores and personality variables (Haight, 1979), and psychoactive drugs (Luke & Kittenis, 2005) have been published.

5 All this work was summarized years later by L. E. Rhine (1981), and by Weiner and Haight (1983). It is debatable if this work was really useful to generate testable ideas to conduct experiments.

Novel and Creative Approaches to Research

Among the many research reports published in the *JP*, there were some that may be considered to be particularly creative due to their innovative research approaches. An early example was a protocol for the study of "pure" telepathy by Elizabeth McMahan (1946). As she described her procedure:

> An ESP experiment was conducted in such a way as to allow telepathy to function, but not clairvoyance or precognition. The sender thought of one of the ESP symbols without making any objective record of it except through a code number, while the receiver in another room tried to identify the symbol. The code was communicated to an assistant experimenter by means of reference to common memories which had no objective basis and hence were inaccessible to clairvoyance. Significant results were obtained. (p. 224, abstract)

Other innovations were studies attempting to influence cats conducted by Karlis Osis (1952), which, even if unimpressive today for its attempts to control for sensory cues, represented an innovative step forward in the study of animal ESP at the time. Several fascinating study designs developed over the decades include experiments to assess whether the identity of the person checking results could affect ESP scores (Feather & Brier, 1968), the use of random event generators (Schmidt, 1969), and the application of repeated guessing and majority vote analysis to detect Morse encoded words as ESP targets (Carpenter, 1991).

Another creative approach was the use of anesthetized mice as targets in attempts to test the possibility of psychic resuscitation (Watkins & Watkins, 1971). As the authors wrote in their abstract:

> Twelve subjects (nine of them professed "psychics" or known to be exceptional performers on PK or ESP tests) were tested for their ability to cause mice to arouse more quickly from ether anesthesia than normally would be expected. Pairs of mice (Swiss-Webster) were simultaneously rendered unconscious .... with ... ether. The pairs were of the same sex, comparable size, and were litter mates. After both mice were unconscious, ... the subject was told to attempt to awaken his or her mouse. The other mouse was used as a control . . . The results were highly significant overall ... , the experimental animal requiring 87% as much time to awaken as the control. (Watkins & Watkins, 1971, p. 257)

Critiques and Discussions

Criticism has appeared in the pages of the *JP* from its early days, an example of which was a discussion of the problem of premature stopping of experiments (Lemmon, 1939). In later years, space was provided in the journal to other criticisms such as those arguing for conventional explanations of experimental results on the basis of problems with the randomization of targets (Gilmore, 1989). In fact, a summary of the well-known critique of ESP research authored by George R. Price in *Science*, in which he suggested the possibility of experimenter fraud, was published as well in the *JP* (Price, 1955).[6] The editor of the *JP* also included replies to Price that had been published first in *Science*. The willingness to entertain criticisms seriously was the way the *JP* made the point that parapsychology is open to criti-

6 Price later changed his mind and apologized publicly (Price, 1972).

cism, but that criticism might be effectively countered as well. Later critics in the journal included C.E.M. Hansel (1961) and Richard Wiseman and Julie Milton (1998).

In fact, from its early days, the *JP* has been an effective forum in which to respond to criticisms of parapsychological research, as can be seen in Charles Stuart's rebuttal published in 1938. Stuart concluded that the criticisms of the Duke work by psychologists could be improved. He wished for a more systematic examination of the data, hoping that a "willingness to face the issues directly in terms of the evidence and the experimental methods available the present difference between critical and experimental conclusions might be happily resolved" (p. 320).

Other reply to critics included J.B. Rhine and J.G. Pratt's (1961) defense of the Pearce-Pratt experiment. More recently, the *JP* republished an important paper by Charles Honorton (1993) in which he summarized, and critiqued both the assumptions and main arguments of the critics of experimental research in parapsychology, finding them to be lacking.

Also important were papers whose two authors, one a "proponent" of the field (Honorton, 1985), and the other a critic (Hyman, 1985) defended and criticized ganzfeld ESP research. The interaction of these two individuals produced a joint article that, at the time, was considered to have provided a constructive opportunity for dialog and a possible guide for further research collaborations (Hyman & Honorton, 1986). Unfortunately, these efforts, and later special issues of the *JP* devoted to discussing ganzfeld work (December, 1986, and December, 1999), did not significantly bring more acceptance of the research work.

Charting the Field

An important function of the *JP*, and of other journals, has been to help us understand the state of the field, in terms of findings and problems, mainly done through overview papers. A very interesting, but now forgotten one was a summary of comments from the literature about variables that favor the appearance of ESP (Smith & Gibson, 1941). Among the topics considered were concentration, disposition, distraction, excitement, interest, mood, personality, and relaxation.

This was followed in later years by reviews of many topics. Among these were examinations of veridical perception during out-of-body experiences (Alvarado, 1982), and of experimenter effects (Kennedy & Taddonio, 1976), among others.

More than other parapsychology journals, the *JP* has been a pioneer in the publication of meta-analyses on various lines of research, among them, ESP in the ganzfeld (Honorton, 1985), precognition (Honorton & Ferrari, 1989), a comparison of clairvoyance and precognition conditions (Steinkamp, Milton, & Morris, 1998), and forced choice ESP tests (Storm, Tressoldi, & Di Risio, 2012).

Many productive and informative literature review papers published by the journal were not focused on research. Examples of this type of papers included work related to psychiatry (Eisenbud, 1949), the language barrier in parapsychology (Alvarado, 1989), parapsychological developments in Ibero-America (Rueda, 1991), and a sociology of science approach to controversy in the field (Zingrone, 2002).

J. B. Rhine's Use of the *JP*

Academic journals are well known for their sometimes quite intentional influence on their fields accomplished by the emphases its editors place on specific approaches, methodologies, and conceptual frameworks. From this perspective, it has been stated that J. B. Rhine "tried to chart the course of the field [in the *JP*] by dispensing his advice, his approval and disapproval, as he sought to shape parapsychology according to his views and priorities" (Alvarado, 2011, p. 95). His particular method of influence were embodied in Rhine's numerous editorials.

These editorials gave prescriptions about the coining of terms in the field (J. B. Rhine, 1945), the application of the scientific method (J. B. Rhine, 1947), and the proper use of spontaneous cases (J. B. Rhine, 1948). Regarding the latter, Rhine said:

> Because of the elusive nature of ESP phenomena, it has not been easy to follow up experiences with experiments which would produce evidential results on demand. Accordingly, there has been a great deal of effort to recruit and dress up these spontaneous experiences themselves as evidence of reliable character, but all attempts to authenticate the reports well enough to allow reliable conclusions to be drawn have been comparatively unsuccessful. The result of this abuse of the case report has been a misunderstanding of its true significance and usefulness . . . If, however, the spontaneous experience can be given its proper role, it can play a great part in the researches ahead, perhaps a crucially important one. Such material can help us to initiate new experimental approaches, not any longer to establish psi, but to find out more about what it is and how it operates (p. 232).

Rhine was also concerned about the boundaries of parapsychology, that is, what was and what was not part of the subject matter of the field. Regarding Kirlian photography, he wrote that because it "has not been reliably reported to have anything to do with psi" it should not be a topic for the field (Rhine, 1972, p. 171). Similarly, he discouraged interest in what he felt the scientific method could not solve. This included such topics as survival of death (J.B. Rhine, 1974).

Of course, over the years, many other authors have presented their views and suggestions about many topics of interest in the field (e.g., Alvarado, 2002; Murphy, 1948),[7] but my impression is that no other author exceeded Rhine in this, and no one used editorials in the same way. In recent times, though, the current editor of the *JP* has started to contribute some thought-provoking editorials (e.g., Cardeña, 2017).

Theoretical Issues

The *JP* was not known for including many theoretical discussions. But the topic was not completely neglected, as can be seen in early articles about ESP (Reiser, 1939; Saltmarsh, 1942). Thouless and Wiesner (1948) wrote that ESP and PK are

7 There have been special issues of the *JP* in which many individuals presented their opinions and recommendations about parapsychological research (March, 1948; June, 1948; December 2012).

> unusual forms of processes which are themselves usual and commonplace, and that in their usual and common place form, they are to be found as elements in the normal processes of perception and motor activity . . . The hypothesis we wish to suggest is that, *in normal thinking and perceiving I am in the same sort of relation to what is going on in the sensory part of my brain and nervous system as that of the successful clairvoyant to some external event, and that this relation is established by the same means* . . . (pp. 195–196).

Other models have been proposed in the journal regarding out-of-body experiences (Blackmore, 1984), precognition (Marwaha & May, 2015), and other topics (e.g., May, Utts, & Spottiswoode, 1995). It was in the *JP* where James Carpenter (2004, 2005) first presented his First Sight model.[8] He wrote:

> This model assumes that each organism, by its nature, extends beyond itself into the larger pre-sensory surround. Psi is assumed to be neither knowledge nor action, but to belong to the outermost temporal edge of those normal pre-experiential mental processes by which the mind structures all its experiences and commences all its actions. Psi processes are posited to function normally as the unconscious leading edge of the development of all consciousness and all intention. This unconscious functioning is normal and continuous, and is a constituent element of all experience . . . It is assumed that unconscious mental processes, including psi processes, are motivated by personal intentions and needs, also largely unconscious. (Carpenter, 2004, p. 217).

Unlike other journals, the issue of survival of death has not received much attention, but there have been important exceptions, as seen in the writings of J. B. Rhine (1956, 1974). During the 1960s, seven papers appeared in the *JP* first presented in 1959 at a conference sponsored by the Parapsychology Laboratory called "Symposium on Incorporeal Personal Agency" (e.g., Roll, 1960).

There were also some debates that took place in correspondence, such as that of Hornell Hart (1957) and Louisa Rhine (1957). Exchanges like these were valuable in that they illustrated the different conceptual assumptions behind phenomena considered by some to be evidence of survival of death.

Concluding Remarks

I am afraid that my attempt at keeping this article short, as commissioned, may not have done justice to the richness of the content of the last 80 years of the *JP*. In addition to mentioning only a small number of papers, I have not covered at all some areas and topics. Among the areas omitted are articles about the future of the field, discussions of the history of parapsychology and of the beginnings of the journal, and the participation of non-American authors in the journal. Furthermore, more could be said about the disciplinary approach to the field's phenomena and theory as represented by authors from psychology, physics, philosophy, and other scientific and scholarly perspectives.

The appearance of the *JP* represents a change from the psychical research tradition that existed in the United States and elsewhere before the late 1930s, which was dominated by the study of cases and of mediumship (Inglis, 1984). Although the research program of J.B. Rhine and his associates was

8 For a full discussion of the model see Carpenter (2012).

to some extent a reinstatement of earlier interest in experimentation, the *JP* greatly assisted the development of parapsychology. This was accomplished by providing a forum that assisted processes such as the standardization of techniques to assess chance, controls for contaminating factors such as sensory cues, and terminology in parapsychology (on this later issue see Zingrone & Alvarado, 1987)[9]. Like every good scientific journal, the *JP* also facilitated communication between researchers and others in the field helping to disseminate ideas and encourage professional attitudes. The presentation of information, in the form of reviews of the literature, and book reviews (not discussed in this paper) has made the journal an essential reference source over the years for researchers, students, and others. One hopes that this tradition of excellence and dedication continues beyond this anniversary as parapsychology moves to new horizons.

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9 Although it has to be recognized that other journals also affected the development of parapsychology (Alvarado, Biondi & Kramer, 2006), none of them has been so influential on statistically-evaluated experimental research in the modern period as the *JP*.

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Huit décennies de recherche psi : Faits saillants du *Journal of Parapsychology*

Résumé : Cet article est un bref passage en revue de 80 années d'existence du *Journal of Parapsychology*. Fondé en 1937, le journal servait de tribune au programme de recherche expérimentale de J. B. Rhine et ses associés à l'Université de Duke. Les faits saillants du journal sont discutés, à commencer par des exemples d'articles reportant des expérimentations de perception extra-sensorielle et de psychokinèse. Sont également discutés des articles sur des cas spontanés, la présentation d'approches nouvelles et créatives, des critiques et des discussions, des vues d'ensemble du champ, ainsi que la façon dont J. B. Rhine utilisait le *Journal of Parapsychology* pour faire des prescriptions quant à l'évolution de la discipline, en y introduisant des concepts et des théories. Le *Journal of Parapsychology* y apparaît comme une influence importante dans le développement de la parapsychologie.

Acht Jahrzehnte Psi-Forschung: Höhepunkte im *Journal of Parapsychology*

Zusammenfassung: Dies ist ein kurzer Überblick über das 80jährige Bestehen des *Journal of Parapsychology*. Die 1937 gegründete Zeitschrift war das Sprachrohr für das experimentelle Forschungsprogramm von J. B. Rhine und seinen Mitarbeitern an der Duke University. Höhepunkte der Zeitschrift werden diskutiert, beginnend mit Beispielen von Artikeln, die über Experimente zur außersinnlichen Wahrnehmung und Psychokinese berichten. Diskutiert werden außerdem Artikel über Spontanberichte, die Vorstellung neuer und kreativer Forschungsansätze, Kritiken und Diskussionen, Übersichten über das gesamte Gebiet, J. B. Rhines Nutzung des *Journal of Parapsychology*, um das Forschungsfeld zu umreißen einschließlich der Konzepte und Theorien. Die wichtige Bedeutung des *Journal of Parapsychology* für die Entwicklung der Parapsychologie wird betont.

Ocho Décadas de Investigación: Lo Más Destacado en el *Journal of Parapsychology*

Resumen: Esta es una breve reseña de 80 años de existencia del *Journal of Parapsychology*. Fundada en 1937, la revista articuló el programa de investigación experimental de J. B. Rhine y sus asociados en la Universidad de Duke. Se discuten los aspectos más destacados de la revista, comenzando con ejemplos de artículos que describieron experimentos de percepción extrasensorial y psicoquinesis. También se mencionan artículos sobre casos espontáneos, la presentación de enfoques novedosos y creativos, críticas y discusiones, descripciones generales del campo, el uso de J. B. Rhine del *Journal of Parapsychology* para dirigir el campo, y conceptos y teorías. El *Journal of Parapsychology* ha sido una influencia importante en el desarrollo de la parapsicología.

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Some Basic Experiments in Extra-sensory Perception: A Background[1]

Joseph Banks Rhine

[Original] Editorial Note: ...This extract is taken from an article entitled "Some Selected Experiments in Extra-Sensory Perception" published by J. B. Rhine in the *Journal of Abnormal and Social Psychology* for September, 1936.

EXTRA-SENSORY perception, under the headings of "telepathy" and "clairvoyance"[2], is not new to science, nor to its psychological branch. Many psychologists have given active attention to it[3], and some have contributed to the evidence for its occurrence. Among the latter are Bechterew[4], Brugmanns[5], Coover[6], Estabrooks[7], and Janet[8]. Of these, Coover apparently did not recognize his own contributions at the time, but several critics have since agreed that his results are positive, while Janet has simply remained silent regarding his findings, allowing others to interpret them. The Danish psychologist, Lehmann, who, with Hansen, offered the only serious criticism of the early telepathic studies, later came hirnself to be convinced of its actual occurrence.[9]

The most extensive study of telepathy and clairvoyance yet made was begun at Duke University in 1930. The first two years of experimentation were described in a brief report entitled "Extra-Sensory Perception of the Clairvoyant Type"[10], and the first three years were covered by a monograph, "Extra-Sensory Perception", published in 1934.[11] Another report gives the results obtained with a special subject, a professional medium.[12]

The reports mentioned submit the results of well over 100,000 trials made in clairvoyant card-calling and in telepathic thought-transmission. The experiments in which the better conditions obtained seem to allow no other interpretation of the results, and appear to demonstrate an extra-sensory mode

1 [Originally published in *JP, 1937, 1,* 70-80.]
2 Telepathy is the perception of another's mental state by other than the known sensory means; and clairvoyance, perception of objects without sensory action. "Extra-sensory perception" includes both.
3 For a more extensive review see J. B. Rhine, *Extra-Sensory Perception,* (Boston: Bruce Humphries, 1934) , Chap. 2.
4 *Zeifschrift fur Psycho-Therapie,* VII I (1924) , 280-304.
5 "L'etat passif d`un telepathe controle par le phenomene psychogalvanique," *Comptes Rendu du IImeCongres International de Metapsychique,* (Warsaw :1923) .
6 Experiments in Psychical Research, *Psychical Research Monographs,* (Stanford: 1917), No. 1.
7 *A Contribution to Experimental Telepathy,* (Boston Society for Psychic Research, *Bulletin V,* 1927) .
8 For selected instances and references see Richet, *Thirty Years of Psychical Research,* (London: Macmillan and Company, 1922); and *History of Psychology in Autobiography,* ed. Murchison (1930) , Vol. I, p. 125.
9 After the above was written, the report by Dr. Bender of his experiment in clairvoyant perception appeared. See Dr. D. K. Adarns' article on Bender's work in this issue, p. 63.
10 *Journal of Abnormal and Sociol Psychology*, XXIX, No. 2, (1934), 151-171.
11 Rhine, *op. cit.*
12 *Character and Personality*, III, No. 2, (1934), 91-111.

of perception. Evidence is offered not only of telepathic and clairvoyant perception, but of the natural character of these capacities. That they have some relationship to certain known functions is shown by the study, and a close interrelationship between the two, telepathy and clairvoyance, is indicated, if not established.

The purpose of this article is to give a brief account of some selected experiments in which both good conditions prevailed and good scores resulted, along with a full account of conditions maintained. and a report of controls and safeguards used. The selections made are chosen to meet the various possible questions or alternative hypotheses that we have to consider in this work. These fall under the following headings: For the clairvoyance tests, the question of sensory cues on the cards used, or of poor shuffling; for the telepathy work, the question of similar habits of order of selection made by the agent and the percipient in choosing and calling the symbols used. And, finally, for both clairvoyant and telepathic perception tests, the question of whether chance alone can explain the results obtained. While these results are selected from our present stock of several hundred thousand trials, they are taken fairly, including all the consecutive trials made under the conditions stated. A given block of data is taken intact, without any omission of results within the series. There was no "selection" in the improper sense of the word.

GENERAL METHODS

The tests for clairvoyance were all made with cards hereinafter called "ESP Cards". These were used in packs of twenty-five, each pack containing five each of five simple designs: ○ + ≋ □ ☆ (star, plus, waves, rectangle, and circle). The cards were cut from white heavy cardboard which was opaque to light from a 100-watt light bulb. The figures were stamped in fountain pen ink with rubber stamps which left no detectable warping or other tactual stimuli.

In the tests for telepathy, the same symbols were used, but, to exclude the possibility of clairvoyance, the mental images of them alone were used without any objective representation until after the test when the record was made. This made the test one of pure telepathy, with "mind-to-mind transference alone possible.

The results were scored by runs of 25. The scores of a series of runs are totaled and the rnean chance expectation for that number of trials found by taking 1/5 of the total trials made in the series. This mean value (np) for 300 trials (12 runs of 25) would be 60. Let us suppose the total seore is 90 (an average of 7.5 hits per 25), Then there is a positive deviation from the mean (np) of 30. This is then measured by means of the probable error, which is found by taking .67449 √(n p q), which here would be .67449 √(300 X 1/5 X 4/5)= ± 4.7. The ratio D/p.e. (*i.e.,* the ratio of deviation to probable error) gives a value that I have been calling X, which can be converted into a fraction of probability P by means of Normal Probability Integral Tables. In the illustration given we would have X = 30/4.7 = 6.4, which would give a P of about 1/50,000; that is, the odds against chance as an explanation of the results are 50,000 to 1, which would of course amount to practical certainty on the point.This method is similar to that used by Coover and Estabrooks and is a standard technique in the field.

THE NEW CARD SERIES

In this series with subject HP, I was myself the recorder and observer. Twenty-five packs of freshly printed cards were used in the series, and the subject had never seen them before. The packs were laid on the table in a stack at the beginning of a sitting, and each was used for three runs, with the exception of two that were used only once and five that were used only twice. A pack was handed to the subject by the observer, and he was asked to shuffle it. The shuffling was always thorough. Then I cut the pack and placed it before the subject, or allowed him to hold it face downward in his hand, removing each card still face downward to another pile on the table after he called it. No one looked at the faces of the cards until the end of the run. There had been no opportunity for anyone to learn the order of the cards nor to study them for sensory cues. The cards were checked after the completion of each run of 25.

In 1650 trials consisting of the first, second and third runs with these new cards, the average per 25 trials was 9.5. This is nearly double the mean value and the X value is very high, 27.0. To give the odds against this being due to chance would require a nurnher of 70 digits. It will be recalled that an X value of 4 is the usual criterion of significance. In these scores the first runs with the new packs were as good as the third.

The results may be grouped as scores in hits per 25 for the 1st, 2nd and 3rd runs as follows: (It will be seen that while all 25 packs were used for a first run only, 23 were used for the second. and 18 for the third. This was due to interruption of the experiments.) 1st runs: 4, 8, 10, 7, 12, 11, 12, 15, 8, 7, 12, 11, 13, 9, 9, 8, 11, 13, 13, 7, 6, 9, 4, 9, 7. 2nd runs : 6, 8, 9, 8, 11, 11, 12, 7, 8, 12, 7, 5, 10, 12, 14, 11, 9, 13, 8, 15, 6, 9, 15. 3rd runs: 8, 14, 10, 8, 12, 7, 5, 11, 8, 10, 6, 12, 10, 8, 8, 10, 10, 8.

The 1st runs average 9.4 hits per 25, and the 2nd, 9.8, and the third, 9.2 per 25. Each group even taken alone is highly significant in its deviations.The ratios D/p.e., or X values, are respectively, 16..3, 17.2, and 13.1.

Any one still skeptical about this mathernatical procedure may get a “commonsense” picture of the results by observing that whereas in a chance distribution the scores ought to be distributed so as to give an average closely approximating 5, in these results, we have 9.5; and whereas the mode in the normal case would be 5, in this there is a bimodal distribution, the principal mode being 8, and the second 12. And the median here is 9 instead of 5. Of the total of 66 runs, only 2 are below the expected mean and 62 are above it.

This New Card Series is submitted as evidence against the hypothesis that the results may be due to poor shuffling of the cards, leaving an order that may be already known to the percipient. It matters not how the cards were shuffled, since in the 25 first-run group, the order was entirely unknown to the subject. He had never seen any of the 25 packs before.

Incidentally, this New Card Series answers the question of whether possible sensory cues could have been given by slight markings on the backs or edges of the cards. It would require experience with them to learn these.

THE DT SERIES

This DT (or "down through") series consists of 1,625 trials, or 65 runs, made with subject HP (myself again the observer) under the following conditions: The pack of cards would be shuttled, frequently by the subject himself under the surveillance of the observer (myself again in this series). The shuffling by HP was really profuse,12 since he felt that he got some advantage from contact with the cards. He habitually did not look at the faces, and could not possibly have "stacked" (*i.e.,* arranged the order of) a pack with the observer watching. The observer always cut the pack and it was left lying on the table while the calls were made. About a dozen different packs were used in the series: sometimes they were new to the subject and sometimes previously used packs were provided. When detectable markings appeared on the cards they were discarded.

The peculiar feature of DT work is the fact that the pack was not touched until after the 25 calls were made. After they were all made and recorded the call-record was checked against the order of the cards in the pack.

The last five calls in the run, representing the bottom of the pack as it lay, were the best. The hits were next best for the first five at the top, and lowest for the central 15 cards of the pack, as they have been for several other subjects—though there is one striking exception.

The 1,625 trials gave a total score13 of 482, which has a positive deviation of 157 ± 10.09. This gives a critical ratio (X) of 14 and a P of 10-20.

The DT data add weight against the hypotheses of sensory cues, memory, and bad shuffling. They bear too upon physical hypotheses involving hyperaesthesia and special radiation, on which other data given below will bear, In a word, they offer difficulty to a wave hypothesis, since waves from 25 cards in a pack must give only splotchy confusion.

Groups A, C, and D were made with the observer in the Physics Building and the subject in the Library. The distance was about 100 yards . In group B, the observer was farther away with the cards, approximately 250 yards. The subject was in the same place in the library as in A, C, and D. In it there are four groups and two conditions of distance. Dr. J. G. Pratt, then a graduate assistant in psychology, was the principal observer throughout the series.

The observer and subject synchronized their watches, and arranged to work at a stated time and distance. At the specified time the observer would take the top card from a shuffled pack of ESP cards in the room agreed on and lay it face down on a book in the center of the table without looking at its face. Thirty seconds later the subject in his cubicle in the Duke Library would record a call for the card. At the end of the minute, the observer would remove the card and take the next one. The cards as removed would be kept in order for later recording. Two runs were made per day.

Group E is included for comparison. It was taken with the subject and the cards in the same room, with Dr. Pratt as observer. In a part of the group, the observer held the cards, and in part the subject did. The conditions were not especially good, but the comparison with the results of the distance tests is very interesting.

In Groups A-D, the records were sealed up after each sitting and delivered to me before subject and observer got together. In Group D, I was present with Dr. Pratt as a second observer.

The cards were shuffled between runs, and this shuffling occurred just before using with HP absent from the room. Even if only one card were displaced at an unknown point and a cut made, the shuffling would baffle any guessing even from a perfect memory of any previous order of the cards in the packs used. The only check needed in the shuffling is that obtained by correlating successive card records for the runs. For Group A this averages 4.5 hits per 25, or slightly below the mean chance expectation. Two packs were used at a sitting. Now if we take only the most favorable combinations in each sitting we raise the average only to 5.6. So the results obtained are clearly not explainable by inadequate shuffling,

The scores[13] may be represented in averages per 25 for the group, and the significance of the derivations represented by the X values.

Group	*Conditions*	*Number Trials*	*Average per 25*	*X-value*
A	Physics Bldg. and Library	300	9.9	12.6
B	Medical Bldg. and Library	1,100	6.7	8.4
C	Physics Bldg. and Library	300	7.2	5.6
D	Physics Bldg. and Library	150	9.3	7.9
E	Same room	900	8.2	13.7
	Total	2,750	7.7	21.0

With all sensory cues excluded by the conditions, with the shuffling checked satisfactorily, and with the chance hypothesis so clearly ruled out by the statistical evaluation of the data, there is left only the hypothesis of some extra-sensory mode of perception. Anyone suspecting observer and subject of collusion must include me also in the conspiracy in view of Group D in which I was a witness, of Dr. Pratt's handling of the cards.

O-Z TELEPATHY SERIES

In this series subject GZ was the percipient and Miss Sara Ownbey was both observer and agent or sender. Both were graduate assistants in Psychology, and it was felt that an independent observer was unnecessary. In Group A of this series the agent and percipient were in the same room; in Group B, in adjoining rooms, and out of sight of each other. A door was open between and about 10 or 12 feet of distance separated the two subjects. In Group C, there were 28-30 feet between and two walls, in line

13 The actual scores are as follows: Group A: 3, 8. 5, 9, 10, 12, 11, 11, 12 13, 13, 12. Group B: 12, 10, 6, 4, 10,10, 2, 6, 5, 12, 7, 5, 12, 11, 9, 10, 6, 3, 0, 13, 10. 12, 12, 4, 4, 1, 4, 4, 7, 6, 5, 0, 6, 3, 11, 9, 9, 4, 8, 6, 0, 6. Group C: 5, 4, 11, 8, 4, 9, 9, 8, 9, 10, 2, 7. Group D: 12, 3, 10, 11, 10, 10. Group E: 10, 12, 11, 11, 9, 7, 5, 8, 8, 9, 10, 5, 8, 7, 6, 4, 8, 7, 8, 7, 6, 11, 12, 13, 12, 9, 11, 5, 9, 5, 6, 10, 4, 6, 7, 5.

of vision, though again doors were open between rooms. For all three groups there was an electric fan going, which would effectually obscure any "involuntary whispering", if it occurred. Signalling was effected by a regular and monotonous tap of a telegraph key.[14]

The agent, using the symbols of the ESP Cards, but not the cards themselves, and deliberately choosing her order of symbols five in advance so as to avoid falling into habitual preferences, would think intently of her first image and then give the signal to the percipient. The percipient would call out his choice and the agent would record the symbol chosen and check it if correct. She was practised in avoiding following any routine selection, and varied her order systematically. There have been several checks made as to her success in avoiding characteristic patterns which might be followed by the percipient. These were made by correlating the consecutive records of the agent. The check results of three such series, one of which is reported in full below (the Junaluska Series) have yielded for 650 trials an average per 25 of exactly 5. This is the mean itself. Very evidently the agent is successfully avoiding repeating herself or falling into any stereotyped order of selection. A day by day check between agent's series for 8 days gave only 3.9 hits per 25.

The question of similarity in habits of choice may be further checked by cross-checking the agent's record with the percipient's of—let us say—the day preceding or following, that is, correlating runs not intended to match. A cross-check was made of the Junaluska Series given below and it yielded an average of only 4 hits per 25. Habitual similarity of order of choice between agent and percipient can, I think, be said to have been satisfatorily eliminated. Moreover, in the series cross-checked as just described, the runs were made daily and the cross-check, therefore, covers also the question of daily routines or patterns of order of choosing.

Finally, as an extreme test of "patterning" or order-habits, we may check the percipient's records against themselves in consecutive order. The percipient might have a pattern of order without its being coincident with a similar one supposed for the agent. But if *he* does *not* have such a pattern, the question of similar order-habits is ruled out. Such a check made on 12 of the percipient's records given below, taken consecutively from a block chosen at random, and on the 8 records of the Junaluska records, yield an average of coincidences of 4.6 between runs, as against an expected 5.

In this series Miss Ownbey was solely responsible for the entire records. In others she was not, as in the Junaluska Series below. In one such short series with GZ[15] as subject the average score is 11.0 and the X value is 7.5. In another, X = 4.2. In both, a recorder was present with the subject. The purpose in presenting this series[16] is, however, not primarily to offer evidence of telepathy as such, but to bear on the relation of distance to scoring rate in considering the wave hypothesis of extra-sensory perception. The rate does not fall off with distance as both the wave theory and the hyperaesthesia hypothesis would require. The succeeding series satisfies the point of independent recording.

14 If it be objected that the sounds might be varied unconsciously and serve as cues, it might be replied that no opportunity to learn the cues was provided. The subject did not know when he was right or wrong.

15 It should be said that GZ does not like to keep a record himself, since it distracts him; he feels that he cannot get into the right mental condition and continue to record. He sits with closed eyes and in deep abstraction from surroundings.

16 The question has been raised as to whether the agent might not, through her strong interest in getting good results, be likely to mistake unclear enunciation by the percipient who is off in anther room, and to give favorable interpretations of calls not clear. First, the names of the symbols are phonetically quite easily distinguishable; each has a different vowel sound. Second, audition was good with open doors between rooms. Third, the scores for this work with the agent and percipient in the same room are nearly as high as with the two separated.

In presenting the data of this O-Z series, I will give first the 12 consecutive runs from which the check on the percipients' records was made. these were made in one sitting, with three conditions, given below. And below these results will be given all the telepathy tests given GZ by the observer under these condidions (one room and two rooms away) during the summer of 1933, and along with them all of the tests made at the same sittings with both in the same room.

	Same Room		*One Room Away* *10-12 feet*		*Two Rooms Away* *28-30 feet*	
	Score		*Score*		*Score*	
	Trials	*Avg. per 25*	*Trials*	*Avg. per 25*	*Trials*	*Avg. per 25*
12 consec. runs . .	50	15.0	125	19.0	125	16.8
Totals	950	14.0	750	14.6	250	16.0

These scores are so high that their significance is obvious without any computation. The 12 runs given above yielded the following scores, in order, 14, 16, 17, 21, 19, 19, 19, 13, 15, 16, 17, 23. In one of the runs of the 950 trials there was a perfect score of 25.

THE JUNALUSKA SERIES

In this series MFT was percipient, with Miss Ownbey again as both observer and agent. The tests were made with synchronized watches corrected from Western Union time. The distance was over 250 miles, MFT being at Lake Junaluska, N. C., and the observer at Durham. One run was made daily, at first at the rate of one call per five minutes and later at the rate of one every three. Reports were to be sent to me by agent and percipient immediately. As it happened the first three days' report was mailed to the observer instead of to me, but she had already given me her records, and brought the percipient's report direst from the post-office (and it was in the familiar handwriting of the percipient). The records of the next five days were sent to me. The average for these eight runs was 10.1, and gave an X value of 10.8, which is, of course, very highly significant

"Chance" is excluded as an explanation. Similarity of preferences or order patterns as a possible factor is shown to be lacking. The data meet all counter-hypotheses quite satisfactorily, I think, all that have ever been raised, except that of possible deliberate collusion. Those not knowing the assistant and subject may, by the very pressure of doubt evoked by such unusual phenomena, raise a question as to this. There never is, however, in any research, an absolute answer to this question. The relative assurances that can be had are briefly the following: As already stated, the observer was a Graduate Assistant in Psychology with no slightest reflection cast on her honesty during the four years I have known her. In one who is seeking a psychological training and career we do not expect to find the motivation necessary to practice a planned course of deception such as this. And the marks of deception are lacking. First, when MFT was tested in the same room with the

agent-observer, just before the distance series, she scored an average of only 7.7 hits per 25 trials. At the time she did not expect to do better with distance. Second, several times before and since this series, we have most urgently wanted high scores in special experiments, in some of which the same percipient was used,--and have failed to get them. In the many series of telepathy tests that were statistically significant and were made under good conditions, more than a dozen different agents have been used and nine different subjects. There were fifteen different individuals involved, and in addition a number of observers. The collusion theory would not be seriously applied to all these by many critics. In fact, it has been raised only by those in psychic research who are close to the subject of fraud in mediumistic phenomena.

The hypotheses (and the criticisms) of "inadequate shuffling" of cards have been met by the New Card Series. That of "possible sensory cues" on the cards was likewise excluded by this series, as well as by the DT ("calling down through the pack") Series, and the Campus Distance Series. In telepathy, the question of natural habitual similarity of patterns in agent's and percipient's choices is answered by proper checks on the point in question. Hyperaesthesia, involuntary whispering, and the like are excluded by the Distance Series. "Chance" as an explanation is well ruled out by the tremendously significant deviations, and the odds against chance to which they correspond. These cover the criticisms thus far raised against the work, except that of possible collusion. It is clear that this must involve myself as well as many others, since in one of the experiments I was in a position to check this point (Campus Distance Series, Group D). It is freely granted that for the extreme skeptic this may rightly be regarded as the weakest point. All scientific work suffers alike in this respect at some point in its development.

If we are correct in thinking that these results under the conditions stated indicate an extra-sensory mode of perception—that is, perception without the recognized sensory processes being essentially involved—no one will dispute their tremendous importance for the general science of mental life. I need not here indulge in speculation as to the bearing it would have. Some cautious suggestions appear in the monograph referred to above.[17] If these are sound, they open up such large possibilities for the study of mind and its place in nature that we need to go all the more slowly to determine the evidential grounds upon which they rest.

17 *Extra-Sensory Perception.*

Quelques Expérimentations Basiques dans la Perception Extra-sensorielle: Éléments de Contexte

Note originale de l'éditeur : ... Cet extrait provient d'un article intitulé « Une sélection d'expérimentations sur la perception extra-sensorielle » publiée par J. B. Rhine dans le *Journal of Abnormal and Social Psychology* paru en septembre 1936.

Einige grundlegende Experimente zur außersinnlichen Wahrnehmung: Ein Hintergrundbericht

Ursprüngliche redaktionelle Anmerkung: ...Dieser Auszug stammt aus einem Artikel mit dem Titel "Einige ausgewählte Experimente zur außersinnlichen Wahrnehmung", der von J. B. Rhine im *Journal of Abnormal and Social Psychology* im September 1936 veröffentlicht wurde.

Algunos Experimentos Básicos en la Percepción Extra-sensorial: Antecedentes

Nota Editorial Original: ... Este extracto está tomado de un artículo titulado "Some Selected Experiments in Extra-Sensory Perception", publicado por J. B. Rhine en el *Journal of Annormal and Social Psychology* de septiembre de 1936.

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Spontaneous Telepathy and the Problem of Survival [1]

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Some 35 years ago I took into my employment a tender, delicate-looking boy, Robert Mackenzie, who, after some three or four years' service, suddenly left. . . . A few years afterwards, my eye was caught by a youth of some 18 years of age ravenously devouring a piece of dry bread on the public street, and bearing all the appearance of being in a chronic state of starvation. Fancying I knew his features, I asked if his name were not Mackenzie. He at once became much excited, addressed me by name, and informed me that he had no employment; that his father and mother, who formerly supported him, were now both inmates of the "poorhouse," to which he himself had no claim for admission, being young and without any bodily disqualification for work, and that he was literally homeless and starving. . . . Suffice it to say that he resumed his work, and that, under the cirumstances, I did everything in my power to facilitate his progress. . . I was apparently his sole thought and consideration, saving the more common concerns of daily life.

In 1862 I settled in London, and have never been in Glasgow since. Robert Mackenzie, and my workmen generally, gradually lost their individuality in my recollection. About 10 to 12 years ago my *employés* had their annual soirée and ball. This was always held . . . on a Friday evening . . . . On the Tuesday morning following, immediately before 8 A. M., in my house on Campden Hill, I had the following manifestation, I cannot call it a dream; but let me use the common phraseology. I dreamt, but with no vagueness as in common dreams, no blurring of outline or rapid passages from one thing disconnectedly to another, that I was seated at a desk, engaged in a business conversation with an unknown gentleman, who stood on my right hand. Towards me, in front, advanced Robert Mackenzie, and, feeling annoyed, I addressed him with some asperity, asking him if he did not see that I was engaged. He retired a short distance with exceeding reluctance, turned again to approach me, as if most desirous for an immediate colloquy, when I spoke to him still more sharply as to his want of manners. On this, the person with whom I was conversing took his leave, and Mackenzie once more came forward. "What is all this, Robert?" I asked, somewhat angrily. "Did you not see I was engaged?" "Yes, sir," he replied; "but I must speak with you at once." "What about?" I said; "what is it that can be so important?" "I wish to tell you, sir," he answered; "that I am accused of doing a thing I did not do, and that I want *you* to know it, and to tell you so, and that you are to forgive me for what I am blamed for, because I am innocent." Then, "I did not do the thing they say I did." I said, "What?" getting same answer. I then naturally asked, "But how can I forgive you if you do not tell me what you are accused of?" I can never forget the emphatic manner of his answer, in the Scottish dialect, "Ye'll sune ken" (you'll soon know). This question and the answer

1 [Originally published in *JP, 1943, 7,* 50-60.]

were repeated at least twice—I am certain the answer was repeated thrice, in the most fervid tone. On that I awoke, and was in that state of surprise and bewilderment which such a remarkable dream, *quâ* mere dream, might induce, and was wondering what it all meant, when my wife burst into my bedroom, much excited, and holding an open letter in her hand, exclaimed, "Oh, James, here's a terrible end to the workmen's ball, Robert Mackenzie has committed suicide!" With now a full conviction of the meaning of the vision, I at once quietly and firmly said, "No, he has not committed suicide." "How can you possibly know that?" "Because he has just been here to tell me."

I have purposely not mentioned in its proper place, so as not to break the narravive, that on looking at Mackenzie I was struck by the peculiar appearance of his countenance. It was of an indescribable bluish-pale colour, and on his forehead appeared spots which seemed like blots of sweat. For this I could not account, but by the following post my manager informed me that he was wrong in writing of suicide. That, on Saturday night, Mackenzie, on going home, had lifted a small black bottle containing *aqua fortis* (which he used for staining the wood of birdcages, made for amusement), believing this to be whisky, and pouring out a wineglassful, had drunk it off at a gulp, dying on the Sunday in great agony. Here, then, was the solution of his being innocent of what he was accused of- suicide, seeing that he had inadvertently drunk *aqua fortis*, a deadly poison. Still pondering upon the peculiar colour of his countenance, it struck me to consult some authorities on the symptoms of poisoning by *aqua fortis,* and in Mr. J. H. Walsh's "Domestic Medicine and Surgery," p. 172, I found these words under symptoms of poisoning by sulphuric acid . . . "the skin covered with a cold sweat; countenance livid and expressive of dreadful suffering." . . . "*Aqua fortis* produces the same effect as sulphuric, the only difference being that the external stains, if any, are yellow instead of brown." This refers to indication of sulphuric acid, "generally outside of the mouth, in the shape of brown spots." Having no desire to accommodate my facts to this scientific description, I give the quotations freely, only, at the same time, stating that previously to reading the passage in Mr. Walsh's book, I had not the slightest knowledge of these symptoms, and I consider that they agree fairly and sufficiently with what I saw . . . .

My manager first heard of the death on the Monday—wrote me on that day as above—and on the Tuesday morning, immediately before the 8 A.M. post delivery, hence the thrice emphatic "Ye'll sune ken." I attribute the whole to Mackenzie's yearning gratitude for being rescued from a deplorable state of starvation, and his earnest desire to stand well in my opinion. I have coloured nothing, and leave my readers to draw their own conclusions[2]

Thus years after the last contact between Robert Mackenzie and his employer, an agonized dream told the latter that Mackenzie was not guilty of a charge against him. The apparition came from nowhere, was apparently suggested by nothing, announced itself with horrible but heroic intensity; the employer would soon know the full authenticity of the dream message.

This is one of a heap of investigated spontaneous cases in which the percipient is not, like an ESP subject, searching, reaching out towards a goal; there is no effort of the percipient to make contact with a specific distant event. Rather, an impluse comes forcing its way through into the percipient's experience. What do such happenings mean?

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2 Proc. S. P. R., III (1885), 95-99.

"ACTIVE AND PASSIVE TELEPATHY"[3]

First, is this ESP as we know it? Of course it will be ESP if one stretches that term to include the entire sphere of the mental phenomena of psychical research. There is, however, no clear evidence that any of the ESP data with which the laboratory is concerned involve a dynamic forcing of an agent's way into the unsuspecting mentality of a percipient who is in no way concerned to make supernormal contact. But there are many spontaneous cases of this type. And there are many "semi-experimental" cases in which the agent's effort to transmit something impresses itself successfully on an *unprepared* percipient [4], as when my friend E. L. Cox gave mental orders which were minutely fulfilled by a friend at a distance.[5]

APPARITIONS OF THE DECEASED

It must, I think, be granted that *active* telepathy is the basis of most of the spontaneous cases.

But *whose* activity is it? That of a living person's mind, or that of the mind of a person deceased? The answer lies partly in a study of the *time-relations* of death and apparition.

It has long been known that apparitions are especially likely to occur at about the time of the agent's death; indeed, the statistical treatment afforded in Phantasms of the Living indicated a very large number within twelve hours of the death (the percipient not knowing that the agent was ill or in danger), and Myers schematically indicated the time-relations (shown in Figure I) in which there is a tapering-off of evidential cases as the days pass after death.[6] Most psychical researchers have always been inclined to attribute cases appearing soon after death to "latency" in the mind of the percipient; i.e., they have postulated that in apparitions of the deceased the impression was actually received during the last moments of the agent's life, *emerging later* in the percipient's experience.

3 This terminology is borrowed from R. Warcollier, *Experimenal Telepathy (Boston, 1938), p. 93.*
4 E. Gurney, F. W. H. Myers, F. Podmore, *Phantasms of the Living* (London: Trűber and Co., 1886), I, 104 ff.
5 C. Murchison (Ed.) *The Case For and Against Psychical Belief* (Worcester: Clark University, 1927) p. 272.
6 See Proc. S. P. R., V (1888-89), 427.

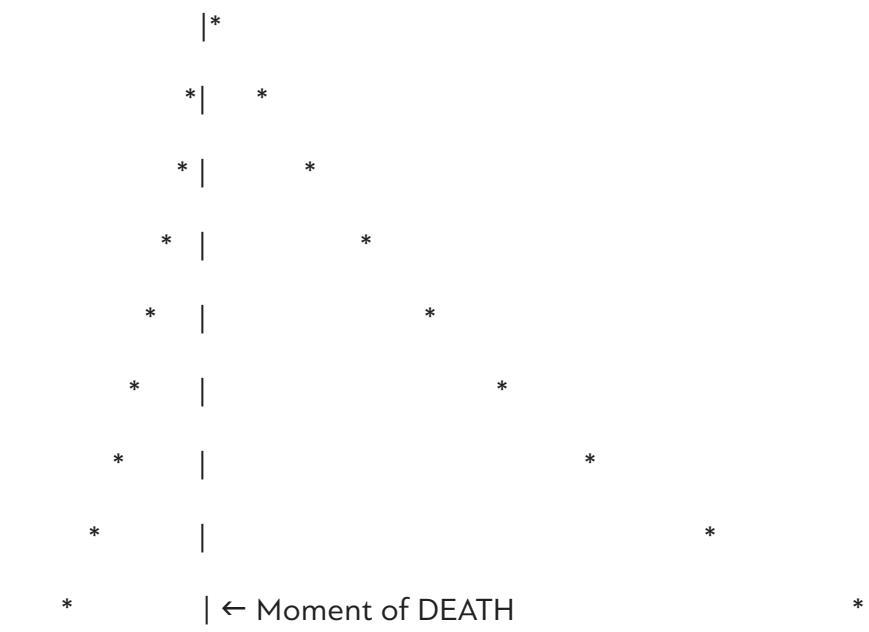

Fig. I

But what shall we do with apparitions which appear *months or years or more after death*, the death being unknown to the percipient?

The following is a run-of-the-mill sample:

V.---From Mr. John E. Husbands, of Melbourne House, Town Hallsquare, Grimsby.

September 15th, 1886.

Dear Sir:---The facts are simply these. I was sleeping in a hotel in Madeira in January, 1885. It was a bright moonlight night. The windows were open and the blinds up. I felt some one was in my room. On opening my eyes I saw a young fellow about 25, dressed in flannels, standing at the side of my bed and pointing with the first finger of his right hand to the place I was lying. I lay for some seconds to convince myself of some one being really there. I then sat up and looked at him. I saw his features so plainly that I recognized them in a photograph which was shown me some days after. I asked him what he wanted; he did not speak, but his eyes and hand seemed to tell me I was in his place. As he did not answer, I struck out at him with my fist as I sat up, but did not reach him, and as I was going to spring out of bed he slowly vanished through the door, which was shut, keeping his eyes upon me all the time.

Upon inquiry I found that the young fellow who appeared to me died in that room I was occupying.

If I can tell you anything more I shall be glad to, if it interests you.

JOHN E. HUSBANDS.

The following letters are from Miss Falkner, of Church-terrace, Wisbech, who was resident at the hotel when the above incident happened.

October 8th, 1886.

The figure that Mr. Husbands saw while in Madeira was that of a young fellow who died unexpectedly months previously, in the room which Mr. Husbands was occupying. Curiously enough, Mr. H. had never heard of him or his death. He told me the story the morning after he had seen the figure, and I recognized the young fellow from the description. It impressed me very much, but I did not mention it to him or anyone. I loitered about until I heard Mr. Husbands tell the same tale to my brother; we left Mr. H. and said simultaneously "He has see Mr. D."

No more was said on the subject for days; then I abruptly showed the photograph.

Mr. Husbands said at once, "That is the young fellow who appeared to me the other night, but he was dressed differently"---describing a dress he often wore— "cricket suit (or tennis) fastened at the neck with sailor knot." I must say that that Mr. Husbands is a most practical man, and the very last one would expect "a spirit' to visit.

K. FALKNER.

October 20th, 1886.

I enclose you photograph and an extract from my sister-in-law's letter, which I received this morning, as it will verify my statement. Mr. Husbands saw the figure either the 3rd or 4th of February, 1885.

The people who had occupied the rooms had never told us if they had seen anything, so we may conclude they had not.

K. FALKNER.

The following is Miss Falkner's copy of the passage in the letter:

You will see at back of Mr. DuF-------'s photo the date of his decease (January 29th, 1884); and if you recollect "the Motta Marques" had his rooms from the February till the May or June of 1884, then Major Money at the commencement of 1885 season. Mr. Husbands had to take the room on February 2nd, 1885, as his was wanted.

I am clear on all this, and remember his telling me the incident when he came to see my baby.[7]

The question confronts us: who, or what *initiated* this experience? The dreamer's mind? Some distant living person? The deceased?

I venture that many readers will intuitively assign to the deceased the *activity* which is the source of the impression without thereby asserting that the apparition necessarily *is* the deceased; F. C. S. Schiller beautifully expressed this hypothesis by saying that apparitions are the "dreams of the dead." At any rate we must press the question: if the receiving of a clear and strong, though unexpected message—a hammer-blow dealt to one's unsuspecting mind—indicates more than passive percipiency, namely a real *activity* by an *agent*, is there any real basis for interpreting messages from the living as *real* messages, while at the same moment forcing all messages from the deceased into the procrustean bed of "ESP," assigning all the roles in the drama to a living percipient, who "does all the work"? When, in the „Chaffin Will" case [8,] an insistent dream message purporting to come from the dreamer's father directs the dreamer to the *discovery of an unknown will*, which is accepted as valid by all who knew the testator, is this simply *clairvoyance* of the will? Perhaps so. But what *initiated* the process?

When Miss R. C. Morton [9] became intrigued by the frequent appearance of a phantasm which walked about the house, making itself visible to several members of the family, and to two neighbors who saw it near the house, she set herself the task of recording its appearances and disappearances, pursued it into corners until it disappeared; tried to make it turn and directly confront her; and, driven to sterner methods, attached a string with glue to the wall by the staircase, stood at the foot of the stairs and watched it walk down through the string.

Now there *is* some ESP in this case, in the sense that there is an unexplained correspondence between the times and places upon which the phantasm is observed by the different individuals, not all of whom are in normal communication with one another; and there may be a certain retrocognitive clairvoyance in the fact that the form and behavior of the phantasmal woman seem to correspond with the appearance and form of a woman who had earlier lived in the house. The main interest of the case, however, lies not in these details, but in the *initiative* taken, so to speak, by the phantasm. Miss Morton and her relatives and neighbors are not engaged in telepathic experiments, even in the broader sense of the word. They are not, at the beginning of these experiences, interested in the deceased individual whom the phantasm seems to portray. Whatever it is that initiates the experience, as in the Mackenzie case, comes from outside, bursts through the ordinary barriers of experience and compels the attention of the living.

7 *Ibid.,* 416-417.
8 *Ibid.,*XXXVI (1928), 517-524.
9 *Ibid.,* VIII (1892), 311-312.

THE "EXTENDED TELEPATHY" HYPOTHESIS vs. THE SURVIVAL HYPOTHESIS

From the very beginning of psychical research, cases of this type were a center of controversy between (1) those who wished to expand the hypothesis of telepathy to cover all instances of supernormal awareness, and (2) those who wished to distinguish between cases of *active* percipiency, the quest of an individual for specified supernormal information, and *passive* percipiency, in which the percipient is merely the receiving station for something initiated from outside his personality. The second group of researchers were interested in determining who were the active agents.

Now as far as I know, there was never any doubt that the latter group had the better of the argument as far as actual evidence was concerned. The basis of argumentation by the former group of investigators was frankly the hope that telepathy could be so defined as to permit evasion of the survival hypothesis. Gurney, who had his hands full establishing telepathy, was in no way anxious to run full tilt into all the difficulties of the survival question, while Podmore, in the same period, and more and more insistently as time went on, made it his special task to stretch telepathy in every conceivable dimension, with the professed purpose of making the survival hypothesis unnecessary. Followers of Podmore convinced themselves that if we once accept the capacity of the individual living mind to make contact with the past or present thoughts or feelings of other living persons, sometimes catching the thought as it develops, some times becoming aware of it after a period in which it has lain dormant in the receiver's subconscious, it will be impossible for any deceased mind, if it exists to demonstrate its continuation beyond death. Myers, on the contrary, early became convinced that this argument was forced and inconsistent; that apparitions behaved autonomously in terms of their own dynamics, not primarily in terms of the dynamics of the percipient; that they came when least expected or wanted, that they appeared to those who had never known them, and that under certain special conditions they could overwhelm the percipient even to the point of "invasion" of his experience, blotting out all competing experiences, momentarily reconstituting psychological space in which he lived.[10]

Thus in many of the best cases of "invasion" described in *Phantasms of the Living* the invading presence *transforms the world around the percipient*, while indeed in several such cases the distant agent, in coma or dream, indicates to those nearby that he is in fact invading the region now occupied by the percipient. Since all this can be established clearly enough, Myers argues that the transmission of telepathic impulses from the living and from the deceased follows the same principle; the invasion experiences express the same dynamics whether the apparition is that of a living or that of a deceased person.

A defender of the ESP hypothesis might reply: To be sure, there is a great deal of difference between looking for something and having it thrust upon us; between listening for something and having it obtrude itself noisily upon our ears. No psychologist in his right mind would apply the terminology of the former cases to clear-cut instances of the latter. All of these, to be sure, are instances of "perception"; but the stimulus, in the one case, lies near at hand and is *sought*, while in the other case, the stimulus, which may happen to be another human personality, comes to meet the perceiver and, if need be, may overpower him as a thunderclap does. Students of ESP are concerned as much with agency as with percipiency.

10 Cf. especially E. Gurney *er al., op. cit.,* II, 277-316.

So far so good. But does this admission carry us far enough? The essential thing, Myers would argue, is the motivation. Thus Mackenzie's employer had only a casual, albeit friendly, memory of his former employee. The dramatization of his suicide serves no great personal aim for the percipient. For Mackenzie, however, the rigor of Scottish Calvinist ethics makes suicide an unbearable burden to contemplate, and nothing would be more reasonable than for the dying man to wish fervently to absolve himself of guilt. If Mackenzie and the scores of others who have appeared hours, months or years after death no longer exist, *what supplies the motivation*?

Here we have clearly come to the definition of two basic differences between the thinking of Podmore and of Myers: (1) Myers is profoundly concerned with motives, Podmore is not; (2) Myers, but not Podmore, distinguishes between the two types of percipiency-active and passive. With Myers we are concerned with the interaction between the subliminal selves of two individuals, the agent being as important in the process as the percipient. For Podmore the living percipient, who is the real subject in telepathy, the person "who does the work," is endowed also with the capacity to simulate and dramatically elaborate the action, speech and purpose of the agent, even to the point of producing a complete phantasm. Thus Mackenzie's employer, paranormally perceiving the fact of Mackenzie's death and circumstances attending it, re-works the material in a dream some hours later and produces it as a message from the deceased, while Miss Morton and her fellow witnesses, paranormally perceiving a few facts about an earlier dweller in the house, project the facts in personal form into a very lively, spontaneous, and unpredictable ghost.

Now there is nothing *psychologically impossible* about this last group of hypotheses. But they are hypotheses supported by incomplete evidence, adduced partly for the avowed purpose of avoiding direct confrontation of the survival question. Myers clearly saw this, and hammered away at the point for nearly twenty years. This is not equivalent to our saying that he necessarily had the better of the argument; the issue is incredibly complicated and the limits of extra-sensory capacity can certainly not be set. One thing, however, can be said: experimental telepathy and clairvoyance, as we know them, do not cover by any means the chief established facts about apparitions, and it is wishful thinking to extend any hypothesis of this type to events of which we know so little.

And let us repeat: the most serious bungling involved in attempting the stretching of hypotheses in this way is the neglect of motivation. Motives are something which the psychologist never has a right to evade. The spontaneous cases are shot through and through with compelling evidence of powerful motivation. Most of the spontaneous cases have to do with death, illness or catastrophe; they come to those who are near and dear to those involved. Motivation is present in both agent and percipient in most cases. *When, however, the percipient has no such motivation*, is not oriented to the agent nor concerned with what is happening to him, and the impulse compels the attention of the percipient willy-nilly, our obligation is just as clear to look elsewhere for motivation processes; and if they exist they must be assigned to the agent. If the agent is living, this fact by itself greatly extends what we call the ESP process. But if the agent is deceased this makes more demands upon us. Somewhere beyond our ordinary time and space, a motivation process deflects the living from their ordinary concerns, twists and wrenches them from their usual preoccupations, lifts them into a timespace experience with a full motivational richness which belongs to the deceased. This is the empirical situation with which ESP investigators and all psychical researchers must honestly deal.

In any era of active experimental research in any science, the great problem is to keep investigators looking at the things that Nature throws at them, rather than only at their own little pile of half-solved problems, half-perfected tools. When Newton's followers got the mechanics of the Universe, and its ways of demonstrating its laws, all settled, it was hard for Nature to make herself heard through the contented buzz of the scientists; when experimental psychology got itself nicely organized for the study of perception it was hard to get investigators to listen to the plain fact that perception expresses the deep dynamics of individual personality. And when ESP research got nicely organized, many of its laboratory investigators tended to forget that Nature *constantly throws at us* a baffling problem, that of apparitions of the deceased, which need more study *both* to teach us the difference between active and passive telepathy *and* to lead us to inquire why personality continues somehow to intrude itself into the experience of others beyond the time of the death of the organism.

I have not meant to imply in any way that apparition are a *proof* of survival, nor even that they are necessarily "better evidence" for survival than are the Piper or Leonard type of communications or the cross-correspondences. But I *do* mean to emphasize two points: first, that as long as they *suggest* post-mortem continuity of personality, and are only with difficulty treated as cases of ESP, they should be fearlessly studied in the light of the survival hypothesis; second, that whether the answer to the survival question is ultimately yes or no, the study of the origins of apparitions will add enormously to our understanding of the telepathic processes—their structure, extent and significance.

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Subjective Forms of Spontaneous Psi Experiences [1]

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ABSTRACT: In this study, approximately one thousand cases of spontaneous psychic experiences were analyzed to find out what range of forms the experiences more commonly took. Four main types were found: (1) Intuitive, in which the subject's experience was a simple, unreasoned impression or hunch. (2) Hallucinatory, in which the experience was projected as if it were a sensation. (3) Unrealistic dreaming, in which the experience was characterized by fantasy. (4) Realistic dreaming, in which the imagery was almost photographically realistic. Groups (1) and (2) occurred only in the waking state.

The fact that these four types are common in mental life and are not peculiar to psychic experiences is of importance in understanding the process. The classification introduces new questions, such, for example, as what part personality differences may play in determining the form which a spontaneous experience will take.

It appears that the main act of acquiring the knowledge occurs on the unconscious level and the form the impression will take in consciousness represents the mental device or "mechanism" by which knowledge of the stimulus event is carried over the threshold. Some act of selective judgment is inferred to occur below the level of consciousness. ---Ed.

Amid the various endeavors to understand and control psi ability, little attention has as yet been paid to the range and types of subjective forms of its expression in consciousness. However, now that it is recognized that the basic psi process occurs on an unconscious level, the importance of correctly interpreting its conscious manifestations is greater than ever before. Neglect of this aspect of psi has doubtless been due to the fact that in experimental situations, the form of the conscious expression is largely limited or channelled by the conditions of the experiment. For example, in card calling tests, the subject is likely to experience a series of visual images, correct or incorrect; in matching tests, to have a motor impulse to place the card in a given position, right or wrong. In spontaneous experiences, however, it would seem that much more of the method of operation of the process would be revealed, for in such experiences conditions are uncontrolled and responses are unrestricted.

That they are unrestricted is evidenced by the many differences of form that result. The forms vary obviously from vague hunches and general intuitions to clear-cut and detailed impressions. It would seem, however, that the variations are not unlimited, for in studying large numbers of spontaneous case reports, similarities of form are observable, and eventually recognizable types emerge. This observation suggests that the forms may have a rationale, may be the result of general underlying principles. But

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if such principles exist, they have not yet been demonstrated, for to date, no study on the subject has been made. Not even a survey of the various forms that psi takes in its manifestations as displayed in spontaneous experiences has been reported. The attention of parapsychologists, long directed away from cases, has too recently returned to them for a classification of their psychological aspects to have been made.

This is not to say, however, that variations in types of experiences have not previously been noted. Perhaps every collector of cases in the past has been to some extent aware of them. Certainly many incidental references to them are made, as for example in Gurney, Myers, and Podmore's *Phantasms of the Living* (1). In this outstanding collection many textual references to the form of expression used in individual cases may be found. In chapter headings as well there occur such references as "Transference of *Ideas* and *Mental Pictures*," and "Transference of *Emotions* and *Motor Effects*." But the major and proper interest of these authors at their stage was in the question of the occurrence of telepathy, not in a psychological study of the way in which it occurred. The objective of establishing the hypothesis of occurrence of telepathy, of course, was not finally attained until much later and then not by case studies but by the experimental approach. But as telepathy and allied psi processes came under experimental study the attention of investigators shifted away from spontaneous cases, and so for many years no systematic study of them was made.

Reawakened interest in the study of cases for their value in guiding research led to a new collection of them at the Parapsychology Laboratory and a first report on it has already been made (5). This collection was classified first into tentative subdivisions representing the leading research questions of the time. One of these was "How does psi occur?" Within this subdivision the cases were classified under two main headings: First, the "manner" of experience; and second, the degree to which completeness of meaning was apprehended. As classification under these headings progressed, the impression grew that these cases had indeed something of importance to suggest about the operation of psi. It seemed increasingly probable that experiences that were for example dramatic, or symbolic, or photographic in character were potentially telling something about how psi occurs. And similarly, those that were fragmented in meaning in contrast to those in which the knowledge secured was relatively complete must be evidence of something in the process that needs to be interpreted.

Accordingly the task was undertaken of surveying as many cases as possible in which such features as those were notable to see what forms of subjective experience could be isolated, to classify such forms, and study their general characteristics. The elements involved in each case would be: first, the individual who had the experience (the subject or percipient); next, the external event unknowable to him by sensory means (the stimulus event or situation); and finally, the knowledge of it that came to him (the psi impression). As mentioned before, preliminary observation had suggested that variations in form of the psi impression fell into certain natural groups. But how many such groups, what their relationships might be or whether any general principle might connect or integrate them or what enlightenment on the nature of the psi process they could afford, remained to be disclosed. To get some answer to those questions was the objective of the study.

The task was to observe the psychological form which the psi impression took in the consciousness of the individual concerned, to try to recognize any generalizable features that might be present in individual cases, and to see what groupings would result. In order to do this it was necessary to disregard the almost endless (but for this study unimportant) variation in the details of individual cases and to pay no attention to the type of psi process that happened to be involved. For whether it was a case of clairvoyance, telepathy, or precognition would not matter in this survey, but only that there seemed to be a psi element in the experience.

As the cases were examined and separated into groups, it was soon evident, however, that the two criteria for separation mentioned above (manner of entry of the psi impression into consciousness, and the amount of knowledge conveyed) were not independent but were apparently related aspects of the procss. The end result of the two was the subjective "form" of the psi impression in consciousness which had been noted in the initial observations.

In order to establish a basis of judgment as to the relative amount of information secured in any individual instance, it was found necessary to distinguish two things: first, the generalized or essential meaning of the stimulus event to the subject; and second, its various details. For example, in a type case, suppose that the event involved were the death of a relative. The essential meaning would be simply that brother John is dead. The details, of varying degrees of importance, might be that he was killed in a car crash and that his car failed to take a curve at the foot of a certain hill. Since individual instances obviously varied, ranging from those in which the psi impression included only the barest cognition of the stimulus event over to those disclosing even minute detail, this distinction proved to be of some importance in judging certain kinds of experiences to be discussed later.

Because of the nature of the spontaneous case the entire study was necessarily one of evaluative judgment. It has not been feasible as yet to have a recheck of the writer's judgments by a second person in any of the phases of the survey. The time and effort involved are at present prohibitive. It is entirely likely that there would not be one hundred percent agreement if such were made, and that a retally of the author's own judgments would not check exactly. The material is too inexact for that. If the study proves useful, the recheck will in time, no doubt, be made by others and extended to other material. It is felt, however, that observations herin reported should be applicable in their essential outlines to any broadly similar collection of reports of psi experiences.

RESULTS

A total of 1,073 suitable cases were available for the survey. They were taken from the Duke collection mentioned above. The number included all cases in that group that had been reported fully enough to make judgment of type possible. (Reports of apparitions, hauntings, physical or PK effects, and those seeming to bear only on the survival question were not adapable to the survey.)

Four types emerged that seemed so different as to deserve to be considered distinct groups. The psi impressions in these were such as to suggest the following descriptive terms: (1) Intuitive; (2) Hallucinatory; (3) Unrealistic Dreaming; (4) Realistic Dreaming. None of the four tems, however, defines

perfectly the group to which it is applied. One is reluctant to coin new terms, but old ones, of course, are limited by definition and usage and are not entirely adaptable to new needs. But with proper explantions perhaps these can be made to serve the present purpose.

Although the majority of cases fell definitely and easily into one or another of these groups, there were some exceptions. There were some borderline cases and some that showed characteristics of two groups. Seventy-seven of the 1,073 cases were of these indeterminate types. They suggested that as in most attempts to systematize nature the distinctions fitted upon it are arbitrary. Definitions and classes must have boundaries, but nature is continuous. In this case it was surprising, not that there were exceptions, but that the exceptions were comparatively few. In this report the seventy-seven exceptions will be omitted from discussion, and for the sake of simplicity only the four main groups will be delineated.

Although separation into the four groups was not made on the basis of the sleep *vs.*waking distinction, considerable division of that kind did result indirectly. The Intuitive and Hallucinatory groups were composed of waking experiences only. Each of the two "Dreaming" groups, however, though made up of dreams mainly, included a few waking experiences. Perhaps there is no clear-cut dividing line separating mental processes exactly at the border line between sleep and wakefulness. At any rate, the characteristics of those waking experiences that were included in the Dreaming groups were similar to those of the dream groups and quite unlike those of the waking groups.

The number of cases in each of the four groups, with their dream *vs.* waking distribution as well, is shown in Table 1. Following that, each group is illustrated and discussed.

Table 1

CASES GROUPED ACCORDING TO FORM OF EXPRESSION AND DIVIDED ACCORDING TO DREAM AND WAKING STATE

Form of Expression	Waking	Dream	Total
I. Intuitive	308	0	308
II. Hallucinatory	66	0	66
III. Unrealistic Dreaming	4	180	184
IV. Realistic Dreaming	85	353	438
Indeterminate	58	19	77
Totals	521	552	1073

I. Intuitive Group

In this category were put all cases in which a subject reported only that he suddenly "just knew" something that was later judged to pertain to an event or situation unknown to him by any sensory means. The form of the experience was that of a simple guess or hunch, or in the terminology of Gurney, Myers, and Podmore, an "idea." These cases were called "Intuitive" because they had no antecedent, sensory or rational, the subject accepting them without any conscious reason. That the term is not perfect will be seen later.

The psi impression in cases of this group was found to be characterized by limitation of content. The "idea" received had to do at the most only with the essential meaning of the ESP stimulus. No details were included. In a type case like the one suggested above the subject would suddenly "just know" that brother John was dead. He would have no reason for his conviction, and he would not know the cause of death, but usually he would be entirely convinced of it. Emotion, if present, would seem to be secondary and except in certain circumstances discussed below, the result of the impression rather than part of it.

In the following and all succeeding instances, examples are taken from actual reports on file, and though abbreviated in some cases, they are given as far as possible in the words of the individuals who reported them. For convenience, a phrase embodying the thought considered to be the essential meaning of the case is given.

Example 1. Essential meaning: My son is not at his camp.
A relative of ours was staying with us several weeks ago. Her son is in the service, a fine strapping lad who had been home for Christmas. One morning she said to my wife, "I know that something is wrong. Jesse is not at his camp." Questioning brought no reason, but she was certain For three days thereafter she was upset. On the third day she received a toll call. It was from her son. He was AWOL, and did not know why he had done such a thing. She urged him to go back and take his medicine. He promised to do so.

Example 2. Essential meaning: Father's condition is critical or Father needs a doctor.
My father was sick for two years before his death. At the time, I was 14 years old. Now my age is 41. Having finished my supper one night, something told me that my father needed a doctor. First I told my mother and sister. We all went up to his room and asked how he was feeling. He replied that he was feeling great and that he was in good spirits. Something still told me that he needed a doctor and I insisted that they send for my older

brother who lived across town from us. Everyone told me that I was upset and imagining things. It was a long time before sleep overtook me that night. Sure enough, my father was found dead in bed the next morning. Now I was the youngest in my family of seven sisters and one brother. Why this warning should come to me at the age of 14, I do not know. It has been on my mind ever since.

Example 3. Essential meaning: Irene is not well.
Last fall I said to my daughter and son-in-law, "I know Irene is not well." Irene is my oldest sister. We have much in common. She is very dear to me. She has always enjoyed perfect health and has led an easy, happy life in Florida. Two or three times later I spoke saying, "Irene is not well." Yet in all her gay, interesting letters which arrived every 10 days or so, she never mentioned ill health.
On December 13th I said to my daughter and son-in-law, "Something terrible is the matter with Irene." Both of them looked at me as though I were talking in riddles. I looked at them steadily saying, "You must believe me, for I know it it the truth. You just remember what I have said." Four days later a brief letter arrived from Irene. Yes, she had incurable cancer. It had started to bother her in the fall, just at the time I felt that something was wrong. At first she did not consult a doctor, never dreaming her condition was serious, and as she is naturally a very cheerful individual, she kept thinking that everything would soon be all right. At the time when I told my family, "I know something terrible has happened to Irene," the cancer made itself known in a most painful way. She is now in a hospital dying slowly.

Example 4. Essential meaning: She has a bird.
I pulled up in front of my niece's house. She had her radio on loud, no other sound. As I sat there a moment looking at her house, I said to myself, "She has a bird." I went in and there in her front room she had two love birds in a cage. Her husband had brought them to her the night before. I didn't see how I could have known this. I don't understand it.

Example 5. Essential meaning: Son's departure overseas.
One of my most vivid psychic experiences was when our 19 year old son was alerted for overseas combat duty on a B-29 bomber late in the fall of 1944. He was due to sail from some southern port, Norfolk, Va., it proved to be. Our home is in Pittsburgh but I was staying with a daughter in Detroit at the time. I knew instinctively the day when his ship left our shores. I awakened my daughter very early on December 20th, so distressed and emotionally upset I could scarcely speak, but at last I told her, "Joe left the United States today at about 5:00 A. M." She was irritated at my humch and was real pleased at the arrival of a card from a chaplain to the effect that Joe had attended a communion service the night before leaving, signed by Joe. The postmark was November 23rd. This indicated that he was still here several days longer than I felt. However, I was so sure it was November 20th that I wrote home to Pittsburgh and told them to just put a ring around that date on the calendar as I knew that that was when Joe left the country. As soon as possible I wrote Joe about the date on the chaplain's card and he replied, "The chaplain must have held our cards a couple of days before he mailed them for we pushed off about 5:00 A. M. November 20th."

In the foregoing examples, although in each case the impression received involved no details but was limited to the simple meaning of the ESP stimulus, still the knowledge of that was relatively complete. In not all of the Intuitive type cases, however, was that true. In some, even the meaning seemed to be only partially or imperfectly sensed. And with that imperfection there was often a strong emotional accompaniment. In this type of case, instead of the impression that brother John is dead, there might for instance (as in Examples 6-8 below), a sudden, significant sadness about John, or a feeling that something had happened to him.

Example 6. Essential meaning: Mother was knocked down.
One day when I was working, I suddenly stopped and said to the girl next to me, "Oh, my mother! Something has happened."
The girl said, "Don't be silly. They would have let you know."
But I got no work done. On returning home for dinner I found mother's head all bandaged. She had been knocked down on the main road at the time I started to worry.

Example 7. Essential meaning: Sister will be killed. (Precognitive)
Friday October 31, 1947 in the night, I became suddenly worried over my sister. She lived in the same town. Sleep was impossible. I walked the floor until 3:00 A. M. The next day, November 1, 1947, almost sick with worry, I tried to telephone my sister. Twice I picked up the receiver and each time hung it back without calling her. An hour went by and then the telephone rang. My sister had been instantly killed in her car. A gravel truck struck her three blocks from her home. I almost went mad thinking that I could have prevented the accident if I had only talked to her instead of hanging up the receiver.
Example 8. Essential meaning: My niece will be killed. (Precognitive)
I had a sense of tragedy hovering between our new home and my brother-in-law's roof just as though a depressive sadness hung between. Of course, I thought it over very hard but I made a mistake. I thought it was for my own daughter and I watched her very carefully. One night I began to feel the familiar hunch of something being wrong, tingling finger tips, chills, hair raising, and butterflies in my stomach. I began to cry in panic. My husband gave me three aspirins and tucked me into bed. I cried myself off to sleep. The telephone rang and we were told my brother-in-law's daughter, the same age as my own, had been struck by a car and killed.

In these instances the impression includes only a certainty of trouble, danger, or tragedy but no knowledge of the form it will take, and there may or may not be awareness of the identity of the individual to be involved. In not all experiences of the Intuitive type, however, was even this limited knowledge of the meaning of the event conveyed. In cases referred to in an earlier paper as Blocked (5) the subject's experience consisted only in sudden, significant emotion or compulsion toward action. The effect was experienced without any rational cause or explanation, but later was found to correspond to some event which would have caused it if it had been known. In *Phantasms of the Living* experiences of this type were referred to as "Emotions and Motor Effects." It seems worth while to consider such experiences as probably cases of psi even though no identifying facts by which a relationship between the experience and the possible ESP stimulus could be more certainly determined get into consciousness. The frequency of the reports of this kind, their vividness, a certain similarity in the emotional quality described, and the strong tension that marks them seem too great to be ignored, particularly in a study made avowedly for the purpose of getting suggestions as to the way the psi process works.

It is here that the question may arise as to the applicability of the term *intuitive* to cases in which the experience consists only in an emotion or compulsion toward action, rather than in an idea. Howev-

er, the manner of entry into consciousness of these experiences is intuitive in that it has no antecedent, either of rational or sensory character. If, as it seems may be the case, the lack of a factual element in the impression means only that it stands at the minimal end of a gradation of possible amounts of information leading up to complete knowledge at the maximum, then these Blocked cases can rightly be classed with the Intuitive ones.

In general, the emotional and compulsive Blocked cases ran as follows: Let the stimulus situation be taken to be the death of brother John with attendant details. The subject would feel a sudden and seemingly unaccountable sadness. His strain and tension would be intense, and if a compulsive factor entered in, as was frequently the case, he also would feel a strong urge to action. In this case it might be to go home (where John lived) but he would know no reason for doing so. In some cases the compulsive aspect might be dominant and the individual would do some *appropriate* thing, perhaps without even stopping to ask himself why he did it. Whether the emotional or compulsive aspect was dominant seemed often to be dependent mainly upon the nature of the case. For example, if John were so far away that going to him was not to be thought on, the reaction would more likely be only emotional. But whether emotional or compulsive the strong tension involved and the sudden nonrational character of the experience made it seem memorable and significant to the subject. In fact, his conviction that the experience had some (hidden) meaning was a definite factor in the majority of cases of this kind.

Example 9. Essential meaning: Father's death.
Some years ago one Friday morning about 10:00, I had an overwhelming feeling that something unusual had happened among or to someone dear to me. I am not often given to uncontrollable weeping, even for a known cause, but that time a floodgate broke and wouldn't be checked, it seems.
After about an hour the tears were still rolling when a neighbor knocked on my door. I tried to be rational enough to answer her questions as to my burst of grief. The tears flowed on, and I knew not why, only that a great grief was in store for me. I was ashamed to let her see me in this plight so very unexplainable at the time.
But in a few hours the answer came in the form of a wire from some 300 miles away. My father had been fatally stricken at the hour of the message. Did he try to call me, do you think?

Exanple 10. Emotional. Essential meaning: Sister is dead.
My grandmother, an energetic, practical and managing woman, had an experience similar to those you speak of in your article.
One morning when she came downstairs, instead of buzzing around doing household tasks and seeing that everyone else did too, she

told us that something terrible was going to happen. She had no explanation or details, but just this dreadful premonition. All day she sat around rocking in a chair on the porch, moaning or sobbing quietly. At bedtime we laughed it off and the next day she was her usual down-to-earth self, but during the day a cablegram was delivered telling of the death the day previously of my grandmother's sister in Scotland.

Example 11. Compulsive. Essential meaning: Daughter is hurt. I was attending church service on Sunday morning when I felt that I must go home. It was before the sermon and it was unthinkable that I should leave, but I did and when I turned into our street, our small son ran to meet me saying, "Eleanor fell off the horse and was very badly hurt." I ran the rest of the way. My husband had put the little girl on the horse and a neighbor's child had whipped it, which made it jump. Fortunately no bones were broken but she was badly bruised.

Example 12. Compulsive. Essential meaning: Servant's attempted suicide. A German writes that he and his wife and small children lived in Sicily at the time. One holiday he decided to take the steamer to a small island for an outing. It was a beautiful warm summer day and they got there around 3:00 in the afternoon. They had intended to spend the whole day and had brought food and were in a happy mood. Suddenly after about an hour, he announced that they must go home. His wife and children put up great protests, but he stood firm. He couldn't think of a really good excuse, but he had the feeling that they must return. When they got home they found the young servant girl on the floor almost dead from gas fumes. She had stuck rags around the doors and windows and was trying to commit suicide. They rushed to the hospital with her where she eventually recovered. She had been in their service only a short time and had given no indication of being unhappy. It turned out that she had fallen into the hands of a marriage swindler who had made off with all her savings.

Example 13. Compulsive. Essential meaning: Wife critically ill.
My husband is like the average American businessman, rather disgusted with what might be called occult ideas. It was to him that a strange vision, no not that, but a presentiment occurred. He was a construction engineer and away from home much of the time for long periods. He was working on a job in Kansas city about 300 miles from our home, when one day when he was eating lunch at the hotel he felt suddenly that he must go home. He hastily packed his bad and reached the station just as the train was pulling out. About bedtime that evening, I had suddenly started a very bad hemorrhage. We lived far out in the suburbs and I was alone with two young children. My husband came in about 11:00 P. M., and his coming undoubtedly saved my life. He went to a neighbor's and called a doctor who made a rush trip and got there after I had become unconscious. My husband was stunned with the thought, the wonder, as to whatever gave him that sudden call.

In all of the above Blocked cases, the individual sensed no factto account for his compulsive feeling or action; yet he was convinced of its significance and responded, much as he might have had he known the facts.

Still another variation of Intuitive cases was found. In it the essential meaning itself did not get into consciousness, but instead an associated item of information that would seem to have been suggested by it. For example, the impression received would not be that brother John is dead, but rather something more removed, as perhaps for instance that John's children need help. In these cases, the emotional element would be secondary rather than primary as in Blocked examples. In emotional accompaniment they were similar, instead, to those cases in which the entire meaning was sensed.

Example 14. Essential meaning: My brother will die. (Precognitive)
A little less than a year ago, I was beginning to plan my two annual pupil recitals. Sometime during the early spring I exprienced a vague uneasiness about the health of one of my brothers. Both brothers had had very serious operations the year before and had recovered. However, my uneasiness was all concerning the one who lived about 500 miles from me. There had been no word from him or his wife except that he was teaching. Nevertheless, there was a distinct uneasiness. I did not want to ask directly how he was because family mail was opened by either of them and any

little note I might send his wife might reach him first and cause him to worry. As time passed the uneasiness became a clearer fact, and also it became connected with the two recitals which were to take place April 25th and May 2nd. Something told me I would not attend the first recital. I even planned in my mind, for I said nothing to anyone else, to ask a friend to take over. Each time the singer who was to assist came to practice the impression was intensified until, on the night before the recital as I sat down to play her accompaniment, a thought came almost like a voice, "There is not much use in this practice. You are not going to play for her. Mrs. S. will play." However, I went on because I did not want to make her nervous. Just as she was leaving, the message came that my brother had passed away at 7:00 just a moment before I started to play. I did not attend the recital and Mrs. S. presided and accompanied.

Example 15. Essential meaning: This train will be wrecked. (Precognitive) When I was newly married I got homesick one day. I just had to go home. Billy fussed a little but gave in to me and ordered the one and only taxi to take me to the morning train. I remember how joyful I was and how I danced about the house waiting for the taxi. Billy and the driver teased me, but I was too happy to care. Just as he bought the tickets, cold fear gripped me. I started to cry, "Give him back the tickets." I said, "Please Billy, we can't go on this train." Billy went into one of his rages but the ticketman reached out and took the tickets. "Do as she says. Always do as she says." We got into the taxi and all the way to the hotel they asked me why. I did not know. I just cried. At dinner that night there was a commotion. The taximan was coming toward me, pushing people to one side and upsetting chairs. He cried, "How did you know not to go on that train? It wrecked at the next town. The car you always ride in turned over and everyone was killed."

Cases like the above seem to indicate awareness of some situation related to or suggested by the stimulus event, but that event itself remains unknown.

Discussion of Intuitive Group

As just shown, considerable variation was noted in the extent of knowledge secured in Intuitive cases. At the most it was limited to the meaning of the stimulus, with no details included and at the least a strong emotion or urge toward action with hardly a trace of knowledge. Between these two limits, there were various degrees of incompleteness in the impression of knowledge of the meaning of the stimulus.

The fact that the emotional aspect of the experience seemed to be present in reverse proportion to the amount of knowledge conveyed is interesting and may have some significance. Quite possibly the earlier designation of emotional cases as "Blocked" may not have been a misnomer. The suggestion at least is that as the passage of the information into consciousness is restricted or "Blocked," perhaps by difficulty at the threshold of consciousness, emotional tension builds up. And with it, as was shown in the earlier report (5), is frequently expressed a high degree of conviction that the experience is meaningful.

But whatever may be the explanation of emotional and compulsive experiences, the fact remains that in all Intuitive cases, knowledge is restricted. The total situation, John's fatal accident at the curve at the foot of the hill, does not become known. At the most only the meaning, that is, that John is dead, is sensed. Now that meaning is a derivation from, an "abstract" of, the entire situation. Even in Blocked cases the emotional trace or compulsive action, being appropriate to the actual event, would seem somehow to have been derived from it.

Just what type of process must have taken place between the stimulus event and the effect registered in the subject's consciousness can, of course, be only the object of conjecture in a study of this kind. However, one possibility is that the stimulus is accessible at some unconscious level and that unconsciously he judges its significance for him. If, as seems possible, that judgment in different cases should meet with varying kinds and degrees of difficulty in crossing the threshold into consciousness, it could explain the variations noted. But further discussion of what may go on beneath the level of consciousness will be resumed later after consideration of the other types of cases.

II. Hallucinatory Group

Not all cases that were characterized by the sudden nonrational entry of a psi impression into consciousness were found to result in an "idea" or even an emotion or compulsion. In some the effect instead was a direct projection of the impression upon some part of the nervous system of the subject. Most frequently one of the special senses was involved; but also, as in feelings of pain, illness, or injury, it was less specific than these. The use of the term "hallucinatory" to cover experiences of these kinds is based upon (and enlarged from) the Myers (**2**) definition of veridical hallucination as a sensory experience that has no objective counterpart accessible to the senses, but which does correspond to events that happen elsewhere. By bringing that definition up to date so as to include events that happen at other times as well as places, it would cover the sensory experiences herein grouped. If the term "sensory" is understood to include all sensations from the body as well as those from the sense organs, the definition would cover all the experiences that were classified as hallucinatory.

Among the experiences that were expressed through the sense organs, those employing vision predominated, with hearing and smell occurring less frequently. In a type case of the first, the subject might think he saw his brother John and only later realize that his sight had deceived him and John was not there. In an auditory type experience he might hear John call and later find that the call coincided with the time of John's accident. In olfactory cases, he would smell something associated with John, a flower or a pipe, for example.

Although, as mentioned above, not all Hallucinatory experiences involve one of the five special senses, those that do so will be discussed immediately below and those involving sensations other than these will follow.

Example 16. Visual hallucination. Essential meaning: Death of son.
My mother-in-law told me that her son was in the habit of going into town each Saturday to get groceries. One Saturday, he was so late returning that they were worried, and she said that she and her brother and another person were watching the road for him when one of them said, "There he comes now." All three of them ran to the door, and he was running very fast with his market basket over his arm, but instead of stopping at the house, he ran on and jumped over the fence into swampy ground and disappeared. A few minutes later someone called and said that he had been killed in an automobile accident.

Example 17. Auditory Hallucination. Essential meaning: Baby is in danger.
I had the most vivid psychic experience. Our baby at the time was sleeping on two chairs on a soft pillow at the side of our bed. One night I heard my dead grandmother calling me. I did not then glance at the baby, but rushed down the hall to the room where my mother was sleeping. "Did you call me?" I asked. "Someone did. It sounded like Grandma." My mother replied sleepily, "No, no, I didn't call." I went back to my room. There I discovered to my horror that the baby, pillow and all, had slipped off the chair and lay under a heap of bed clothes on the floor. In another few minutes he would have smothered.

Example 18. Olfactory Hallucination. Essential meaning: Wife thinking of me.
When I was in the Army at about 10:00 one Sunday morning, I was alone in the barracks writing a letter. Suddenly I became aware of a very strong smell of roses. At first I paid no attention,

then it struck me queer as there was no one else present but myself. Then I seemed to get the feeling that I was in church with my wife. I looked at the clock and noted that it was 10:00. That same week my wife wrote and told me that she had placed roses in front of her favorite saint, entreating her to protect me, at the mass which was at 10:00.

Experiences in which it seemed that there was a direct projection of the impression upon parts of the nervous system less specific than the sense organs were reported with greater frequency than had been expected. They covered a wide range of nervous, muscular, and general bodily sensations. The individual concerned had no immediate hint as to the psi nature of the effect, but considered it an unexplainable physical infirmity until later when by inference or information he learned of the stimulus situation and realized that his experience was appropriate to that.

In the type case, an example might consist in a sudden feeling of physical shock, "I was knocked down," corresponding, it would be later learned, to the time of John's accident. In other cases he might experience extreme pain of the general nature of that that John must have suffered, although only later learning about the accident and only then recognizing the possible connection between the two events. The following are examples of cases of this type:

Example 19. Essential meaning: Daughter giving birth to baby. Here is a very recent experience of my daughter, the wife of an Army officer. Last autumn they returned from a two-year duty tour in Germany. They left behind their older daughter who had married a young officer whose term overseas had not ended. Our granddaughter announced the arrival on January 9th at 11:30, in Germany, of the most beautiful baby boy in the world. To avoid useless anxiety, she had not told us on this side of her prospects but on January 9th, her mother in Baltimore experienced the pains of childbirth, backache and bearing down, so severe and similar that the thought occurred to her that her daughter might be experiencing the actuality and made a record of the time. When the report came as above, the time was found to be the same.

Example 20. Essential meaning: Friend in accident has broken back. I awoke with a terrible backache, the first I had ever had, and my first words to my family were, "I had a terrible nightmare. I called Shirley." There was no connection for she had not been

in my dream. yet all day I kept thinking of her. Just as I was about to call her, the phone rang and a mutual friend called and told me to brace myself. Shirley had driven down to Texas and as she was driving back, the car overturned. She was thrown clear of the car and her back was broken.

Example 21. Essential meaning: Mother has operation on jaw. Last year I had an impacted wisdom tooth. Infection developed and an operation had to be performed. At the exact time I was going through it, my son in Niagara Falls had a terrific pain in his face and without any warning. It continued for the three days I hemorrhaged.

Example 22. Essential meaning: Husband will injure arm. (Precognitive) Once when I was first married, my husband was working away from home. Before going to sleep one night, I had a numb feeling in one arm and leg. No pain, but just a numb sensation. For no reason I suddenly thought of something happening to my husband. I wasn't at all excited, which is unexplainable, as I am and always have been nervous and excitable.
The next day at noon, my husband was brought home with a badly mashed and broken arm. I still wasn't excited, as I felt I had been told of this hours before, though actually the accident occurred several hours after my feeling.

In all these cases, the physical effect experienced was appropriate to the stimulus. In some cases it was accompanied by suggestive thought of the individual to whom it applied, but when this was so, the thought was vague and unaccompanied by conviction.

Discussion of Hallucinatory Group

Experiences in which psi effects are expressed by impressions from one of the special sense organs have, of course, been reported since the earliest times. Perhaps because of the predominance among them of visual types, including the more spectacular apparitional effects, the definition "veridical Hallucination" has commonly been thought of as applying only to experiences involving one or more of the five special senses. However, there seems no reason to exclude from the same category those involving sensations of other than this limited sensory origin. As Tyrrell observed, "any idea can be expressed in this way" (**6**); that is, as a hallucination created within the personality by "a controlling idea," rather than through normal, physical channels. Even though this author was not considering the multiplicity of sen-

sations that could arise within the nervous system of the percipient, but rather those arising on the exterior, still his analysis would seem to be applicable, for the internal evidence suggests no difference in the processes regardless of the origin of the sense data. In neither type of case is there any primary rational component, and in all there is the same characteristic of spontaneity, the same convincing sense to the percipient of the reality and genuine nature of the experience, and the same lack of other cause for it.

In all instances in this group, factual knowledge of the ESP stimulus is as entirely lacking in the primary experience as it is even in the Blocked cases. Only as the individual is able to interpret his sensations either by inference or later information, is he able to recognize its meaning and its hallucinatory character. There is even less knowledge conveyed by this method than by that used in Intuitive cases. In either type, evidently there is a limitation inherent in the medium employed, whether it be by idea, or by sensation. Further, in Hallucinatory cases as in Intuitive, it would seem that the stimulus must have been accessible unconsciously, and must have been recognized and assessed below the conscious level in order to have made possible the registering, in the nervous system of the subject, of an appropriate effect.

III. Unrealistic Dreaming Group

Cases in which the psi impression included detail and imagery, but of a fanciful unreal sort, were grouped together. Although in the group were found a few waking experiences, it was made up almost entirely of dreams. The waking experiences had the same general characteristics as those of the dreams. In both, the distinguishing feature was an element of unreal or fanciful detail.

Among the various individual cases, the degree of departure from reality in this detail varied greatly. In some it was slight, and in those the meaning was obvious. In the type case, for example, the subject might dream that he saw his brother laid out for burial, not in a coffin, but on a misty cloud. The meaning in such a case, whether believed or not, was obvious. Another person might dream that he was hunting for his brother and unable to find him. In such a case the meaning would be implied, but less obvious. In still other experiences, the fantasy might be so far removed from reality as to be interpretable only as symbolism. A dream that muddy water, for example, was separating him and John might be taken as a symbol of death.

Examples below are chosen to illustrate various types of fantasy beginning with some in which the departure from reality is slight and leading up to some in which it is so great as to be symbolic.

Example 23. Fantasy simple. Meaning (my daughter is pregnant) obvious, but embodied in a dramatization of the situation, including conversation.

My married daughter visited me. She said nothing about her condition. I have a bad heart and they never tell me anything. I dreamed she was pregnant and I teased her in my dream about it and she didn't deny it. I told my single daughter something of my dream. She laughed at me. About two weeks later, the married

one wrote that she was in a hospital being given penicillin and glucose and that her little five-year-old girl wouldn't have a little sister. She had miscarried.

Example 24. Meaning (sinking of the liner "Antonio," or newspaper account of that sinking) dramatized into a personal experience.

One time about 1920, I awoke from a dream. A white ship had been sinking in a vast blue calm sea, clear sunshine. It was slowly settling with a starboard list. In the dream, I went aboard the ship and was thankful that not a soul was aboard. Then I suddenly was concerned for the ship's cat but searched the vessel from stem to stern but could find no cat. I returned to the deck and thought how glistening white it was and wondered if it were scrubbed with sand, and woke up.

That afternoon I saw an *Evening Standard* placard, "Liner Antonio Sinks." I bought a copy of the paper. The ship had sunk in the Mediterranean in clear weather and in a perfectly calm sea with a starboard list. The list was attributed to shifting machinery. No lives were lost, the account concluded, and even the ship's cat was saved. That is the only time I ever have seen a cat mentioned in an account of that sort. This happened in the early morning before I was awake.

Example 25. Meaning (death of mother) dramatized into a child's somewhat fantastic concept of a funeral scene.

When I was at the age of nine my mother was taken from our home to the hospital where she was operated on for a ruptured appendix. My sister and I were sent to the neighbor's to stay the night. At 1:00, I had a vivid dream in which I saw my mother laid out on our baby grand piano which she played often, with beautiful flowers banked all about her. I realized that she was dead and I awoke screaming with fright. The neighbor came in to see what was wrong. I told her that mother was dead.

In order to console me they called the hospital, fully expecting to hear that mother had come through the operation successfully, but instead they were told that she had died at the time of my dream.

Example 26. Meaning (son will die) dramatized into a suggestive scene, with auditory effect included. (Precognitive)

In 1930 I lived in Cologne. I had been with my wife to a merry evening party, so there was no reason whatever for sad thoughts, but in the night I had the following dream. I saw before a large building an enormous staircase on which some men carried down a small casket covered with a black cloth. Simultaneously I heard a voice say to me quite distinctly, "This is a sign for you that a member of your family will soon die." This happened in the night of Tuesday. On the following Saturday, my second boy, a son of 16 years, had to be operated on because of appendicitis. On Sunday night he fell, owing to some unfortunate circumstances, through the window onto the street. Some passersby carried him dying back into the hospital.

Example 27. Meaning: Death of old acquaintance.

One of the rather rare waking cases of fantasy type, in which the imagined personal experience seems to be one that might have been suggested by the death.

I was working away at the dishes one morning as usual, and my thoughts were a thousand miles away. The day was bright and sunny and I suppose my eyes were looking at the field across from the back yard. For no apparent reason I found that I was hurrying along the main walk in a cemetery where we had a family lot. However, I did not turn down the path leading to our lot. I kept right on the main road and I was in quite a hurry. There was no sorrow attached to the scene and not the least surprise or curiosity. I was simply there, hurrying along in a casual sort of way, when I got to a certain lot which I had seen only once before, over 20 years ago. The lot belongs to an old neighbor friend and when his son had been buried I stood there in reverence some 20 years before. In the more recent years I had scarcely thougt of the M's burial lot.

Work chased the incident away. I had the whole day before me and forgot the entire scene. That night when I picked up the evening paper I did an unaccountable thing, which was to turn directly to the death column. Almost at once I looked directly at the name M------. The neighbor known to my childhood had

passed away in New York. She was 90. I had not heard from her for many years. I had once written to her at the time of my mother's death concerning immortality. She had replied that her faith in immortality was deep and that she would like to have some reassurance of it. I did not hear from her again.

Example 28. Meaning Friend will die. Fantasy rather extreme, approaching symbolization.
I had a dream about an old acquaintance, a Mr. D----- the Commissioner of Lahore. D----- was a little old gentleman much my senior in age and whom I did not know at all well. Like myself he did not go about very much and he played no games and was never seen at the club, but I liked him though I saw him so little.
I heard that he was indisposed but had no reason to think that he was ill. Yet one night I dreamed that I saw him toiling up a terrifically steep hill with a perfectly enormous rock bound upon his back. That was all, but I was so struck by this that in the course of the morning I called at his house to make inquiries about his health. I think it was one of his daughters who received me and I must confess she was a little surprised at my visit since I saw them so little. She told me that her father was very ill and that he had had hiccups for three days and nights and the doctors could not stop them. This seemed very serious and I went away without telling her of my dream, but felt sure that poor D----- would die, and he did within a very short time.

Example 29. Meaning: Number and kind of animals to be shot in the next day's hunt. Fantasy becomes recognizably symbolic (Precognitive)
When I was very young we lived in a heavily wooded part of Northern Maine. We lived there until I was 17 and at that time there was a lot of big game there, such as deer, moose and bear. We got part of our food by hunting and fishing.
When I was 16 one night I dreamed that I killed people with my rifle. Next day I went hunting and killed as many carribou as I had killed persons the night before in my dreams. After that every time I dreamed of shooting people, I would go hunting and I was sure of getting big game. One night I dreamed that I shot

two little girls. They were dressed alike and they looked alike. Apparently they were twins. The next day I went hunting and shot two little doe deer. They looked exactly alike and no doubt they were twins. Those dreams never failed.

Example 30. Meaning: A death in the family. Fantasy symbolic. Habitual. My mother lived several hundred miles away from her own people of which there was a large family. Every time she dreamed she saw a tree fall, there would come a death message. If it were an old person, it would be an old tree, if a young child, a young tree. This happened many times over a long period.

The preceding examples testify to the range of fantasy by which the psi impression may be carried. They show also that the reality may be either obvious in the fantasy or so removed as to constitute symbolism.

Discussion of Unrealistic Dreaming Group

As just shown, many degrees of deviation from reality of the psi impression were found, extending from the slight to the extreme. But the process involved seemed to be the same in all cases. In all, it suggested a tendency of the imagination or fancy to run on uncurbed by reality, as in fantasy or daydream. The extent to which the reality was thus embroidered seemed to be incidental, a difference in degree but not in kind. A definition of fantasy or day dreaming that fits the case rather well is given by White as an effortless, undirected mental activity, divorced from reality, originating in the memory and influenced by the desires and interests (**7**). The impressions in these cases give evidence of undirected mental activity not bounded by reality. However, they apparently have as their source, not the memory but the stimulus event. The latter, as well as the desires and interests of the subject, seems also sometimes to influence the course of the process.

The foregoing examples attest to the fact that the final form of the impression remembered upon waking probably varies as widely from individual to individual as would the dreams and daydreams of the people involved. Thus variations in degree on complexity of the dream fantasy could presumably arise from personality differences, perhaps from differences in the nature of the stimulus event, and also from variations in the conditions under which the dream occurred.

In some instances, especially if the fantasy took a disturbing or nightmarish turn, the individual might be awakened before its completion. In other cases, if it were uninterrupted, perhaps because it pursued quieter or more remote channels, the result might be far removed from the stimulus event. In such a case it would be interpreted as symbolic. Through all the variations, however, as stated above, the differences seem to be only differences of degree.

Again in these cases as in the preceding groups, the assumption seems pertinent that the stimulus event is accessible to the unconscious. It seems further that it somehow activates or is acted upon by a process similar to that of daydreaming or fantasy which develops into the variety of forms that indi-

vidual conditions cause or permit. Here as in the waking groups, the origin of the process seems to be below the threshold of consciousness. In this case, however, that process is fantasy sufficiently vivid to be recalled upon awaking.

IV. Realistic Dreaming Group

There was a large number of cases, mostly but not all dreams, in which detail in the psi impression took the form of clear and realistic and true mental imagery instead of imagery of a fanciful, unreal character as in the preceding group. Cases of this realistic kind were like motion picture (or still) reproductions of all or some part of the stimulus situation. Although imagery of various kinds, auditory, verbal, etc., was included, the visual kind predominated. In fact, it was experiences of this kind which in *Phantasms of the Living* were referred to as "mental pictures." And yet these were not hallucinatory experiences, for the subjects even in waking instances did not confuse them with actual vision but knew that what had occurred was seen "in the mind's eye" only.

In some of these cases, as in none of those considered in any of the above groups, the psi impression included all the details of the stimulus event. For example, the scene of brother John's wreck at the foot of the hill, the curve, and the precise nature of the accident might be sensed almost as if actually witnessed. In this study cases in which the stimulus event was so pictured have been considered as showing complete knowledge. Not all of them, however, were complete; various kinds of incompleteness occurred. In many cases the impression was as if gotten from a specific viewpoint and it then was subject to whatever limitations that viewpoint implied. For example, in some instances the subject might be unable to "see" the victim, and consequently would be unable to recognize him even though the geographical details were clear, correct, and unmistakable. Sometimes a scene only tangential to that of the stimulus event or one that would have been suggested by it was sensed.

Incidentally, one notable feature of the group as a whole was the predominance in it of precognitive experiences. Although precognitive cases occurred in most of the other groups the proportion of them to non-precognitive ones was not outstanding, but here precognitive dreams outnumbered other types in a ratio roughly of two to one.

The following examples represent complete and various kinds of incomplete cases of realistic dreaming, many of which are procognitive. Also illustrated are waking experiences which, although a minority, occur with greater frequency than do the exceptions of any other group.

Example 31. Meaning complete: Father's wreck.
When I was about 14 our high school basketball team had an excellent record and large following. This evening a friend of my father's had requested that my father take a group to a basketball game in a nearby town. I was ill that evening so I could not attend. When my father left he came up to my bedroom to say goodbye to me. At that time he didn't have his coat on or with

him. Later that evening it began to storm but that didn't impress me at all as I knew my father was a good driver. Still later that evening, around 11:00 P. M., I was lying in bed awake with my eyes closed. The radio was playing and my mother was sitting beside my bed knitting. As I said my eyes were shut and before my eyes appeared this scene: Our station wagon was smashed against the bridge. I could see my father lying there with his left leg broken and a cut over his left eye and his tan jacket covered with blood. Also I saw my sister lying there with her left leg broken. I was very upset by this scene so I asked my mother what coat my father had worn to the game, and she said his tan jacket. This upset me even more. About 20 minutes later we received a telephone call telling us about the accident which my father had had, exactly as I had seen. it.

Example 32. Meaning complete: Death of janitor. (Precognitive) My sister-in-law lives in Baltimore and teaches a Sunday School class in one of the Episcopal churches there. One Saturday night she dreamed that she was at Sunday School and that as she went out of the church, two men came from the basement of the church carrying a man on a stretcher with his hands folded across his breast, and upon looking closer she saw that it was the church janitor. The next morning when she was leaving Sunday School, she was quite shocked to see two men carrying a stretcher with the janitor on it, his hands folded on his breast just as she had seen them in her dream. The janitor was dead. A heart attack in the basement during Sunday School. Why should she have dreamed, not being a relative and not even knowing the janitor well?

Example 33. Meaning complete: Son with broken arm. Slight detail incorrect. (Precognitive) During the second year that my husband was in the service, our little boy broke his wrist. The night before it happened I dreamed that he was tossing in his bed, hot and feverish with his arm in a cast, and I felt faint from the smell of ether. The only thing that was different the next night was the ether smell, for they had given him gas when they set it.

Example 34. Picture exact but meaning (nature of mother's accident) not obvious.
My mother was a widow for several years and I am the older of her two daughters. The last year of her life she was in very poor health and she was with me most of the time. In July of 1934 she went to visit my sister. A few weeks later one night I dreamed that I stepped into another room and there sat mother with no covering over the upper part of her body and her face and body were swollen and black as though from black ointment. I said, "Oh, that old erysipelas!" She looked as she had 27 years before when she had had it and I had had to take all the care of her. I don't know how soon after the dream I received my sister's letter telling how mother had fallen with a pot of hot coffee and had scalded her face and upper part of her body badly and they were putting black ointment on her. The accident had happened before my dream.

Example 35. Picture exact, but situation involved. Reason for son's suffering (thirst) not sensed.
My mother was going quietly about her concerns one afternoon. My eldest brother with whom she always had a very close understanding was in California. I don't remembr whether we knew at the time that he was out on a prospecting trip or not. Suddenly my mother screamed out, "Walter is in danger. I see him lying on the sand with that dreadful patient look he has when he is in pain. I see doves flying around him." She paced the house in hysterical agony for some time, but gradually quieted down. In response to anxious inquiries my brother wrote that he had become separated from the group and was dying for lack of water. He lay down and rested on the sand. As he rested he noticed doves flying, following them he found water which was bad, but which saved his life. He walked into town safely. I was about 10 or 12 years old at the time, but I remember vividly the suddenness and violence of her distress.

Example 36. Meaning incomplete. Identity of accident victim not sensed.
Early in 1944 I had gone back to bed after arising, as I didn't feel well. I dreamed that a plane crashed. I saw three young men

lying there. One had on a different uniform from the other two like an officer's uniform. He was slim faced and dark headed. I said to the woman with me, "I must get a doctor quick. Billy is hurt. His back is injured and he may be hurt internally." I had seen this dark headed boy move his hand and I knew he was not dead. The ringing of my phone awoke me. It was a cousin of mine. He said his sister's son Billy was missing in action and she was nearly crazy. I hadn't seen him since he was small, so I asked him if Billy was slim faced, dark headed and an officer on a plane. He said yes. I told him to call her and tell her that he was alive but injured. In less than a week she got a letter from an English hospital. He had been lucky enough that the underground had gotten him. He and the others who were injured were carried by night in hammocks made of parachutes to safety. Two of the crew were killed.

Example 37. Incomplete. Not sensed whether or not daughter hurt. Viewpoint limited.
The Sunday School was giving the children a picnic a few miles below the town. My little girl Genevieve, who was 9 years old, had gone on the hay ride with the neighbor children. The superintendent was driving the truck and I had no uneasiness about their safety. Late that afternoon, about the time for the picnickers to be getting home, a neighbor dropped in and we sat talking on the screened porch. While she was talking my mind drifted off and I was seeing the truckload of children coming up the road a mile or more away. I saw them clearly, a noisy crowd on a hay ride and the older ones sitting with their legs hanging over the side. Suddenly from the other direction a car came down the road. Just as it reached the truck, it swerved in and scraped the edge clean of children. It was a horror of confusion with arms and legs in all directions. The picture snapped off suddenly then. I told myself it was only a horrible imagination, but it made me uneasy and I paid little attention to what my neighbor was saying. As she talked I kept on wishing the children would come.
It was not more than 30 minutes later that I glanced out the screen and saw a strange man bringing home the little girl that

lived next door. Instantly I knew there had been an accident. I ran to the child and asked where Genevieve was—not what had happened, for that I knew. The child was dazed and she looked at me like she didn't see me. Finally she said, "She is sitting down there." The man said she didn't know what she was saying, that there was nobody there now. The injured had been taken to the hospital and the others would be along as cars gave them a lift. I ran toward town and met Genevieve and the other neighbor children walking up from the car. They were unhurt but five children had broken limbs and lay for months in the hospital. When I learned the details about the accident, I knew I had seen it at the very time it happened and in exactly the way it happened, and I was too far away even to have heard the siren when the ambulance came to pick up the injured children. The judge ordered the drunk driver to work and pay all the hospital expenses, which he did.

Example 38. Picture exact but tangential to meaning. Death of cousin. (Precognitive)
The first night we lived in the new house, my mother had a dream. She said she saw a funeral down the street a way. The house where she saw it was quite small and occupied by Italians. The hall in it was so narrow that it was necessary to bring the casket out through the windows. But there was not much sense to that dream and what could it mean, if anything? But early in March, sure enough, a funeral procession came by our house. We heard afterward that the coffin was passed out through the window of the little house. On the other side of the street, waiting for the hearse and carriages to pass, was a uniformed Western Union boy. He hurried over to our house as soon as he could get by and gave mother a telegram. Her favorite cousin had been killed in an accident.

Example 39. Incomplete. Victim not recognized. Viewpoint inadequate. (Precognitive)
In 1912 after my marriage, my youngest sister (I am the oldestof 10) had scarlet fever and it left her with a heart condition which killed her in 1918. First in 1913 and then on three occasions during that time in my own home—once when cleaning the floor—I

had the following vision: A coffin across the fireplace in my father's back sitting room. The family all standing around including two aunts, one six feet, one five feet, standing with their backs to the organ. I mention this as we often moved the furniture around to make it into one large room by opening the folding foors. My eldest brother was in uniform. I couldn't place it at all in 1913. Then the war came. He had several different uniforms, including the last one in 1918 which was entirely different from the others. There were four members absent in each vision. One brother in France; my next sister to me was too late for the funeral services; two youngest members I couldn't see, so I never knew which one was in the coffin until the day when my father bent over and kissed his 12-year-old daughter goodbye. Then I realized that it was my vision and looked around and saw all the people and objects in my vision.

In all the above examples the impression consisted of a clear and detailed picture which may or may not have been a complete reproduction of the stimulus, and may have been concerned with the stimulus scene itself or with another related to or suggested by it.

Discussion of Realistic Dreaming Group

In many instances in this group knowledge of the total stimulus situation complete with all details got into consciousness. In no other type of case was this true. In all others there was some limitation upon the extent of knowledge. On that account it might seem that here at last there is no evidence of mental action below the threshold of consciousness and that in these instances, reality is somehow experienced directly and that the assumption here of a subconscious cognitive process is unnecessary. If such were the case, then these experiences would seem to have been produced by unconscious processes different from those of all the proceding groups. In a study of this kind, it cannot of course be proved whether or not such is actually the case. However, it is necessary to consider the cases in which the knowledge is incomplete as well as those in which it is complete. The general form of the experience in each seems the same. There is the same characteristic of faithful and generally true detail. There may be the same completeness of picture "seen" whether from it the meaning of the ESP stimulus is self-evident or obscure. The viewpoint may be such as not to permit identification of the most impoortant item of the stimulus situation, as in Example 39, or it may be a scene apparently only suggested by that situation, as in Example 38; but still the type of experience per se, is no different from that in complete cases.

In many ways the realistic imagery of cases in this group suggests that of memory. The technique employed seems very similar to that which produces the imagery commonly experienced in the recall of past events. If realistic imagery be a handy tool of memory, the recognition of past experience, perhaps one should not be surprised if it is found to serve as a preferred technique for the expression of future

(as well as distant) events, as it seems to do here. According to the data of Table 1, in this collection of cases it is a method preferred above all others, for nearly half of the total number of cases are found in this Realistic Dreaming group. And the group includes the majority of the dreams as well as the largest number of atypical (in this case, waking) experiences. It is necessary of course to keep in mind that the hazards of reporting may unduly distort the actual frequencies of occurrence of any case type. This especial type may be more likely to be reported or more likely to be well reported or even only more likely to be clearly remembered than other types. But if the apparent preference for this type be real, it would be understandable in view of the fact that visual imagery is obviously a most effective technique for reconstructing a scene or distant event. It is also common in dreaming, whether of real events or fictitious ones.

Again, as in each of the three preceding groups, it would seem first that knowledge of the stimulus must be accessible in the subject's unconsciousness; and somehow, from all the world's events, this one that has significance for him is selected and "reflected" directly or indirectly into his consciousness. Even in cases in which details, instead of essential meaning, are reflected, as when the chosen viewpoint discloses only the accident but not the identity of the victim, it seems necessary to assume that the identity must have been apprehended at some unconscious mental level. How else could its selection from all other wrecks then occurring be explained? And so, it would seem that experiences of realistic dreaming have their origin in the unconscious, just as all the others seemed to have, and that these as well as all the rest give evidence that mental action of unconscious nature has preceded the conscious experience.

DISCUSSION

The first point of interest arising from the foregoing study is the fact that approximately one thousand instances of human experience suggesting psi should fall as well as they do into general groups. There were marginal and borderline exceptions but they were comparatively few in number considering the large number of cases studied. The factor of subjectivity of judgment is recognized as a possible source of error, but still the fact stands out that the cases were classifiable, that spontaneous experiences from a subjective viewpoint can be "typed." No doubt the four types herein suggested could be more sharply outlined, but for the present, it seems worth while to consider these groupings and see what their implications may be. Their value will lie in that which they may suggest that can be followed up.

Most of the waking experiences have been divided into two groups, the Intuitive and the Hallucinatory. The Intuitive includes all those in which the meaning of a stimulus event or some associated sign of it emerged into consciousness as an immediate experience, simply as direct, nonsensory, and nonrationally received knowledge.

The Hallucinatory group includes all experiences in which the subject appeared to be experiencing an event sensorially even though there could be no actual sensory experience of the stimulus.

In dreams, whether "waking" or sleeping, psi experiences could be divided conveniently on the basis of whether they were unrealistic or realistic. If unrealistic, they showed the embellishment of imagination and fantasy in varying degrees from very little up to the entirely symbolic. If realistic, they

were like objectively true pictures, very much the same as the stimulus event would have appeared if sensorially perceived at the moment, though the subject, even if daydreaming, did not have the conviction of actual sensory experience that he did in the hallucinatory cases.

One of the first observations to make on the above four forms of expression of psi experience is that they are not unusual but already familiar ones. Each of the four commonly occurs in mental activity having nothing to do with psi. Intuitive psi experiences are so much like those of ordinary intuitive experiences of everyday life that the two are difficult to distinguish. The hallucinatory psi occurrence is less common, just as hallucinations are less common in nonpsi experiences. But considered as subjective experiences and from the viewpoint of the individual experiencing them, the two kinds of hallucinations are not distinguishable until later when those of the psi type are shown to be relevant to the stimulus evnet. Hallucinations that have nothing to do with psi are commonly associated with some exceptional condition, intense emotion, a drug effect, psychosis, strong hypnotic suggestion, or a vivid religious experience. There is a general impression that in psi experiences, hallucinations are the product of similarly exceptional states but this itself deserves to be the subject of a further study. It is true that certain differences between psi and non-psi hallucinations have been recognized (**3**). Those differences, however, concern what might be called the objective rather than the subjective aspect of the experience and are not made from the viewpoint of the individual who experiences the hallucination. Subjectively the two kinds of experience almost by definition must be similar.

Further, in both types of dreams, realistic and unrealistic, one would look in vain for an internal distinction, between those of psi and those of non-psi nature. Thus it seems that psi converts itself into a conscious experience by utilizing methods already well known in psychology and in common experience.

The implication arising from the fact that no peculiar method of expression of psi was found is quite different than if the reverse had been the case. For one thing, if the survey had tended to show that psi has a method of expression peculiar unto itself, it would have suggested that psi is a recent evolutionary development. Instead, the analysis points rather to a more fundamentally underlying place in nature for it. And so, along with other developments of recent years, the suggestion is given here that the acquisition of psi must have occurred a long way back in the evolution of species, back beyond the origin of the various forms of expression which it utilizes. The suggestion is that they evolved around it as an already existing function. This, however, is of course at this stage suggestion only.

The fact that spontaneous psi is expressed in different forms, even though familiar and commonplace ones, raises new questions, questions that could not come up before. If psi may be expressed in different forms, what causes the difference? Is it something inherent in the external situation or in the personality of the individual subject?

The most obvious contrast in conditions to examine (whether it be considered external or a factor of personality) is that of the extremes of consciousness, the dream versus the waking condition. As has been shown, most waking experiences fell into categories different from those of dreams, although there was some overlap. One might suppose that the two states of consciousness present different degrees of ease of transmission of an impression from the unconscious to the conscious. The sudden, sometimes

rather violent, character of many of the waking experiences could be an indication that considerable motivation is necessary for a break-through when an upsurge from the unconscious is strong enough to penetrate the preoccupations of consciousness. Those same preoccupations could well explain also the lack of detail in most waking experiences, in which case one might suppose that the most pertinent or poignant aspects, rather than nonessential details, might get through.

Dreams, on the other hand, could well be expected to show greater freedom of detail and imagery than waking experiences. Playing, as they do, close to the border of consciousness, they have of course long been recognized as convenient and likely vehicles for the conveying of unconscious impressions to consciousness. If this presumptive ease of transmission were the deciding factor, surely one should find most psi experiences embodied in dreams, rather than occurring as the result of waking processes. However, in this collection, nearly half the experiences involved occurred in the waking state. Although numbers in case studies cannot be reliable, still these hardly suggest that ease of transmission is the explanation, or at least that it is the only one responsible for the dream-versus-waking determination.

Of course, in some instances, timing and relevance could play a part. If the stimulus be an event that takes place when the subject is awake and if it is a situation of concern to him, it is understandable that the result (based on immediacy) might be a waking experience (or if occurring when the subject was asleep, a psi dream). But not all waking experiences are of great concern to the subject, even though the majority are, and many waking experiences and many dreams do not have to do with an event of the moment. Some of each kind are precognitive. Timing, therefore, does not explain them all. Neither does it or any other obvious external factor explain the choice in a given case as to whether, even if waking, the experience shall be of the Intuitive or the Hallucinatory type.

A similar question arises concerning the two dream groups. What determines whether a dream experience shall be a fantasy or a realistic one? In view of the common impression that dreams are imaginative, unreal, should one not expect most psi dreams to be of the fantasy type? But instead, in this collection, nearly three times as many dreams are realistic as unrealistic. The waking experiences that are included in these two categories also follow the same proportion as the dreams, there being many realistic to very few unrealistic cases. Evidently, therefore, mere coincidence of time and state of consciousness and ease of "break-through" do not adequately explain the determination of the method by which psi impressions are transferred to consciousness.

But if external conditions do not appear to be sufficiently explanatory, then it would seem all the more likely that individual personality differences are involved. It could be something peculiar to the individual that causes one case to be an emotion only, another an intuition of the bare meaning, and yet another a complete mental picture of a stimulus event. And whether psi can occur in the waking or only in a dream state might also perhaps be dependent on something in the personality. There might, in short, be personality factors that determine the kind of experience. But whether or not there are is still unknown, for the question has not been asked, could not be asked until the types themselves were outlined.

The above considerations suggest a new approach to the still baffling problem of the relationship of psi and personality. Of course, there has long been a general question as to whether any group of

individuals who have had any kind of psi experience could be differentiated by personality measures from those who have had none. The experimental outlook so far, however, does not promise an association of psi with personality traits or types, but as the most only with whether psi is allowed to operate in positive or negative fashion (**4**). It would be a different approach, therefore, to look for personality differences between individuals having intuitive and those having hallucinatory experiences; or between those having psi dreams and those whose experiences occur in the waking state; or between the realistic and the unrealistic dreamers.

If personality factors working on an unconscious level should be responsible for these forms in spontaneous stiuations, what part do they play in affecting the success of experimental techniques? Experiments are almost of necessity conducted in the waking state. It is possible that it would be an advantage to try to fit the technique to the subject. For example, if a subject hallucinates easily, he may succeed best by cultivating an hallucinatory way of making his responses in the ESP test.

There is another observation on the cases of these four groups that needs to be made. It concerns the feeling of conviction expressed by the subjects involved that their experiences were true, that something of significance had happened to them. In my earlier report (**5**), in a study of these same cases, it was found that more than fifty percent of the individuals concerned seemed to have been convinced of the veridicality of their experiences, even though such conviction was not rationally justified. It is not that the significance lies in a feeling of conviction in itself. All sorts of misguided and mistaken convictions prevail in people's minds. But a feeling of conviction along with a psi experience is the significant thing. In such cases the conviction is strong even though the subjective form of the experience is commonplace. The feeling of conviction therefore would seem to be something in itself. It can now be studied against the classification of subjective forms reported here. Whether or not it is independent of the form of the manifestation should be a study in itself. This combination of facts could well give a clue to the control of psi.

And now, finally, going back to reflections on the mode of operation of psi, commented upon earlier in the discussion of each of the four groups of cases, what theoretical implications are there? What unconscious process seems to be involved in the production of a psi impression from a stimulus event?

In each group the same basic suggestion emerged. This suggestion, not new to this study, was that there must be some unconscious reception, some causal relation between the subject's impression and the stimulus event. In other words, the stimulus must be "accessible," even though not at a conscious mental level. But more than mere accessibility seemed called for. In each of the four groups there was indication that the stimulus not only had been received or registered in the unconscious, but that to some degree it had been assessed as well. Even in cases of the complete realistic dream type, in which it seemed least necessary to assume that unconscious judgment had occurred, the selection of a pertinent stimulus could hardly be explained without the action of an unconscious mental process. In all other groups it seemed necessary to assume not only an unconscious act of selection but also some degree of judgment in order to account for the limitations and modifications of meaning and detail found in the various groups of cases. The total impression given, then, was of accessibility, plus unconscious selective judgment that assessed the significance of the stimulus event for the subject.

One could attempt to probe still deeper and ask, "How can the stimulus 'be accessible'?" And one can wonder by what sort of mental process the transfer of meaning from the stimulus to familiar processes like those herein involved (intuition, hallucination, realistic and unrealistic dreaming) can be effected, but at this stage speculation on obscure unconscious processes cannot be very profitable, hampered as it must be by the clumsiness of present concepts of mental life and by ignorance of the basic nature of personality.

The hope is that the study, like a miner's lamp, will at least have carried a bit of light a little farther into a dark area. Perhaps through it the place to dig for answers may be a bit more clearly suggested. At least by the classification as outlined, the question of improving it is raised. The present attempt is submitted, not as a finished product, but as itself a suggestion of what needs yet to be done to increase the insight by which the experimental approach is guided.

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Formes Subjectives des Expériences Psi Spontanées

RESUME : Dans cette étude, environ un milier de cas spontanés d'expériences psychiques ont été analysés pour les formes les plus courantes de ces vécus. Quatre prinicipaux types ont émergé : (1) Intuitives, dans lesquelles l'expérience du sujet était une simple impression non raisonnée ou une intuition. (2) Hallucinatoires, au cours desquelles l'expérience est projetée comme s'il s'agissait d'une sensation. (3) Rêves non réalistes, dans lesquels l'expérience était caractérisée par des fantasmes. (4) Rêverie réaliste, dans laquelle l'imagerie semblait aussi réelle qu'une photographie. Les Groupes (1) et (2) se produisaient seulement à l'état d'éveil.

Le fait que ces quatre types sont communs dans la vie mentale et ne sont pas propres aux expériences psychiques est important pour comprendre ces processus. La classification introduit de nouvelles questions, par exemple celle de savoir quelles différences de personnalité peuvent jouer un rôle dans la détermination de la forme que prendra l'expérience spontanée.

Il apparaît que le simple fait d'acquérir de la connaissance se produit au niveau inconscient, et la forme que l'impression va prendre dans la conscience représente le processus ou « mécanisme » mental par lequel la connaissance de l'événement stimulus franchit le seuil de la pensée. Certains actes de jugement sélectif sont inferrés comme se produisant en-deçà du niveau de la conscience. —Ed.

Subjektive Formen Spontaner Psi-Erfahrungen

ZUSAMMENFASSUNG: In dieser Studie wurden etwa tausend Fälle von spontanen parapsychischen Erfahrungen analysiert, um herauszufinden, welche Erlebnisformen die Erfahrungen im Allgemeinen annahmen. Es wurden vier Haupttypen gefunden: (1) Intuitiv, bei dem die Erfahrung des Probanden ein einfacher, unreflektierter Eindruck oder eine Ahnung war. (2) Halluzinatorisch, bei dem die Erfahrung so projiziert wurde, als handele es sich um eine Wahrnehmung. (3) Unrealistische Träume, bei denen die Erfahrung von Fantasie geprägt war. (4) Realistische Träume, bei denen die Bildeindrücke beinahe so realistisch wie auf Fotos waren. Die Gruppen (1) und (2) traten nur im Wachzustand auf.
Die Tatsache, dass diese vier Typen im seelischen Geschehen weit verbreitet und nicht für parapsychische Erfahrungen charakteristisch sind, ist für das Verständnis des Prozesses von Bedeutung. Die Klassifikation wirft neue Fragen auf, wie zum Beispiel, welche Rolle können Persönlichkeitsunterschiede bei der Bestimmung der Form spielen, in der eine spontane Erfahrung stattfindet..
Es scheint, dass der Wissenserwerb hauptsächlich auf unbewusster Ebene stattfindet und die Form, die der Eindruck im Bewusstsein annimmt, den mentalen Apparat oder den “Mechanismus” darstellt,
durch den das Wissen über das Reizereignis über die Schwelle übertragen wird. Vom Akt des selektiven Urteils wird angenommen, dass er unterhalb der Bewusstseinsebene stattfindet. - Hrsg.

Formas Subjetivas de Experiencias Psi Espontáneas

RESUMEN: En este estudio, se analizaron aproximadamente mil casos de experiencias psíquicas espontáneas para descubrir cuáles eran los tipos de experiencias más comunes. Se encontraron cuatro tipos principales: (1) Intuitivo, en el que la experiencia del sujeto fue una impresión o presentimiento simple y no racional. (2) Alucinatorio, en el que la experiencia fue proyectada como si fuera una sensación. (3) Sueños irreales, en los que la experiencia se caracterizó por la fantasía. (4) Sueños realistas, en los que las imágenes eran casi fotográficamente realistas. Los grupos (1) y (2) ocurrieron sólo en el estado de vigilia.
El hecho de que estos cuatro tipos sean comunes en la vida mental y no sean propios de las experiencias psíquicas es importante para comprender el proceso. La clasificación introduce nuevas preguntas como, por ejemplo, qué papel pueden jugar las diferencias de personalidad en la determinación de la forma que tomará una experiencia espontánea.
Parece que el acto principal de adquirir el conocimiento se produce a nivel inconsciente y la forma que tomará la impresión en la consciencia representa el dispositivo mental o “mecanismo” en el que el conocimiento del evento de estímulo sobrepasa el umbral. Se infiere que algún acto de juicio selectivo ocurre por debajo del nivel de consciencia. -Ed.

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Precognition of a Quantum Process [1, 2]

Helmut Schmidt [3]

ABSTRACT: In two precognition experiments, the subjects were faced with four colored lamps which were lit in random sequence. Their objective was to guess which of the four lamps would light up next and to press the corresponding button. In the first experiment, there were three subjects, who carried out a total of 63,066 trials. Their combined results were highly significant ($p < 2 \times 10^{-9}$).

In the second experiment, two of the same subjects plus a third had their choice of trying to predict, as before, which lamp would light up next (to try for high score) or to choose one which would not light next (low score). In a total of 20,000 trials, the subjects were again successful in achieving their aim to a highly significant extent ($p < 10^{-10}$).

For providing the random target sequence, use was made of single quantum processes which may represent nature's most elementary source of randomness. A practical advantage of the device is that it works fast and that the randomness can be easily computer tested.

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I. INTRODUCTION

The main objective of the experiment was to test the existence of precognition[4] of quantum processes. The experiment was set up so as to provide what the author believed (as a result of a study of the literature) to be particularly favorable conditions for the occurrence of ESP, but no systematic study was made of the psychological conditions affecting performance.

The following are offered as the features of the equipment most likely to be conducive to the occurrence of ESP.

a. The equipment is transportable so that tests can be done in the subject's home.

b. The recording is done automatically. This not only excludes recording errors, but also relieves the experimenter from the role of watchdog. If desired, the subject can be left alone with the apparatus without the possibility of fraud.

c. The subject can proceed unrushed at his own rate.

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1 [Originally published in *JP, 1969, 33,* 99-108]

2 Presented on Dec. 30, 1967 at the Winter Review Meeting of the Institute for Parapsychology.

3 It is the writer's pleasure to thank J. E. Drummond for continuous criticism and encouragement, G. Marsaglia for help with statistical problems, and the test subjects for their cooperation.

4No attempt has been made in this experiment to distinguish between precognition and psychokinesis as an explanation of the results. The reader may substitute "precognition or psychokinesis" for the term "precognition," used for simplicity.

d. The subject sees immediately whether or not his guess (registered by pressing one of four buttons) is correct; if it is, the lamp next to the pressed button lights. Thus the subject is faced with the challenge of "beating the machine."

The psychologically important characteristics of the test procedure are:

a. The two experiments to be reported were done with teams of preselected subjects.

b. The total length of each experiment (number of trials to be made) was specified in advance.

c. Test sessions were held only when the subject felt in particularly good shape. The length of each particular session was not specified in advance; it was terminated whenever the subject felt he would perform poorly.

Some critics suggested that one should specify in advance the days on which test sessions should be held and how many trials each subject should make in each particular session. These critics felt, intuitively, that the high scoring rate obtained might be due to the subjects' freedom to stop whenever their scoring rate fell below average. However, it is easily seen that, provided the target sequence is random and the subjects have no advance information (no ESP) about the next target, the probability for obtaining *n* hits among the *N* trials is given by the binomial distribution independent of how many subjects participated and how often and where the subjects stopped in between.

$$PN(n, p) = \binom{N}{n} p^n q^{N-n}, \text{ with } p = 1 - q = 1/4, \binom{n}{}$$

A more rigidly predetermined experimental schedule is advantageous for studying some details of ESP performance, like a possible decline of the scoring rate towards the end of a session, or differences in the performance of different subjects. In favor of psychologically optimal conditions, however, I restricted the objective of the experiments to testing only the existence of precognition.

II. THE ELECTRONIC TEST EQUIPMENT

During a test, the subject sits in front of a small panel with four pushbuttons and four corresponding colored lamps. Each of the pushbuttons simultaneously activates a recorder switch and a trigger switch. The recorder switch serves to register which of the buttons has been pressed. The four trigger switches are connected in parallel such that pressing any one of the buttons closes a circuit, in turn triggering the random lighting of one of the four lamps. The system is designed so that on repeated pressing of the buttons, the lamps light in random sequence, i.e., each lamp lights with the same average frequency, and there is no correlation between succesively lit lamps, or between the buttons pushed and the lamps lit. In part of the tests, the subjects had to guess repeatedly which lamp would light next and to register this guess by pushing the corresponding button. This triggered the random lighting of one lamp, and the subject could see immediately if the guess was correct, i.e., if the lamp next to the pressed button was lit. The objective here was to obtain a large number of hits, i.e., coincidences between the button pressed and the lamp lit. In the other part of the tests the subjects tried to obtain a small number of such coincidences by pressing a button next to any lamp which they expected would not light.

The Principle of Random Number Generation [5]

The random lighting of the lamps is provided by a quantum mechanical random number generator (Figure 1). Before a button is pressed, electrical pulses pass a gate and arrive at the rate of one million per second at an electronic four-position switch (modulo-4- counter), such that each arriving pulse advances the switch by one step, in the sequence 1,2,3,4,1,2,3,4,. . . . At this stage the lamps are unlit. After pressing a button, the gate is closed, so that the switch stops at random in one of its four possible positions, and the lamp corresponding to this position is lit.

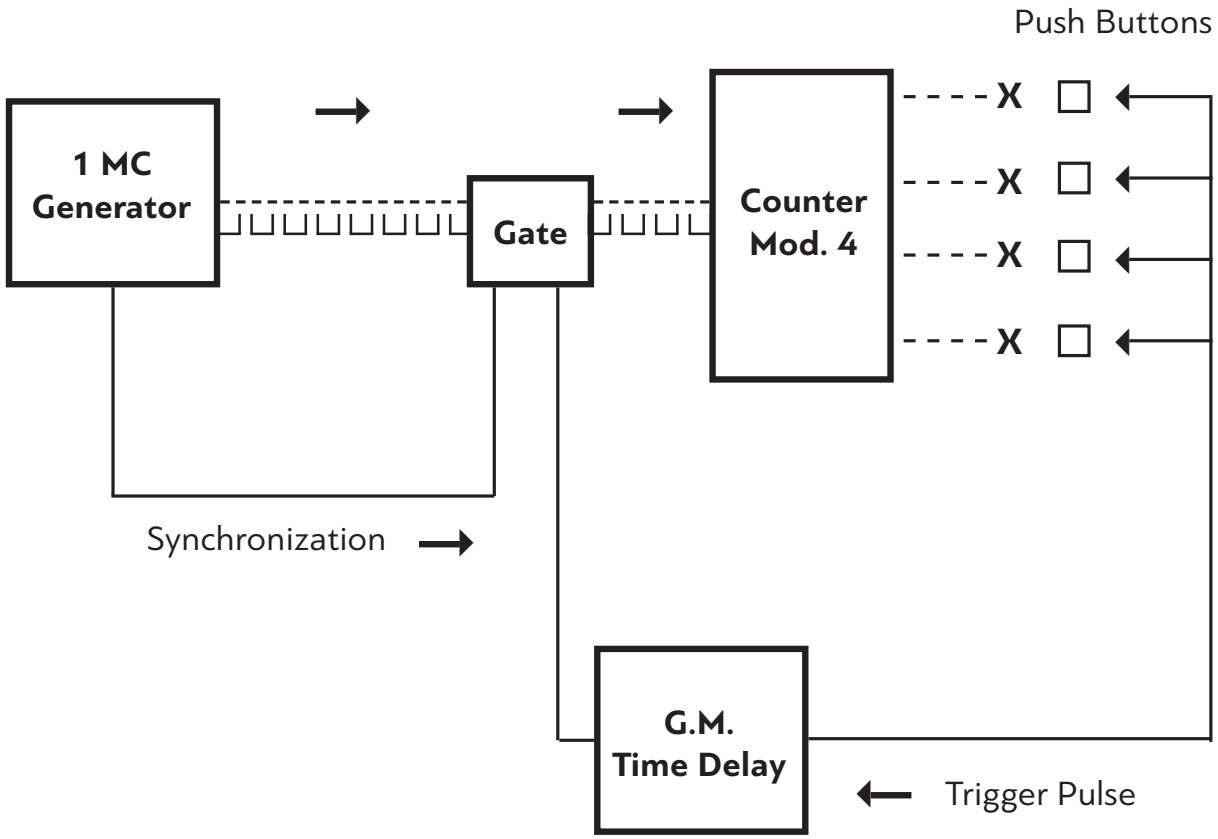

Fig. 1. The principle of random number generation.[6]

In order to exclude the (fairly remote) possibility that the subject might synchronize (within a millionth of a second) the pushing of a button with the high-frequency pulse generator and thus produce a nonrandom sequence, a time delay was introduced. After pressing the button, there is a waiting time of unpredictable length (average 1/10 sec.) before the gate is closed. This waiting time is determined by a single quantum process, the arrival and registration of an electron (from a radioactive strontium-90 source) as a Geiger-Müller tube. A further short-time delay guarantees that the gate is never closed while the modulo-4-counter is in the process of switching, which might impair the proper electronic operation.

5 Details on the circuitry and on the randomness tests are [were] available on request from the author.
6 [The graph in the original paper is better drawn than this scanned recreation.]

After the electron is registered and the switch has stopped, there is a locking time interval of approximately 1/2 sec. during which the mechanical counter for the number of trials advances by one step. The other counter, for the number of hits, advances by one step if the lamp corresponding to the pressed button has been lit. In addition, output channels receive voltage pulses which serve to register, on an external tape puncher, which button has been pressed and which random number has been generated. When the button is released and the locking time of 1/2 sec. has elapsed, the electronic switch continues advancing.

Recording and Safety Features

The RNG is designed to preclude fraud by the subject and to eliminate recording errors by the experimenter. In particular, simultaneous pressing of several buttons or extremely fast pressing of many buttons in succession does not impair proper operation of the machine. If one of the mechanical push-buttons is pressed, a corresponding electronic switch (flipflop) is flipped and, within less than one-millionth of a second, the other buttons are electronically blocked until the mahine has, after approximately 1/2 sec., completed the cycle described above and all the buttons have been released. Furthermore, if it should ever happen that two (or more) buttons were pressed simultaneously with an accuracy such that two (or more) of the electronic switches should flip, then the mechanical counters would become blocked and this event would not be registered.

The sequence of buttons pressed and lamps lit is recorded automatically on paper punch tape. In the research reported here, the two types of test (trying for a high or low number of hits) were recorded in different codes, such that the evaluating computer could distinguish between them. The numbers of trials made and hits obtained were displayed to the subject by electromechanical reset-counters. These numbers were also registered by nonreset counters, and the readings of all counters were regularly recorded by hand. This record agreed with the results obtained from the paper tape. The equipment was fraudproof, so that one could, in principle, let the subjects work alone. This was done, however, only in a small part of the tests with subject OC in the first experiment and did not increase the scores. In all other tests the writer was present in the same room with the subject.

III. RANDOMNESS OF THE TARGET SEQUENCE

For the interpretation of the experiments reported in this paper, it is most important to ascertain that the targets were sufficiently random, i.e., that their sequence did not have any pattern which the subject could detect and utilize for making correct predictions. Theoretically, the randomness should be guaranteed by the use of digital electronics in combination with a quantum device, provided no gross malfunction of the electronics occurred. But even though the electronic performance was checked repeatedly, explicit randomness tests of the generated numbers seemed desirable.

Analysis of the circuitry suggests that any breakdown should manifest itself in an increased generation rate in either one number or a pair of consecutive numbers. In randomness tests made on a sequence of five million generated and on paper tape recorded numbers, therefore, the frequency of the four possible numbers and of the 16 possible pairs of consecutive numbers was counted and evaluated. (Chi-square tests were applied to the whole sequence and to all 1,000 and 10,000 blocks in the

sequence.) No indication of non-randomness was found.[7] The five million numbers used for the randomness tests were recorded [8] on 100 different days, preferably directly after the experimental sessions. Thus the possibility that the electronics did work well in all randomness tests but might mal-function in most ESP tests is practically excluded. Consider also the other possibility, that there was some higher order pattern in the target sequence, a pattern which the subjects utilized, but which the randomness tests overlooked. Against such a pattern is the simplicity of the circuitry, for which all types of malfunction that could occur should affect the correlations tested. A further argument, which is stated only qualitatively, is that the majority of the subjects obtained their highest scoring rate in their first session of the preliminary tests, where they had a maximum of enthusiasm but a minimum of experience.

IV. THE TWO EXPERIMENTS

Prior to the experiments, preliminary tests were done with approximately 100 persons. Some of these were chosen because they reported having occasional "psychic experiences." These preliminary tests suggested that a few of the 100 persons could predict the lamp to be lit next fairly consistently, with slightly more than the expected 25% accuracy. For each of the two experiments, a team was selected from among these high-scoring people, and the total number of trials to be made was specified.

Whenever a subject from the team was available, eager to perform, and not too preoccupied with other matters, a test session was held. The number of trials to be made in a session was not set in advance, but the session was terminated when the subject lost interest or felt he would perform poorly. The whole experiment was terminated after the preset total number of trials was completed.

The tests were carried out in the persons' homes and great care was taken to have the subject work only under what seemed to be psychologically favorable conditions. During the tests, the paper tape recorder was connected to the RNG and the electromechanical reset and nonreset counters for the numbers of hits and trials were switched on. Between tests, the subjects were allowed to play with the machine in order to determine their momentary efficiency in predicting. During these play periods, the paper tape puncher and the nonreset counters were disconnected. It was decided in advance to evaluate all the events and only the events recorded on tape.

The First Experiment

From among the people who showed promise in the preliminary tests, three were selected for the first main experiment. Two of these (JB and KR) were professional mediums, while the third (OC) was an amateur psychic. The two mediums were chosen because of their being accustomed to "psychic" work over long periods of time. OC was selected because he had to stay at home with a broken foot and could thus fully concentrate on the tests.

The results are summarized in Table 1. It is seen that OC and KR obtained significantly high scores, while JB's score is only slightly above the expectation value. The total result is highly significant.

7 For example, it can be concluded with confidence 1017:1 that none of the numbers is generated systematically with a relative frequency above 0.252.

8 For this purpose, one of the pushbuttons was activated automatically at an average rate of one per second.

Table 1
RESULTS OF THE FIRST EXPERIMENT (FEB.—MAY, 1967)

Subject	No. Sessions	Prespecified No. Trials	No. Trials	Dev.	CR	*p*
OC	11	15,000 < *N* < 20,000	22,569	285.75	4.39	< 1 : 27,000
JB	5	20,000 < *N* < 25,000	16,250	90.5	1.64	< 1 : 6.5
KR	2	20,000 < *N* < 25,000	23,247	315.25	4.68	< 1 : 94,000
Total	18	55,000 < *N* < 70,000	63,066	691.5	6.36	< 2 X 10-9

In calculating *p* it was taken into account that the number (*N*) of trials to be made was prespecified only within certain limits. The *p* column gives the probablity for obtaining, by chance alone, the actual or a higher value of CR, anywhere in this *N*-interval.

The probability is less than 2 X 10-9 that chance would give a value this high or higher for the critical ratio (anywhere within the pre- specified *N*-interval).[9]

After the tests with the first subject, OC, were completed, the following control test was made: From the paper tape the whole sequence of the numbers predicted by OC (i.e., the sequence in which he pressed the buttons) is on record. In the control test the buttons of the machine were activated automatically in the same sequence in which OC had pressed them, with different input speeds. Ten such control runs were made. The numbers of hits above average obtained here (+5, +4, -53, +13, +54, +28, +73, +5, -20, -36) are not significantly high (OC's value +285, $\sigma = 65$).

The subject KR had in preliminary tests used two different approaches for obtaining high scores. In some tests he waited for an intuition concerning the next light and then pressed the corresponding button. In other tests, however, he concentrated on the red lamp (the colors were blue, green, orange, red), operated only the button corresponding to this lamp, and tried to enforce the lighting of this lamp with increased frequency by PK. KR used this latter approach throughout the reported test runs and succeeded in having the red lamp lit with significantly above expectation frequency. The experimental setup, however, does not permit a distinction between precognition and psychokinesis. KR might have obtained the high score also by pressing the button only at times when he felt, precognitively, a good chance for the red lamp to light; and conversely, the high scores of the subject OC might also be the result of PK.

9 The mathematical formulae necessary for the derivation of the probability (taking into acount the prespecified *N*-interval) may be obtained on request.

The Second Experiment

One of the subjects from the previous test (KR) had become unavailable and was replaced by SC (16-year-old daughter of OC). In this experiment, the subjects had the option of either to predict which lamp would light next and to press the corresponding button (try for high score) or to try to select one lamp which would not light and press that button (try for a low score). At the beginning of each session it was decided whether to try for a high or a low score. The two modes of operation were recorded on tape in different codes such that the evaluating computer could separate the two types of test. The results are summarized in Table 2. The subjects were so successful [10] that the probability for obtaining this or a better score by chance is less than $10^{-10.}$

V. DISCUSSION

The highly significant results which were obtained indicate that the subjects in this experiment were able to predict randomly selected events to a degree far exceeding what would be expected by chance.

Explanations of the high scores as the result of recording errors, chance, or nonrandomness in the target sequence can be ruled out with reasonable confidence, due to the precautions taken.

The experiments done so far do not permit a distinction (if such a distinction is at all meaningful) between the three possibilities:

Table 2

RESULTS OF THE SECOND EXPERIMENT (SEPT.—NOV., 1967)

Subject	No. Sessions	Goal	No. Trials	Hits Above Chance	CR
OC	4	High Score	5,000	+ 66	2.15
JB	high+low:	High Score	5,672	+123	3.77
JB	11	Low Score	4,328	- 126	4.42
KR	6	Low Score	5,000	- 86	2.81
Total	21		20,000	401	6.55
					$p < 10\text{-}10$ b

Note.—The subjects had the option to try for a high or for a low score. The number of trials to be made was prespecified to be either exactly 20,000 or exactly 40,000. Actually, 20,000 trials were made.

*Evaluated is the sum of the deviations (of the number of hits) from chance in the desired direction.

[b]The *p* is the probability for obtaining for the corresponding CR the actual or a higher value by pure chance.

10 Comparison of Experiment 1 and Experiment 2 raises an interesting question: Why did JB score so differently in these two experiments? JB thinks the improvement was due to some learning process. Another contributing factor might have been this: shortly before the second experiment, the writer gave JB the opportunity to demonstrate abilities in "psychometry" (a term meaning free association tests of ESP with the use of token objects) which might be, as judged from a few tests only, quite outstanding. This certainly did raise JB's self-confidence, remove some of the writer's prejudices against professional mediums, and thus create psychologically more favorable working conditions.

1. Precognition within the mind of the subject: the mind can pre-see a signal, which it will receive approximately 1/10 sec. later.
2. Precognitive coupling between the random number generator and the mind. The mind can pre-see directly the future state of the random number generator. (This mechanism should, contrary to the previous one, lead to high scores even if the subject is not, after the trial, informed by a lamp of the target aimed for.)
3. Psychokinetic coupling between the mind and the number generator.

But the available equipment can easily be adapted to more specific experiments.

Précognition d'un Processus Quantique

RESUME : Dans deux expérimentations de précognition, les sujets faisaient face à quatre lampes colorées qui étaient allumées selon une séquence aléatoire. Leur objectif était de deviner laquelle des quatre lampes allait ensuite s'allumer et de presser le bouton correspondant. Dans la première expérience, trois sujets ont réalisé un total de 63.006 essais. Leurs résultats combinés étaient hautement significatifs ($p < 2 \times 10\text{-}9$).

Dans la seconde expérimentation, deux des mêmes sujets et un troisième pouvaient choisir soit d'essayer de prédire la prochaine lampe qui devrait s'allumer (celle qui obtiendrait un score élevé), comme dans le test précédent, ou bien de choisir quelle lampe n'allait pas s'allumer (score faible). Dans un total de 20.000 essais, les sujets étaient à nouveau en réussite selon le but choisi avec un score très significatif ($p < 10\text{-}10$).

La source de la séquence aléatoire provenait de processus quantiques uniques qui pourraient représenter la source la plus élémentaire d'aléatoire dans la nature. Un avantage pratique de ce dispositif est qu'il fonctionne rapidement et que l'aléatoire peut être facilement testé par un ordinateur.

L'auteur est un physicien qui est particulièrement intéressé par les statistiques physiques et les fondements de la théorie quantique. Après avoir enseigné dans des universités en Allemagne, au Canada et aux Etats-Unis d'Amérique avant de rejoindre les laboratoires de recherche scientifique de Boeing. —Ed.

Präkognition Eines Quantenprozesses

ZUSAMMENFASSUNG: In zwei Präkognitionsexperimenten wurden den Versuchspersonen vier farbige Lämpchen gezeigt, die in zufälliger Abfolge aufleuchteten. Ihre Aufgabe bestand darin, zu erraten, welches der vier Lämpchen als nächstes aufleuchten würde und die entsprechende Taste zu drücken.

Im ersten Experiment gab es drei Versuchspersonen, die insgesamt 63.006 Einzelversuche absolvierten. Zusammengefasst waren die Ergebnisse hochsignifikant ($p < 2 \times 10\text{-}9$). Im zweiten Experiment hatten zwei dieser Versuchspersonen zusammen mit einer Dritten die Wahl, wie bisher vorherzusagen, welches Lämpchen als nächstes aufleuchten würde (um eine hohe Punktzahl zu erreichen), oder ein Lämpchen zu wählen, das als nächstes nicht aufleuchten würde (niedrige Punktzahl). In insgesamt

20.000 Einzelversuchen konnten die Versuchspersonen ihr Ziel erneut in einem hoch signifikanten Ausmaß erreichen ($p < 10\text{-}10$).

Zur Herstellung der zufälligen Zielfolge wurden Quantenprozesse verwendet, die die elementarste Zufallsquelle der Natur darstellen. Ein praktischer Vorteil des Gerätes besteht darin, dass es schnell arbeitet und die Zufälligkeit leicht am Computer getestet werden kann.

Der Autor ist ein Physiker, der sich besonders für statistische Physik und die Grundlagen der Quantentheorie interessiert. Nach Lehrtätigkeiten an Universitäten in Deutschland, Kanada und den USA ist er Mitarbeiter bei den Boeing Scientific Research Laboratories. -Hrsg.

Precognición de un Proceso Cuántico

RESUMEN: En dos experimentos de precognición, los sujetos observaron cuatro lámparas de colores que se encendían en una secuencia aleatoria. Su objetivo era adivinar cuál de las cuatro lámparas se iluminaría a continuación y pulsar el botón correspondiente. En el primer experimento, tres sujetos llevaron a cabo un total de 63,066 ensayos. Sus resultados combinados fueron altamente significativos ($p < 2 \times 10\text{-}9$).

En el segundo experimento, dos de los mismos sujetos más un tercero tuvieron la opción de intentar predecir, como antes, qué lámpara se encendería a continuación (para intentar obtener la puntuación más alta) o elegir una que no se encendería (puntuación baja) . En un total de 20,000 ensayos, los sujetos nuevamente tuvieron éxito en lograr su objetivo en un grado altamente significativo ($p < 10\text{-}10$).

Para proporcionar la secuencia objetivo aleatoria, se hizo uso de procesos cuánticos específicos que pueden ser la fuente más elemental de aleatoriedad de la naturaleza. Una ventaja práctica del dispositivo es que funciona rápidamente y que la aleatoriedad se puede evaluar fácilmente por computadora.

El autor es un físico particularmente interesado en la física estadística y los fundamentos de la teoría cuántica. Después de enseñar en Universidades de Alemania, Canadá y los Estados Unidos se ha unido a Boeing Scientific Research Laboratories. -Ed.

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Studying Individual Psi Experiences [1, 2]

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ABSTRACT: To understand psi, we must study it at multiple levels of analysis. We need a sociology of psi, examining its patterns in different cultures and social groups; life history data so that we can find causes for changes in an individual's psi ability; research on the conditions which affect ESP and PK in short periods like a single experimental session; and we also need careful examination of the individual psi experience. The latter is particularly difficult for several reasons: (1) The duration of the individual experience is unknown (a review of spontaneous cases suggests it may last for the briefest reportable flash, perhaps a tenth or hundredth of a second, or may continue for several minutes); (2) any one hit in an ESP experiment may be due to chance, not to psi; (3) psi often gives imperfect information (only partially correct or systematically misdirected); and (4) psi is not ordinarily under conscious control.
Suggestions for coping with these difficulties are examined. An ongoing experiment is described which investigates EEG changes related to the individual psi experience and, at the same time, attempts to teach subjects to identify and control psi success.

Our mental experiences flow in a stream of consciousness, as William James told us long ago; and as Freud told us only a little later, under this stream there are deeper flowing unconscious currents which sometimes rise to mingle with the conscious ones. All of these are a part of the ongoing flow of brain and other body processes; and of course these affect and are affected by the flow of events that make up our universe. A major research problem in parapsychology and indeed in any science is how best to try to understand their causal relations, and finally to try to predict and even control processes which are often so evanescent that they have merged into other processes by the time we begin to respond to them.

Almost the same problem comes up if we try to describe a movie or a television show. The simple, natural way is to state the theme or the plot outline; but this is such a vague, over-generalized summary that it loses the significant details. It is not good enough. At the other extreme we could isolate one frame—one static picture— and stay with it until it is fully described. This gives us precision; but even if we did it for each frame, it would leave out the most meaningful aspect of what we are talking about, its movement. Obviously this is not good enough either.

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1 [Originally published in *JP, 1970, 34,* 197-209]
2 This paper was presented as the luncheon address at the Winter Review Meeting of the Institute for Parapsychology on January 3, 1970.

But do we need to make a choice between one extreme and the other? Of course not. My basic argument to you today is that we need, for good description, multiple levels of analysis from the largest possible units to the smallest and most precise, and that somehow these analyses must be welded together so that they show the time sequences within each level and between levels. This thinking has led me to my current research, an attempt with Laurence Lewis to study the individual psi experience; and my talk today will present the line of reasoning, sketch out the project, and tell you of the high hopes that we hold for it now—the kind of unrealistically high hopes that we can enjoy in the planning stage before the data come in, but that are all too likely to be cast down as soon as we have results to study.

Let me continue with the basic argument. Any research project ordinarily compromises with the ideal multilevel description by choosing some convenient time sample and treating all the events that take place within it as if they were a unit. Sometimes we choose a big sample, like someone's childhood—those complex events of many years which can be (inadequately) summarized in a single word, "deprived" or "happy." Or we may choose a smaller sample like a child's learning to read or an adult's decision on how to vote in an election or a psychic's period of vacillation between his first psychic experience and his decision to develop his psi ability. Or we may select a still smaller sample, like the token-object sittings that Mrs. Garrett held here. Or a still smaller unit like the score of a subject on a single ESP run. In one sense it is fair to say that the bigger the sample, the better and more meaningful it is because it encompasses a reasonably large span of events; but on the other hand the bigger the sample, the worse it is, the less accurate, because it will blur separable processes into a single description. To put it briefly, we must choose breadth or accuracy or some fuzzy compromise.

Where does this leave us if as scientists we want to describe the psi process (or any other psychological function)? In a bad spot, because no single project will give us what we need. We must have information from varied approaches which give supplementary data.

I think, for example, that we need something we have never had: a sociology of psi, an analysis of why some cultures or some periods of history seem to produce more massive ESP or PK phenomena and more gifted sensitives than do others. But I also think we need the smaller scale study of individual lives. This could relate a person's shifting psi sensitivity—sometimes strong and sometimes weak or perhaps absent—to his changes in needs and moods, his distress and contentment and loving, his varying openness to the world around him, and also to the other persons with whom he is in contact and to their shifts in needs and sensitivity and response to him. But besides these sociological and life history approaches, I think we need research on smaller units, like a person's ESP or PK scores in a single session of a half-hour or hour (research in which I have a vested interest because I have been doing it for so many years). Even though today my emphasis will be on the virtues of the purest and smallest unit possible, the single response, you should not take this as derogating the other kinds of study. I am urging supplementation, not substitution.

Suppose for the sake of argument that we opt, as I am currently doing, for accuracy rather than breadth. Is it appropriate to choose as our unit the score of a singe ESP run? No, it is not. Both Dr. Rhine and Mrs. Rhine have separately told us that if we look closely at a single run we will find in it a combination of psi-hitting and psi-missing. To state that even a high score like a 12 shows good per-

formance throughout the run would be as bad a mistake—as silly, really—as saying that in Durham the mean annual temperature is 60 degrees and therefore ovbiously the Durham climate is moderately cool. Perhaps it is, overall; but to describe it without blur you must show how it ranges in irregular cycles from subfreezing to uncomfortably hot.

Then how small a time span do we need to capture the psi unit? Should it be the interval between one response and the next? No, this also is too long, because it can include so many hesitations, blank pauses, rejections of tentative decisions. What we must ask is, in effect: How long does the actual psi experience last? Should our units of measurement be milliseconds, or seconds, or very much longer? I wish we knew; but as things lamentably stand on the third day of 1970, we have neither the data that permit a firm statement nor the theory that would let us set up a good guess.

Where laboratory data and theory are lacking, our best source of information is likely to be naturalistic observation. I therefore have turned to the reports of those who collected spontaneous cases for us—from Frederick Myers to Louisa Rhine—for a first answer to this question of how long an ESP experience is likely to last.

The records give no clear directive. (This in itself may be our first useful item of information, an important warning that any single, simple answer is likely to fall far short of the truth.) Two major difficulties confront us in the case collections. The first is that so many of the best cases are dreams. This is no help when you are looking for time estimates, because even though reports from dream laboratories indicate that a dream is likely to last as long as would a corresponding experience in waking life, it still seems risky to assume a one-to-one relation. In this current reading I have therefore skipped the many dream reports (though perhaps it would be wiser to take them as a model and plan to put all our subjects to sleep before collecting data).

The second major difficulty is that so many of the waking cases describe two kinds of events. One is the psi information. The other is a changed state of consciousness: perhaps a feeling of being out of the body, or a sort of double vision which mysteriously includes everyday reality and the disparate psi hallucination, or an intense anxiety that continues for hours or recurs over several days. Should this changed state be considered an intrinsic part of the psychic experience? If it is, it implies that psi can last for a very long time. But we must hesitate before deciding, because within the prolonged period of altered consciousness there are likely to be only a few bits of information that seem psychic—what Ehrenwald calls “tracer elements.” Thus, if there is a forty-minute period of unusual, intense misery with the feeling, “Something is the matter with John,” we do not know if the psychic information itself is emerging during that entire period. Maybe it is; it might be coming through repeatedly in a redundant message. But another possibility is that the prolonged altered state represents the consequence of opening the gates for a short psychic message; it and the message may be the results of the same cause. And still a third possibility is that the long conscious state was caused by the psychic message but was not in itsef psychic. We do not yet know how to interpret it; I was interested but dared not use it.

What I looked for, to get a clearer answer, was case reports which seemd to describe specific psychic information and also to indicate upper and lower limits for the period in which specifics came through. Lower limits were easy to find. Again and again percipients said, “In a flash. . .” or “The picture

flashed into my mind," or "Suddenly I heard . . ." The smallest describable unit of psychological time seemed to set the lower limit for the psi experience. Since information on short-term memory suggests that 10 to 100 milliseconds is about how long it takes to process an item of visual information, we can infer that the same time range approximates the flash of a single psi impression.

But what is the upper limit? Are there longer psi experiences? Here is a case from an early report by Mrs. Rhine which sounds as if it lasted considerably longer. The woman who describes it writes:

> The Sunday School was giving the children . . . a hay ride. . . The superintendent was driving the truck and I had no uneasiness about their safety. Late that afternoon, about the time for the picnickers to be getting home, a neighbor dropped in and we sat talking on the screened porch. While she was talking my mind drifted off and I was seeing the truckload of children coming upthe road a mile or more away. I saw them clearly, a noisy crowd on a hay ride and the older ones sitting with their legs hanging over the side. Suddenly from the other direction a car came down the road. Just as it reached the truck it swerved in and scraped the edge clean of children. It was a horror of confusion with arms and legs in all directions. The picture snapped off suddenly then.

She goes on to write that her own child came home unhurt; that she then learned the accident had happened as she saw it, with several children injured, at just about the time of her vision; and that it had taken place so far away that she did not hear the sirens of the ambulances when they came to take the children to a hospital. It seems to have the ring of an authentic psychic experience; I found it impressive that she did not claim to know her own child was safe or to see which children were hurt. If we try to judge how long it could have taken her, while a neighbor was speaking, to see the first normal scene, then the car first going straight and later swerving, the catastrophe, and a part of its aftermath, it is clear that we cannot estimate with much assurance. Perhaps two seconds would be a minimum approximation and 20 to 30 seconds a more reasonable one.

This case seemed the longest of those I read in two books and over a dozen articles by Mrs. Rhine, though perhaps her case of a fruit vendor on 124th Street would be a competitor if its details were clearer. Back in the leisurely Victorian or Edwardian period, psi seemed to last longer; and if you read cases from recent decades and then from the decades about the turn of the century, you may agree with me that a sociology of psi would be useful to explain the differences. Take, for example, this story from the England of 1892, reported by Sir Joseph Barnby after a visit to the home of Lord and Lady Radnor:

> The Radnors had as house guest a woman designated as Miss A., who had had many psychic experiences, some of them induced by crystal-gazing. One evening Miss A. looked into a crystal ball and saw there a room which she described as "a kind of London dining room" with large squares of black and white marble on the floor. From this, and her later descriptions of people in the room, Lady Radnor thought she could identify Lord and Lady L. and their dining room in a house outside of London (which was, as Miss A. had said, "a *kind* of London dining room." Miss A. went on to say, "Here are a number of people coming into the room. Why, they're smelling their chairs!" then continued until it became clear to her listeners and to her that they were not smelling, but kneeling before their chairs for family prayers.

> She described the ceremony and the man conducting it, then said, "A lady just behind him rises from her knees and speaks to him. He puts her aside with a wave of the hand, and continues his reading." The woman who made the unconventional interruption seemed from the description to be Lady L. The next evening, when Lord and Lady L. visited the Radnors, Lady Radnor (in the presence of Sir Joseph Barby, who had heard about the last evening's crystal gazing) learned from Lord L. that the preceding evening at the time of the crystal vision, the L.'s had been at family prayers in the room that Miss A. described, that Lady L. had risen from her knees and spoken to him and that he had put her aside with a wave of his hand. The account sounds as if Miss A. had been observing in the crystal distant but contemporaneous events as they occurred and as if her psi experience continued with veridical detail for a substantial period of time, of the order of ten minutes.

Even in Victorian England, such a long psychic description was uncommon. Others I have found all came, as did this one, from individuals who had had many psychic experiences. However, in most cases the fresh items seem studded among generalized descriptions; they sound as if there was a long altered state of consciousness during which a few new details occasionally appeared. Perhaps our best guess about the duration of psi experience is that the distribution of times is J-shaped; that frequency is highest in the shortest unit of psychological time, so that almost all occur "in a flash"; but that they trail off into longer periods, perhaps extending in some few extreme cases to several minutes, or conceivably even longer.

This quick survey suggests several useful possibilities for research, and I will propose three. One is to work with the best, longest instances of psi because they offer more time to examine their concomitants. This would entail a two-stage project. The first step would be to find and encourage those who are capable of experiencing long psi interludes; the second to examine the conveniently long times when they occur. (I will return to this tempting method later and examine some of its problems.) A second kind of research would attempt to find those distinctive altered states on which psi is likely to be embedded. This also would be a two-part project, aimed at finding the distinguishing features both of the altered state and of the psi itself. A third line of research would be to take psi as it ordinarily comes, that is, quickly. We would need to use such short time-samples that we are, as nearly as possible, working with the pure period of the psi impression undiluted by the nonpsi states that precede and follow it. This probably, as I mentioned, means working with time units of the order of 10 to 100 milliseconds (though a possibility we must consider is that the shortest psychic event is so different from the shortest visual event that longer or even shorter periods are needed for registration and response).

Research with such short periods requires sophisticated instrumentation of the kind that only rather recently has been used in parapsychology, by such innovators as Tart or Ullman. By now, however, many others, like Kahn in Georgia and Van de Castle in Virginia, have moved in the same direction. I will tell you soon how I am beginning to probe along these lines. But first it is necessary to make a digression and describe still another difficulty that impedes our way. It will be a long digression, because the difficulty is compounded of three related but separable parts: that subjects can ordinarily neither be sure of recognizing psi when it appears nor of controlling its appearance; that psi information is often incomplete or misdirected (as in psi-missing or other displacement effects); and that there is no completely reliable way as yet of distinguishing for any one success whether it is due to ESP (or PK) or is only a matter of chance.

This last is a problem that all of us in parapsychology have lived with for a long time: identifying psi when it appears and distinguishing it from nonpsi. It is like the problems that physicists have in spotting a new particle from the tracks it leaves in a cloud chamber; it takes a high level of expertise to distinguish one track from another. It is a pressing problem in many parts of psychology too. Perception is often hard to distinguish from guessing guided by response bias; empathy can be confounded with a person's own natural mode of response. In parapsychology we put the difficulty this way: When is a particular hit an ESP hit instead of an accidental coincidence?

The simplest way of getting around the problem is for the experimenter to choose as the correct response one which is so distinctive that it would almost never appear by accident, one name or word or object out of the multitude of possible ones. If the subject identifies this unique item, it is a success; if not, it is a failure. The method may be a good one when the subject's ESP ability is strong. But as we all know too well, it is inefficient—not a good method— for studying run-of-the-mill, weak ESP ability. Most subjects would have so many hundred failures before a single success that both they and the experimenter would grow discouraged; the research would probably stop because of boredom before it was well begun. The method we use instead in parapsychology—and in signal detection research, and in running rats through mazes—is to specify as correct one out of some small number of possible responses. Even if a subject is functioning at or near the chance level he will have some successes this way; both he and the experimenter will feel more cheerful; and there is more likelihood of the project's being completed without dropouts.

Obviously the trouble with this useful approach is that it demands a large sample of responses. Because chance expectation of success is high, we need to average many trials to find what has been happening. We end up, in laboratory slang, with dirty data. Even when the overall score seems clearly better than chance, with ESP cards we must assume that about one hit in five tries was only a chance hit. If a preselected brilliant subject makes the phenomenal average of 9 hits per run over 20 runs, more than half of those hits should conservatively be assigned to coincidence rather than psi; and we do not know which are which. The run gives a blur of psi-hitting and psi-missing and perhaps displacement too; and it gives a blur of psi hits and chance hits even when hits and misses are segregated.

What can we do about it? My first recommendation is simple; we should use it to help build a model for psi. Consider again the hypothetical brilliant subject who was selected for special examination because of his prior good scores and who then for 20 runs averaged 9 hits per run. We infer that about 4 out of every 25 calls, on average, showed effective psi. But suppose that instead of ESP cards, our targets had been playing cards, or words from the dictionary, like Carington's targets. Would we expect the subject to average 4 extra hits out of 25 calls with these long-shot targets? Of course not. Even with gifted subjects, the number of extrachance hits changes as the odds change. This tells us something. Psi is not a simple on-off process which gives full information whenever it appears. To study it well, we must look for gating and decision mechanisms and for cases where it is part right, part wrong (displacement effects, or the near misses that Fisk found with his clock-card technique, or psi-missing, which we may think of as 180-degree displacement).

A second thing we can do when we try to clean up the data is a kind of microanalysis of scoring patterns within the run. We may find, for example, that the first segment (the first five calls) includes the whole surplus of hits; and in the rare cases where this occurs we can infer that the psi hits are those of the first segment. Or we may find that the surplus comes from hits on circles, as Fisk found in his research with erotic symbols. Or that it appears on the second or third target of a repetitive sequence, or only for the third item in an ABA type of sequence (like square, star, square; or circle, plus, circle). I do not need to remind you that Gaither Pratt found that this approach is usable; some subjects sometimes seem to function in almost as clear-cut a way as this, making it possible to segregate what seem to be their psi successes from what seem to be their chance successes. But one person will vary from time to time in his success-failure patterns (as Gaither Pratt also found), and of course subjects vary from each other. Long-term consistent effects of this clear-cut sort are too infrequent to help us much. Though this within-run microanalysis is interesting in itself, it does not seem likely to be an efficient technique for studying pure cases of psi.

A third course is to find gifted subjects with whom we can use longer odds so that they will give us cleaner data. This is a counsel of perfection: the easiest possible advice to give and the hardest to follow. One advantage is clear; even with long odds, such gifted subjects would have enough successes to stay hopeful. We could clean up the research without washing it out. Another possible advantage is that, by inference from the case material mentioned earlier, we could reasonably hope that gifted subjects would produce individual psi experiences which would last longer than the usual flash; and this would give us more time to explore their correlates.

But the disadvantages of restricting our research to gifted subjects are clear too. Such subjects are hard to find; and each is so different from the others (as even we nongifted folk are from each other) that when you work with a second one, your experience with the first is of little use to you. With each, you need a long preliminary project to identify when he is entering his own psi-hitting phase. By the time a gifted subject has been tested enough to establish that he is gifted and to show how he typically functions, he is likely to be so bored with the testing methods that he demands new methods to sustain his interest, and the new methods require a fresh preliminary investigation. This can grow discouraging.

One further possibility, a variant of the last one, is that instead of merely trying to find gifted subjects, we try to develop them. This is what Lewis and I are now attempting, even though training subjects in the laboratory has been tried often before with limited or no success. It may be that we are following a will-o'-the-wisp. At least it has the advantage that we can choose subjects—or let potential subjects choose us as experimenters—who are tolerant of laboratory procedure and will give a fairly long-term commitment to it. The big question is: Will the training procedure work? We have tried to hedge our bets by combining with the training, as part of the research design, an intensive study of each response. This makes it a kind of double- or triple-barreled project which examines the individual response and the accompanying events in which it is embedded and also tries to encourage the mental state which is conducive to psi. Let me take the last few minutes of this talk to tell you about it.

It starts from the work that Kamiya has been doing on alpha feedback. You probably know that when brain waves are analyzed on an EEG record, certain fairly slow, regular waves are for convenience

given the name "alpha." They are associated with drifting thoughts or a kind of relaxed, detached attention; they disappear in sleep and are inhibited by concentrated thought or interesting stimuli. Although these and others, like beta and theta waves, are presumably associated with mental functions, Kamiya was the first to show that we could become aware of when we are "in alpha" (that is, when our EEG shows clearly the distinctive alpha pattern). Kamiya's method, an elegantly simple one, was to signal a subject and ask, "Now?" The subject had to say either yes or no without the foggiest notion of what he was guessing about. If he had been in alpha and said yes, Kamiya told him that he was right; but if he had said yes when he was not in alpha, Kamiya told him he was wrong. Many subjects learned very quickly, with extraordinary success, to say yes and no at the right times. They had become conscious of a brain state which, until then, they had never identified. Later work showed that along with identifying this state, they had gained substantial control over it. They could put themselves in alpha voluntarily and could eliminate it when they chose.

This is fascinating in itself, quite aside from its implications for ESP. The alpha state is usually experienced as pleasantly calm, and I have even had one subject who thanked me enthusiastically for having learned it. She was a graduate student and had recently become so upset with one of ther textbooks that each time she opened it she found herself too anxious to understand what she was reading. After the alpha training she deliberately put herself into the alpha state before studying her next assignment in this text and found herself calm enough to read and understand it. Alpha training has seriously been suggested as a substitute for tranquilizers.

Meanwhile, in other laboratories besides Kamiya's, the same kind of feedback training has been used successfully for other body functions. Animals were trained by immediate rewards to speed up their heart beats or slow them down; humans have learned to change their skin temperature; there is a long list of previously unconscious functions which have been bought under voluntary control. This makes the method seem a natural for psi training, where the problem of establishing voluntary control of an unconscious function has given us such difficulty. And what we are trying to do in our own research—which, as I said, is just beginning, so that it's too early to tell you or even to guess ourselves if it's going to work—is first to train subjects in alpha, because we know this can be done. Then, after they have learned to identify and control alpha, we hope (in the classic phrase) that they will have "learned to learn" and we will set them to do their next learning with the indispensable help of Dr. Helmut Schmidt's machine; to try to identify psi and control it, just as they have learned alpha identification and control.

You probably see how this ties in with the general topic of the talk: studying individual psi experiences and their relation to other events. The machine that we use for recording gives a continuous record that can be divided into very small time-samples of the right order of magnitude for us. While the subjects are making their ESP calls, their brain waves are being recorded on the same tape as the ESP hit or miss. We can therefore analyze the records for general brain state over a long time-span like five minutes and can also analyze them for the particular state preceding and during the ESP call. One thing we will do, of course, is see if we replicate Honorton's finding that periods with a higher proportion of alpha give more ESP hits. We will also look at whether alpha is present during the individual psi hit or miss, and if not, whether any other distinctive wave pattern is present then. At best, we can teach identification and control of psi and find how it relates to the accompanying states in which it is embedded;

and from the latter information we will be able to distinguish psi hits from chance hits. At second best, we will turn up something unexpected. At a weak third best, we will have such clear null results that we will keep other experimenters from wasting time on the same useless project. In any event, we'll have been in there, trying.

Etudier Les Expériences Psi Individuelles

RESUME : Pour comprendre le psi, nous devons l'étudier à de multiples niveaux d'analyse. Nous avons besoin d'une sociologie du psi, examinant les patterns de ses manifestations dans différentes cultures et différents groupes sociaux ; des données biographiques de manière à comprendre les causes pour les changements de capacité psi individuelle ; recherche sur les conditions qui affectent l'ESP et la PK dans de courtes périodes telles qu'une unique session expérimentale ; et nous avons également besoin d'un examen minutieux des expériences psi individuelles. Cette dernière voie est particulièrement difficile pour plusieurs raisons : (1) La durée des expériences individuelles est inconnue (une revue de cas spontanés suggère qu'elles peuvent durer le temps d'un flash, soit un dixième ou centième de seconde, ou peut continuer pour plusieurs minutes) ; (2) tout succès à une expérience d'ESP pourrait être dû à la chance, non au psi ; (3) le psi donne souvent des informations imparfaites (soit partiellement correctes ou systématiquement mal orientées) ; et (4) le psi n'est pas habituellement sous le contrôle conscient.

Nous examinons des suggestions pour faire face à ces difficultés. Une expérimentation en cours est décrite dans laquelle les modifications d'EEG sont étudiées en fonction des expériences psi individuelles et où sont réalisés, en même temps, des essais pour apprendre aux sujets à identifier et contrôler les succès psi.

Zur Untersuchung Individueller Psi-Erfahrungen

ZUSAMMENFASSUNG: Um Psi zu verstehen, müssen wir es auf mehreren Untersuchungsebenen analysieren. Wir brauchen eine Soziologie von Psi, die seine Muster in verschiedenen Kulturen und sozialen Gruppen untersucht; lebensgeschichtliche Daten, damit wir Ursachen für Veränderungen in der Psi-Fähigkeit eines Individuums finden können; Erforschung der Bedingungen, die ASW und PK in kurzen Zeiträumen—wie in einer einzelnen experimentellen Sitzung—beeinflussen; und wir benötigen auch eine sorgfältige Untersuchung des individuellen Psi-Erlebens. Letzteres ist aus mehreren Gründen besonders schwierig: (1) Die Dauer der individuellen Erfahrung ist unbekannt (eine Auswertung der Spontanberichte deutet darauf hin, dass sie, für den kürzesten berichtbaren Einfall von vielleicht einer Zehntelsekunde oder Hundertstelsekunde, bis zu mehrere Minuten andauern kann); (2) jeder einzelne Treffer in einem ASW-Experiment kann zufällig und nicht durch PSI verursacht worden sein; (3) Psi liefert oft unvollkommene Informationen (nur teilweise korrekte oder in systematisch fehlgeleiteter Information); und (4) Psi entzieht sich normalerweise der bewussten Kontrolle.

Vorschläge zur Bewältigung dieser Schwierigkeiten werden geprüft. Ein in laufendes Experiment wird beschrieben, das EEG-Änderungen im Zusammenhang mit der individuellen Psi-Erfahrung untersucht und gleichzeitig versucht, den Probanden beizubringen, den Psi-Erfolg zu identifizieren und zu kontrollieren.

El Estudio de las Experiencias Individuales de Psi

RESUMEN: Para entender a los fenómenos psi, debemos estudiarlos en varios niveles de análisis. Necesitamos una sociología del psi, examinando sus patrones en diferentes culturas y grupos sociales; datos de la cronología de la vida para que podamos encontrar las causas de los cambios en la capacidad psi de un individuo; investigar las condiciones que afectan a la PES y la PK en períodos cortos como una sesión experimental única; y también necesitamos un examen cuidadoso de la experiencia individual de psi. Lo último es particularmente difícil por varias razones: (1) La duración de la experiencia individual es desconocida (una revisión de casos espontáneos sugiere que pueden durar el momento informable más breve, quizás una décima o centésima de segundo, o pueden continuar por varios minutos); (2) los aciertos en un experimento PES pueden ser debidos al azar, no a psi; (3) psi a menudo da información imperfecta (sólo parcialmente correcta o sistemáticamente mal dirigida); y (4) psi no está normalmente bajo control consciente.

Se examinan sugerencias para hacer frente a estas dificultades. Se describe un experimento en curso que investiga los cambios de EEG relacionados con la experiencia individual de psi y, al mismo tiempo, intenta enseñar a los sujetos a identificar y controlar los aciertos psi.

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A Joint Communiqué: The Psi Ganzfeld Controversy [1]

Ray Hyman and Charles Honorton

ABSTRACT: Instead of continuing with another round of our debate on the psi ganzfeld experiments, we decided to collaborate on a joint communiqué. The Honorton-Hyman debate emphasized the differences in our positions, many of these being technical in nature. But during a recent discussion, we realized that we possessed similar viewpoints on many issues concerning parapsychological research. This communiqué, then, emphasizes these points of agreement. We agree that there is an overall significant effect in this data base that cannot reasonably be explained by selective reporting or multiple analysis. We continue to differ over the degree to which the effect constitutes evidence for psi, but we agree that the final verdict awaits the outcome of future experiments conducted by a broader range of investigators and according to more stringent standards. We make recommendations about how such experiments should be conducted and reported. Specific recommendations are about randomization, judging and feedback procedures, multiple analysis and statistics, documentation, and the growing role we believe meta-analysis will play in the evaluation of research quality and the assessment of moderating variables. We conclude that psi researchers and their critics share many common goals, and we hope that our joint communiqué will encourage future cooperation to further these goals.

The *Journal of Parapsychology* had planned to publish one more exchange between us on the debate that we initiated in the March 1985 issue (Honorton, 1985; Hyman, 1985). In fact, one of us had already written and submitted his reply, and the other was preparing a rejoinder when we encountered each other at the 1986 meetings of the Parapsychological Association. The idea of replacing another round of exchanges with this joint communiqué emerged from a discussion during a luncheon meeting.[2, 3] During the discussion we realized that each of us had not fully and accurately understood the other's position on some of the major issues dividing us. In addition, much of our disagreement at this stage involves technicalities and differences of opinion about the proper ways to assign and rate studies on specific attributes. To put emphasis on these details detracts from the broader and more important propositions, on which we find ourselves in agreement.

These propositions relate in general to how psi researchers and critics can work together toward

1 [Originally published in *JP, 1986, 50,* 351-364]
2 Marcello Truzzi participated in this discussion. We would like to thank him for his encouragement and suggestions.
3 To the best of our knowledge, this is the first time a parapsychologist and a critic have collaborated on a joint statement of this type. Hyman prepared the first draft and we continued exchanging drafts until we had one we were both satisfied with. For those who are interested, the final product is draft 4.

the resolution of their differences. Specifically, they relate to how we believe psi ganzfeld experiments should be conducted and reported in the future.

GENERAL AREAS OF AGREEMENT

As in any other area of scientific inquiry, research in papapsychology requires continual scrutiny and criticism. Both critics and parapsychologists want parapsychological research to be conducted according to the best possible standards. The critic can contribute to this need only if his criticisms are informed, relevant, and responsible.

As to the psi ganzfeld data base, we agree, as our earlier exchanges indicate (Honorton, 1983, 1985; Hyman, 1983, 1985), that the experiments as a group departed from ideal standards on aspects such as multiple testing, randomization of targets, controlling for sensory leakage, application of statistical tests, and documentation. Although we probably still differ about the extent and seriousness of these departures, we agree that future psi ganzfeld experiments should be conducted in accordance with these ideals. In the second section of this joint communiqué, we shall make a number of specific recommendations about the conduct and documentation of future psi ganzfeld studies. It is our hope that these recommendations will lay the groundwork for a new round of studies that will serve to resolve the differences remaining between us.

Although we probably still differ on the magnitude of the biases contributed by multiple testing, retrospective experiments, and the file-drawer problem, we agree that the overall significance observed in these studies cannot reasonably be explained by these selective factors. Something beyond selective reporting or inflated significance levels seems to be producing the nonchance outcomes. Moreover, we agree that the significant outcomes have been produced by a number of different investigators.

Whereas we continue to differ over the degree to which the current ganzfeld data base contributes evidence for psi [4], we agree that the final verdict awaits the outcome of future psi ganzfeld experiments—ones conducted by a broader range of investigators and according to more stringent standards.

The strongest disagreements between us might appear to be over the relationship in the data base between "flaws" and study outcome. Honorton finds no significant correlation between indices of study quality and study outcome. Hyman agrees that there is no significant correlation between study outcome and some procedural indicators, such as multiple analysis, sensory leakage, statistics, and security. But he finds a positive correlation between study outcome and other procedural indicators, such as suboptimal randomization, feedback, and inadequate documentation.

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4 As regular readers of the *Journal* know, the term *psi phenomena* was introduced by Thouless and Wiesner as a neutral label denoting unexplained interactions between organisms and their environment. Their intention was to avoid the surplus meaning associated with terms such as *extrasensory perception.* Although this usage is generally understood within parapsychology, as reflected in the glossary definitions in this *Journal*, the term *psi* has unfortunately taken on broader connotations within the popular culture. Even within parapsychology, the terms *psi* and *paranormal* are sometimes used interchangeably and in a way that confuses description with explanation.
Consistent with the original usage, the term *psi* in this paper simply denotes a communications anomaly. No particular explanation of the anomaly is intended, nor do we believe any is warranted at the present time. We suggest that communication between parapsychologists and other scientists could be improved if this distinction were maintained.

Which correlation one obtains depends on how the "flaws" are assigned to individual studies, how one orders the seriousness of flaws when constructing scales, how many different attributes are included as flaws, and similar judgments. But these differences, no matter how controversial, should not be allowed to obscure our agreement that the present data base does not support any firm conclusion about the relationship between flaws and study outcome. Our disagreements about the actual correlation only emphasize this point.

If psi is responsible for the outcomes obtained in this data base, then the ganzfeld experiment should continue to produce successful outcomes when the various problems that Hyman pointed out are eliminated. Indeed, what differentiates the ganzfeld debate from many earlier controversies between psi researchers and critics is that the claim is one of replicability. Consequently, the best way to resolve the controversy between us is to await the outcome of future ganzfeld psi experiments. These experiments, ideally, will be carried out in such a way as to circumvent the file-drawer problem, problems of multiple analysis, and the various defects in randomization, statistical application, and documentation pointed out by Hyman. If a variety of parapsychologists and other investigators continue to obtain significant results under these conditions, then the existence of a genuine communications anomaly will have been demonstrated. The demonstration of an anomaly, of course does not explain it. Such a demonstration would, however, be very important because it would require acknowledgment that there is, indeed, something to be explained, and the debate would then shift toward such efforts. Whether the anomaly is ultimately to be considered "paranormal" will, as Palmer (1985) suggests, depend on further developments such as the extent to which the findings can be brought under lawful control and the construction of a positive theory of the paranormal.

On the other hand, if the findings can all be attributed to various artifacts, this too is important to determine. Discovering the nature of such artifacts and how they are produced could have important methodological implications for all scientific inquiry. Thus, we agree that further research in this area is important, not only for parapsychology, but for science generally. And we believe it is essential, in order to develop a clear picture of what is actually going on, that the research should be conducted not only by parapsychologists but by a range of investigators with diverse opinions concerning psi. Studies conducted by investigators who are skeptical of the psi hypothesis would be particularly useful from a number of perspectives. It is possible, for instance, that such studies might reveal potential sources of artifact that have been overlooked or that are not obvious from analysis of existing research reports. Further, investigators favoring conventional explanations of papapsychological findings could contribute substantially to a resolution of the psi controversy by systematically testing and delimiting the explanatory power of various proposed alternative hypotheses (Palmer, 1986).

Finally, before moving on to our recommendations for future psi ganzfeld studies, we believe it is appropriate to say a few words about the process in which we are engaged. As is evident from what has been said above, there are areas in which we continue to disagree. We agree to disagree. Even though our continuing disagreements about the degree to which the existing studies in this area contribute evidence for psi, for example, are not inconsequential, we fully respect each other's position and we disassociate ourselves from the more strident advocates on both sides of the psi controversy who would label those with opposing views by such pejoratives as "prejudiced," "credulous," and "irrational."

RECOMMENDATIONS FOR FUTURE PSI GANZFELD EXPERIMENTS

Although much of what we say in this section might also apply to other areas of parapsychological research, we will make our recommendations specific to the ganzfeld psi experiment and its data base that we discussed previously (Honorton, 1985; Hyman, 1985). The recommendations are intended to illustrate what a parapsychologist and a critic might accomplish when trying to seek common grounds for agreement. What follows are specific recommendations to other parapsychologists and other investigators who intend to conduct a ganzfeld psi experiment.

Control for Sensory Leakage

We agree that future investigators should strive to eliminate all possibilities for sensory communications between sender and receiver—both during the ganzfeld session and at judging. The typical two-experimenter psi ganzfeld experiment effectively eliminates sensory leakage during the actual ganzfeld period. The use of duplicate target pools or the binary coding system (Honorton, 1975) guards against sensory leakage at the time of judging. Proper attention to monitoring and recording the actual target should undermine the possibility, suggested by Hyman, of leakage from receiver to sender during feedback. Fortunately, the use of single target pools has disappeared in recent ganzfeld psi experiments.

Randomization of Targets

We agree that more careful attention needs to be given to the procedures for selecting targets and that the procedures should be thoroughly documented.

The method of target selection should be described in full. The following details should be included: (a) the person performing the randomization, (b) the specific source of randomness, (c) the method of sampling the random source (i.e., obtaining entry points for random number tables, seeds for pseudorandom generators, or specific values for hardware random generators). To illustrate what we agree would constitute adequate documentation of randomization procedures for each of the above random sources, consider a hypothetical ganzfeld experiment involving 20 target pools of four pictures each:

Target preparation was performed by R. H., a member of the laboratory staff who was not otherwise involved in the experiment. . . .

> *Random number tables.* Pools and targets were selected using the RAND tables (RAND Corporation, 1955). An entry point into the RAND table was obtained for each session as follows. The first digit of the row was determined by coin toss ("heads" = 0, "tails" = 1). A deck of numbered cards (0-9) was then shuffled and cut four times. The uppermost card for each iteration provided subsequent digits for the row. The block (0-9) was then determined by again shuffling the numbered deck. Cards bearing the digits 6-9 and 0 were then removed from the deck, which was again shuffled to determine the specific column (1-5) within the block. The first two digits within the range 01-20 thus provided the pool for the session. R. H. then removed the appropriate judging pool and left it where it could be retrieved by the experimenter. Only after this was done was the actual target for the session determined. This was done by repeating the above procedure and obtaining the first digit within the range 1-4.

Pseudorandom generators. Pools and targets were selected using the random number generator function in the Applied Statistics module of a Texas Instruments TI 59 Programmable Calculator (Texas Instruments, 1977). The seed was obtained by subtracting the six digits comprising the subject's birthdate from the six digits comprising the date of the session. A uniform random number within the range of 1-20 provided the pool for the session. R. H. then removed the appropriate judging pool and left it where it could be retrieved by the experimenter. Only after this was done was the actual target for the session determined. The next random digit within the range of 1-4 was the target.

Hardware random number generators. The targets were selected using a PsiLab II random number generator interfaced to an Apple II computer (Psychophysical Research Laboratories, 1984). A BASIC program sampled the RNG, such that the first byte value returned within the range of 1-20 was the pool. R. H. then removed the appropriate judging pool and left it where it could be retrieved by the experimenter. Only after this was done was the actual target for the session determined. This was done by repeating the above procedure and obtaining the first digit within the range of 1-4.

Although random number generators are often today more convenient than tables of random numbers, several caveats are in order regarding their use in serious research applications. In general, we do not recommend use of microcomputer random functions. The algorithms used are generally not documented, and some have been shown to produce spurious results (e.g., Hansen, 1986). A good discussion of the characteristics of some of the more widely used pseudorandom algorithms is given in Radin (1985). Hardware random number generators can have design flaws or may develop intermittent problems that will lead them to fail. And even though a brief description of the circuitry and its theory of operation is desirable, technical descriptions of the hardware cannot be substituted for empirical tests of the output. Ideally, randomness tests would be done on the actual targets used in an experimental series. However, owing to the typically small sample size of psi ganzfeld studies, such tests would be of little value. Control tests should be reported to insure adequate randomness of the targets. Because ganzfeld experiments involve only one target selection per session, sequential bias is not likely to be an issue as it could be in other areas of psi research, and the ganzfeld investigator can restrict his or her attention to a frequency analysis allowing assessment of the degree to which targets occur with equal probability. A good discussion of randomness tests is given by Davis and Akers (1974).

Hyman believes that the best way to assure adequate randomization is to include empirical, in addition to the usual theoretical, baselines. One way to do this would be to systematically compare the percipient's first choice both against the intended target and against the intended target for a control trial (which could be the actual trial for another percipient). If the randomization procedure is adequate, the control comparisions should produce observed means and standard deviations consistent with the theoretical distribution on the null hypothesis. This recommendation is identical to the "cross-check" method used in the early card-guessing experiments (Rhine et al., 1940/1966, p. 46).

Judging and Feedback

We agree that the judging and feedback procedures should be presented in greater detail than has generally been the practice in past ganzfeld studies. Specifically, the report should explicitly doc-

ument the following procedures: (a) the manner in which persons knowing the identity of the actual target (i. e., the sender and sender's experimenter) remain isolated from the receiver and receiver's experimenter until completion of judging; (b) the instructions given to the receiver for judging; (c) how the judging pool is presented to the subject; (d) the manner in which the subject's ranks or ratings are recorded; and (e) how feedback to the actual target is delivered at completion of the subject's judging.

Multiple Analysis

The problem of multiple analysis pervades all the sciences. Determining the size of the total critical region is often difficult even when the investigator has conscientiously set out in advance the tests that will be made. More typically the investigator has a more or less general idea of the hypotheses to be tested, but the precise details have not been worked out in advance. Under these conditions, the precise indices, cutting points, and tests are constructed after the data have been collected and assembled. The temptation is strong to tailor the specifics of the testing to the peculiarities of the data.

Even though it is not possible to make a generalized recommendation that will meet all contingencies, clearly investigators should specify all the confirmatory tests, as well as the precise critical region in advance of collecting the data, and such specification of confirmatory tests should be explicitly stated in the experimental report. Adherence to this recommendation should not be taken as being inconsistent with exploratory data analysis. The point of the recomendation is to prevent confusion between confirmatory tests and suggestive findings that require confirmation by future experiments. When multiple tests are planned, appropriate adjustments should be made to keep the total overall error rate within the commonly accepted region. One approach involves using the Bonferroni inequality (e.g., Rosenthal & Rubin 1984).

We recommend that future investigators consider the possibility of increasing statistial power by using, with appropriate adjustments, two or more of the several indices that have been used as indicators in psi ganzfeld experiments.

Hyman believes either theoretical or empirial investigations might suggest that a linear combination of two or more of these indices could usefully increase statistical power. Or, it might turn out that more power can be achieved by performing separate tests on two or more of these indicators and then adjusting the overall level of significance appropriately. It is not clear at this time what might be the best combination. If the investigator decides to use just two indices, for example, statistical considerations might suggest choosing those two that are least correlated. This would argue for using both direct hits and binary hits. Simulation studies by Hyman indicate that these two measures correlate 0.61, whereas the intercorrelations between any other pair of the most common indicators are approximately 0.80 or higher. On the other hand, the studies in the current data base that used binary hits rather than direct hits appeared to yeld less impressive results. Those studies that used the special slide pool allowing use of Honorton's binary coding system (Honorton, 1975) indicate that the resulting index is uncorrelated with the four major indices of direct hits, binary hits, sum of ranks, and normalized ratings. However, the binary coding index also seems to yield smaller and less significant effects (Hyman, 1985).

Honorton believes a good case can be made for using both direct hits and sum of ranks measures. Because the two measures are discretely distributed, the penalty required for using both is minimal. Consider, for example, a study involving 20 trials and a hit probability of .25. If alpha is set to .05 and the direct hits measure alone is used, a significant outcome will be achieved with 9 or more direct hits (p = .041). If the sum of ranks measure alone is used, significance will be achieved with a sum of ranks equal to or less than 41 (p = .036). If both indices, adjusted by the Bonferroni method, are used, significance will be achieved by either 10 direct hits or a sum of ranks equal to 40. Thus, the added flexibility achieved by allowing use of either measure is, in this case, purchased at a cost amounting to one additional direct hit. The Bonferroni method is overly conservative. As Hyman's simulation shows, direct hits and sum of ranks are highly correlated. Hansen (1986) has recently reported a simulation study involving these two measures and is preparing tables for various sample sizes that will provide more accurate p levels for the use of both direct hits and sum of ranks.

File-Drawer and Retrospective Experiments

Given the Parapsychological Association's policy of actively discouraging the selective reporting of "positive" results, the file-drawer problem is probably less acute in parapsychology than it is in many other scientific disciplines. Certainly reports of nonsignificant outcomes are far more common in the parapsychological literature than in other areas of psychology. This is not to imply, however, that the file-drawer problem is nonexistent in parapsychology. Investigators should bear in mind that registration of statistically nonsignificant outcomes is esential to the development of a realistic appraisal of a research area and that a study's value is independent of its statistical significance.

As to Hyman's suggestion concerning "retrospective" experiments, we recommend that along with specifying the critical region, the investigator should also specify in advance the status of the experiment. Designations such as *classroom exercise, confirmatory experiment,* or *process-oriented* will help future reviewers to both classify and properly evaluate the results.

Statistics

Over 20% of the experiments in the meta-analytic sample of 28 studies contained errors in the use of statistical tests. Although some of these errors may not have had serious consequences, their existence should be a cause for concern to the parapsychological community. We believe that the parapsychological journals, along with the authors, share responsibility for insuring the adequacy of statistical tests used in empirical contributions and that some of these problems could be avoided if authors adhered to the following recommendations printed on the inside back cover of this *Journal*:

1. State concisely the precise statistical formulation of the hypothesis being tested and list it in advance of the results section. It is recommended that the type of statistical tests that are planned be given along with the hypothesis.
2. For any statistical analysis that was not preplanned, give a brief statement of why it was done; the probability value should be placed close enough to this statement that its association is obvious.

3. When statistical analyses are done, report not only the inferential statistics (e.g., *t* values) but also the descriptive statistics for the data evaluated (e.g., group means and standard deviations). Also, report the actual values of correlation coefficients.
4. Have the data and statistical analyses independently rechecked before submitting the paper.

Documentation

In general, we believe that readers (including research analysts and prospective replicators) should be able to reconstruct the author's procedures from the descriptions provided in the experimental report. Although this is not common practice in science generally, we believe it is important in areas such as parapsychology where routine replicability cannot be taken for granted. More detailed exposition of methods and procedures should serve not only to aid evaluation of research quality, but also to increase the likelihood that other investigators will be able to replicate the original investigator's results successfully. As for future ganzfeld psi experiments, we recommend that, in addition to the procedural details described above, investigators routinely supply information on the following: the training, supervision, and qualifications of student experimenters; information on the subject population, including sources of subject recruitment and prior psi-testing experience; the individual ranks and target selections; the acquaintanceship of sender and receiver; the status of the experiment (confirmatory, exploratory, exercise, etc.); and similar information that is germane to the evaluation and replication of the study.

The Role of Meta-Analysis

The standards and recommendations we have discussed so far, for the most part, apply to the individual experiment. Indeed, almost all the guidelines for doing good research are aimed at the individual experiment. And the statistical procedures have been developed and taught with the idea that they apply to the evaluation of a single experiment. But scientific inquiry is cumulative and the outcome of a single experiment rarely, if ever, determines the acceptance or rejection of laws and theories. Science progresses by the cumulative outcomes of many experiments done by many investigators.

This fact has been recently recognized in the contemporary interest in the development of formal techniques for the statistical integration of a series of experiments. The field of meta-analysis is still in its infancy and somewhat controversial. Some of the controversy, as reflected in our debate, deals with the extent to which meta-analysis can compensate for the individual inadequacies of the specific experiments that are included in the data base.

Nevertheless, meta-analysis realistically emphasizes that scientific evidence rests on the consistency of results across many experiments. Before the focus on meta-analysis, the individual investigator designed, conducted, and reported the results with little, if any, consideration of how this particular experiment fit into a larger series of experiments.

Our next recommendation takes into account the growing role we believe meta-analysis will play, both in the evaluation of research quality and in the assessment of moderating variables. We urge parapsychological investigators to plan and report their experiments with the idea that their single experiment will contribute to a future meta-analysis. Much of this information could be encapsulated in summary tables at the end of the research report, as illustrated by Table 1.

TABLE 1

ILLUSTRATIVE STUDY SUMMARY

Subject information										
Session	ID	Recru	Belief	Pract	Test	Gz	Acq	Tgt	Resp	Rank
1	1	AD	7	1	1	0	0	3B	3D	2
2	2	ST	2	0	0	0	0	11A	11A	1
3	3	OS	5	1	0	0	0	6C	6B	3
4	4	EA	3	0	1	1	1	1D	1D	1
5	5	VI	6	1	1	0	2	17C	17A	4
*	*	*	*	*	*	*	*	*	*	*
*	*	*	*	*	*	*	*	*	*	*
*	*	*	*	*	*	*	*	*	*	*
*	*	*	*	*	*	*	*	*	*	*

Note. Abbreviations are defined as follows:

ID = subject ID.

Recru = source of subject recruitment.

AD = response to newspaper ad.

ST = student volunteer.

OS = recruited by other subjects.

EA = acquaintance or friend of experimenter.

VI = laboratory visitor.

Belief = belief in psi (1 = low, 7 = high).

Pract = practices such as meditation (0 = no, 1 = yes).

Test = prior psi-testing experience; not ganzfeld (0 = no, 1 = yes).

Gz = prior psi ganzfeld experience.

Acq = sender/receiver acquaintance.

0 = none; sender is laboratory staff member not previously acquainted with subject.

1 = lab friend; subject is a friend/acquaintance of laboratory staff member serving as sender.

2 = friend; sender is a friend/acquaintance of the subject.

Tgt = pool and target for session (e.g., 3A = Pool 3, element 'A').

Resp = pool element selected as first choice by subject.

CONCLUSION

In making these recommendations, we recognize the need to distinguish between ideals and practicality. We believe that the above recommendations are consistent with what is realistically attainable given the current resources of parapsychology. The psi ganzfeld paradigm is now over a decade old, and though the need for innovation and individual creativity is as great as ever, we believe there is also a need for greater discipline and standardization. We hope that parapsychological investigators and journal editors will welcome our suggestions and view them as a constructive step forward, one that, with their active cooperation, could lead to a broader-based consensus on at least the basic empirical "facts."

Our final recommendation, unlike the others, is probably not feasible under present circumstances, but it is proposed here only to indicate how many of the problems under debate could be avoided if certain ideals could be achieved.

Many of the problems we encountered in evaluating the ganzfeld psi experiments could be avoided in future experiments if the reviewers could be sure that they were dealing with the entire population of relevant studies and could insure the internal validity of those studies. Ideally, the best way to achieve this would be to sponsor a systematic replication series under the auspices of a neutral agency such as the National Science Foundation. The sponsoring agency would establish guidelines and rules based on the joint recommendations of successful investigators and knowledgeable critics. The guidelines would then delimit the experimental designs, the investigator-base, and the time frame for the experimental series, as well as the basic framework for a subsequent meta-analysis of the series as a whole.

The writing of this joint communiqué convinces us that, despite obvious differences, parapsychologists and their critics share many common objectives. These commonalities rarely are noticed in the debates, which focus on the differences. Yet such commonalities hold the key for how the parapsychologist and the critic can join forces to achieve the ends to which they both aspire.

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Un Communiqué Commun : La Controverse Psi Ganzfeld

RESUME : Plutôt que de poursuivre avec un nouveau round de notre débat sur les expérimentations sur le psi par le Ganzfeld, nous avons décidé de collaborer à un communiqué commun. Le débat Honorton-Hyman met l'accent sur les différences dans nos positions, dont plusieurs sont de nature technique. Mais lors d'une récente discussion, nous avons réalisé que nous possédions des points de vue similaires sur plusieurs problèmes concernant la recherche parapsychologique. Ce communiqué met donc l'accent sur nos points d'accord. Nous nous accordons sur la présence d'un effet globalement significatif dans cette base de données qui ne peut pas être expliqué raisonnablement par un biais de sélection des rapports ou des analyses multiples. Nous continuons à ne pas nous accorder sur la valeur de cet effet en tant que preuve du psi, mais nous sommes d'accord que le verdict final sera donné par les résultats de futures expériences conduites par un plus grand nombre de chercheurs et selon des standards plus rigoureux. Nous avons fait des recommandations sur la façon dont de telles expériences devaient être conduites et reportées. Des recommandations spécifiques portent sur les procédures d'aléatoirisation, de jugement et de feedback, sur les analyses multiples et les statistiques, la documentaion, et le rôle croissant que nous croyons devoir faire jouer aux méta-analyses dans l'évaluation de la qualité de la recherche et l'évaluation des variables modératrices. Nous concluons que les chercheurs psi et leurs critiques partagent des buts communs, et nous espérons que notre communiqué commun encouragera des coopérations futures pour atteindre ces objectifs.

Ein Gemeinsames Communiqué: Die Psi-Ganzfeld-Kontroverse

ZUSAMMENFASSUNG: Anstatt mit einer weiteren Runde unserer Debatte über die Psi-Ganzfeld-Experimente fortzufahren, haben wir beschlossen, an einem gemeinsamen Communiqué mitzuwirken. Die Honorton-Hyman-Debatte betonte die Unterschiede in unseren Positionen, von denen viele technischer Natur sind. Aber in einer aktuellen Diskussion stellten wir fest, dass wir zu vielen Fragen der parapsychologischen Forschung ähnliche Standpunkte einnehmen. Dieses Communiqué unterstreicht also diese übereinstimmenden Punkte. Wir stimmen darin überein, dass es in dieser Datenbank ein-

en signifikanten Gesamteffekt gibt, der nicht vernünftigerweise durch selektive Berichterstattung oder multiple Analysen erklärt werden kann. Unterschiedliche Meinungen existieren weiterhin darüber, inwieweit der Effekt einen Nachweis für Psi darstellt, aber wir stimmen darin überein, dass das endgültige Urteil vom Ergebnis zukünftiger Experimente abhängig ist, die von einem breiteren Spektrum von Forschern und nach strengeren Standards durchgeführt werden. Wir geben Empfehlungen, wie solche Experimente durchgeführt und berichtet werden sollen. Spezifische Empfehlungen beziehen sich auf Randomisierung, Beurteilungs- und Feedbackverfahren, multiple Analysen und Statistiken, Dokumentation und die wachsende Bedeutung, die Meta-Analysen unserer Meinung nach bei der Bewertung der Forschungsqualität und der Einschätzung von moderierenden Variablen spielen werden. Wir kommen zum Schluss, dass Psi-Forscher und ihre Kritiker viele gemeinsame Ziele haben, und wir hoffen, dass unser gemeinsames Kommuniqué die zukünftige Zusammenarbeit zur Förderung dieser Ziele unterstützen wird.

Un Comunicado Conjunto: La Controversia de Psi en Ganfeld

RESUMEN: En lugar de continuar con otra ronda de nuestro debate sobre los experimentos de psi en ganzfeld, decidimos colaborar en un comunicado conjunto. El debate de Honorton-Hyman enfatizó las diferencias en nuestras posiciones, muchas de las cuales son de naturaleza técnica. Pero durante una discusión reciente, nos dimos cuenta de que poseíamos puntos de vista similares sobre muchos temas relacionados con la investigación parapsicológica. Este comunicado, entonces, enfatiza estos puntos de acuerdo. Estamos de acuerdo en que hay un efecto significativo general en esta base de datos que no puede explicarse razonablemente por informes selectivos o análisis múltiple. Seguimos diferenciándonos en el grado en que el efecto constituye evidencia para psi, pero estamos de acuerdo en que el veredicto final espera el resultado de futuros experimentos realizados por una gama más amplia de investigadores y de acuerdo con estándares más estrictos. Hacemos recomendaciones sobre cómo se deben realizar y reportar tales experimentos. Las recomendaciones específicas se refieren a la aleatorización, los procedimientos de evaluación y retroalimentación, el análisis múltiple y las estadísticas, la documentación y el papel creciente que creemos que desempeñará el meta-análisis en la evaluación de la calidad de la investigación y la evaluación de las variables moderadoras. Llegamos a la conclusión de que los investigadores psi y sus críticos comparten muchos objetivos comunes, y esperamos que nuestro comunicado conjunto fomente la cooperación futura para promover estos objetivos.

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An Assessment of the Evidence for Psychic Functioning [1]

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[In this issue of the *Journal*, we are pleased to publish evaluations of government-sponsored research of psychic functioning, predominantly remote viewing, conducted at SRI International and Science Applications International Corporation (SAIC) during the past three decades. These evaluations were commissioned by the American Institutes for Research (AIR) at the request of Congress and the Central Intelligence Agency. The purpose was to assess the validity of psychic functioning and its potential applications as a means of determining whether the research should receive continued government funding. The AIR, in turn, formed a panel consisting primarily of Dr. Jessica Utts (a leading proponent of psi research) and Dr. Ray Hyman (a leading critic of parapsychology) to undertake the evaluations... –[Original] *Editor's Note*]

ABSTRACT: Research on psychic functioning, conducted over a two-decade period, is examined to determine whether the phenomenon has been scientifically established. A secondary question is whether it is useful for government purposes. The primary work examined in this report was government-sponsored research conducted at Stanford Research Institute (later known as SRI International) and at Science Applications International Corporation (SAIC).

Using the standards applied to any other area of science, it is concluded that psychic functioning has been well established. The statistical results of the studies examined are far beyond what is expected by chance. Arguments that these results could be due to methodological flaws in the experiments are soundly refuted. Effects of a magnitude similar to those found in government-sponsored reasearch at SRI and SAIC have been replicated at a number of laboratories around the world. Such consistency cannot be readily explained by claims of flaws or fraud.

The magnitude of psychic functioning exhibited appears to be in the range between what social scientists call a small and a medium effect. It is thus reliable enough to be replicated in properly conducted experiments, with sufficient trials to achive the long-run statistical results needed for replicability.

A number of other patterns have been found, suggestive of how to conduct more productive experiments and to produce applied psychic functioning. For instance, it does not appear that a sender is needed. Precognition, in which the relevant information is known to no one until a future time, appears to work quite well. Recent experiments suggest that, if there is a psychic sense, it works much as our other five senses do, by detecting change. Physicists are currently grappling with an understanding of time, and it may be that a psychic sense scans the future for major change, much as our eyes scan the environment for visual change or our ears allow us to respond to sudden changes in sound.

1 [Originally published in *JP, 1995, 59,* 289-320.]

The author recommends that future experiments focus on understanding how psychic functioning works and on how to make it as useful as possible. There is little benefit in continuing experiments designed to offer proof, since there is little more to be offered to anyone who does not accept the current collection of data.

1. INTRODUCTION

The purpose of this report is to examine a body of evidence collected over the past few decades in an attempt to determine whether psychic functioning is possible. Secondary questions include whether such functioning can be used productively for government purposes and whether the research to date provides any explanation for how it works.

There is no reason to treat this area differently from any other area of science that relies on statistical methods. Any discussion based on belief should be limited to questions that are not data driven, such as whether there are any methodological problems that could substantially alter the results. Too often people on both sides of the question debate the existence of psychic functioning on the basis of their personal belief systems rather than on an examination of the scientific data.

One objective of this report is to provide a brief overview of recent data as well as the scientific tools necessary for careful readers to reach their own conclusions based on those data. The tools consist of a rudimentary summary of how statistical evidence is typically evaluated and a listing of methodological concerns particular to experiments of this type.

Government-sponsored research in psychic functioning dates back to the early 1970s, when a program was initiated at what was then the Stanford Research Institute (now called SRI International). That program existed until 1989. The following year, government sponsorship moved to a program at Science Applications International Corporation (SAIC) under the direction of Dr. Edwin May, who had been employed in the SRI program since the mid-1970s and had been Project Director from 1986 until the close of the program.

This report will focus most closely on the most recent work, that done at SAIC. Section 2 describes the basic statistical and methodological issues required to understand this work. Section 3 discusses the program at SRI. Section 4 covers the SAIC work (with some of the details in Appendix B). Section 5 deals with the issue of external validation by exploring related results from other laboratories. Section 6 includes a discussion of the usefulness of this capability for government purposes. Section 7 provides conclusions and recommendations.

2. SCIENCE NOTES

2.1 Definitions and Research Procedures

There are two basic types of functioning that are generally considered under the broad heading of psychic or paranormal abilities. These are classically known as *extrasensory perception (ESP),* in which one acquires information through unexplainable means, and *psychokinesis*, in which one physically ma-

nipulates the environment through unknown means. The SAIC laboratory uses more neutral terminology for these abilities; they refer to ESP as *anomalous cognition (AC)* and to psychokinesis as *anomalous perturbation (AP).* The vast majority of work at both SRI and SAIC investigated anomalous cognition rather than anomalous perturbation, although there was some work done on the latter.

Anomalous cognition is further divided into categories based on the apparent source of the information. If it appears to come from another person, the ability is called *telepathy*; if it appears to come in real time but not from another person, it is called *clairvoyance*, and if the information could have been obtained only by knowledge of the future, it is called *precognition*.

It is possible to identify apparent precognition by asking someone to describe something for which the correct answer will not be known until later in time. It is difficult to rule out precognition in experiments attempting to test telepathy or clairvoyance, since it is almost impossible to be sure that subjects in such experiments never learn the correct answer at some point in the future. These distinctions are important in the quest to identify an explanation for anomalous cognition, but they do not bear on the issue of its existence.

The vast majority of anomalous cognition experiments at both SRI and SAIC used a technique known as *remote viewing*. In these experiments, a *viewer* attempts to draw or describe (or both) a *target* location, photograph, object, or short video segment. All known channels for receiving the information are blocked. Sometimes the viewer is assisted by a *monitor* who asks the viewer questions; of course, in such cases the monitor is blind to the answer as well. Sometimes a *sender* is looking at the target during the session, but sometimes there is no sender. In most cases the viewer eventually receives *feedback* in which he or she learns the correct answer, thus making it difficult to rule out precognition as the explanation for positive results, whether or not there was a sender.

Most anomalous cognition experiments at SRI and SAIC were of the *free-response* type, in which viewers were asked simply to describe the target. In contrast, *forced-choice* experiments are ones in which there are a small number of known choices from which the viewer must choose. The latter may be easier to evaluate statistically, but they have been traditionally less successful than free-response experiments. Some of the work at SAIC addressed potential explanations for why that might be the case.

2.2 Statistical Issues and Definitions

Few human capabilities are perfectly replicable on demand. For example, even the best hitters in the major baseball leagues cannot hit on demand. Nor can we predict when they will hit or when they will score a home run. In fact, we cannot even predict whether or not a home run will occur in a particular game. That does not mean that home runs do not exist.

Scientific evidence in the statistical realm is based on replication of the same average performance or relationship *over the long run*. We would not expect a fair coin to result in 5 heads and 5 tails over each set of 10 tosses, but we can expect the proportion of heads and tails to settle down to about one half over a very long series of tosses. Similarly, a good baseball hitter will not hit the ball exactly the same proportion of times in each game but should be relatively consistent over the long run.

The same should be true of psychic functioning. Even if there truly is an effect, it may never be replicable on demand in the short run even if we understand how it works. However, over the long run in well-controlled laboratory experiments we should see a consistent level of functioning, above that expected by chance. The anticipated level of functioning may vary based on the individual players and the conditions, just as it does in baseball; but if players of similar ability are tested under similar conditions, the results should be replicable over the long run. In this report we will show that replicability in that sense has been achieved.

2.2.1 P values and comparison with chance. In any area of science, evidence based on statistics comes from comparing what actually happened to what should have happened by chance. For instance, without any special interventions, about 51% of births in the United States result in boys. Suppose someone claimed to have a method that enabled one to increase the chances of having a baby of the desired sex. We could study their method by comparing how often births resulted in a boy when that was the intended outcome. If that percentage was higher than the chance percentage of 51% *over the long run*, the claim would have been supported by statistical evidence.

Statisticians have developed numerical methods for comparing the results of an experiment to what is expected by chance. The *p* value is the answer to the following question: *If chance alone is responsible for the results, how likely would we be to observe results this strong or stronger?* If the answer to that question (i.e., the *p* value) is very small, then most researchers are willing to rule out chance as an explanation. In fact, it is commonly accepted practice to say that if the *p* value is 5% (.05) or less, we can rule out chance as an explanation. In such cases, the results are said to be *statistically significant*. Obviously, the smaller the *p* value, the more convincingly chance can be ruled out.

Notice that when chance alone is at work, we *erroneously* find a statistically significant result about 5% of the time. For this reason and others, most reasonable scientists require replication of nonchance results before they are convinced that chance can be ruled out.

2.2.2 Replication and effect sizes. In the past few decades scientists have realized that true replication of experimental results should focus on the *magnitude* of the effect, or the *effect size*, rather than on replication of the *p* value. This is because the latter is heavily dependent on the size of the study. In a very large study, it will take only a small magnitude effect to rule out chance convincingly. In a very small study, it would take a huge effect to rule out chance convincingly.

In our hypothetical sex-determination experiment, suppose 70 out of 100 births intended to be boys actually resulted in boys, giving a rate of 70% instead of the 51% expected by chance. The experiment would have a *p* value of .0001, quite convincingly ruling out chance. Now suppose someone attempted to replicate the experiment with only 10 births and found 7 boys, that is, also 70%. The smaller experiment would have a *p* value of .19 and would not be statistically significant. If we were simply to focus on that issue, the result would appear to be a failure to replicate the original result, even though it achieved exactly the same 70% boys! In only 10 births it would require 90% of them to be boys before chance could be ruled out. Yet the 70% rate is a more exact replication of the result than the 90%.

Therefore, although *p* values should be used to assess the overall evidence for a phenomenon, they should not be used to define whether or not a replication of an experimental result was "successful." Instead, a successful replication should be one that achieves an effict that is within expected statistical variability of the original result or that achieves an even stronger effect for explainable reasons.

A number of different *effect size* measures are in use in thesocial sciences, but in this report we will focus on the one used most often in remote viewing at SRI and SAIC. Because the definition is somewhat technical, it is given in Appendix A. An intuitive explanation will be given in the next subsection. Here, we note that an effect size of 0 is consistent with chance, and social scientists have, by convention, declared an effect size of 0.2 as small, 0.5 as medium, and 0.8 as large. A medium effect size is supposed to be visible to the naked eye of a careful observer, whereas a large effect size is supposed to be evident to any observer.

2.2.3 Randomness and rank-order judging. At the heart of any statistical method is a definition of what should happen "randomly" or "by chance." Without a random mechanism, there can be no statistical evaluation.

There is nothing random about the responses generated in anomalous cognition experiments; in other words, there is no way to define what they would look like "by change." Therefore, the random mechanism in these experiments must be in the choice of the target. In that way, we can compare the response to the target and answer the question: "If chance alone is at work, what is the probability that a *target* would be chosen that matches this *response* as well as or better than does the actual target?"

To accomplish this purpose, a properly conducted experiment uses a set of targets defined in advance. The target for each remote viewing is then selected randomly, in such a way that the probability of getting each possible target is known.

The SAIC remote-viewing experiments and all but the early ones at SRI used a statistical evaluation method known as *rank-order judging.* After the completion of a remote viewing, a judge who is blind to the true target (called a *blind judge*) is shown the response and five potential targets, one of which is the correct answer and the other four of which are "decoys." Before the experiment is conducted, each of those five choices must have had an equal chance of being selected as the actual target. The judge is asked to assign a rank to each of the possible targets, where a rank of 1 means it matches the response most closely, and a rank of 5 means it matches the least.

The rank of the correct target is the numerical score for that remote viewing. By chance alone the actual target would receive each of the five ranks with equal likelihood, since, despite what the response said, the target matching it best would have the same chance of selection as the one matching it second best and so on. The average rank by chance would be 3. Evidence for anomalous cognition occurs when the average rank over a series of trials is significantly lower than 3. (Notice that a rank of 1 is the best possible score for each viewing.)

This scoring method is conservative in the sense that it gives no extra credit for an excellent match. A response that describes the target almost perfectly will achieve the same rank of 1 as a response that

contains only enough information to pick the target as the best choice out of the five possible choices. One advantage of this method is that it is still valid even if the viewer knows the set of possible targets. The probability of a first place match by chance would still be only one in five. This is important because the later SRI and many of the SAIC experiments used the same large set of photographs as targets. Therefore, the experienced viewers would eventually become familiar with the range of possibilities since they were usually shown the answer at the end of each remote-viewing session.

For technical reasons explained in Appendix A, the effect size for a series of remote viewings using rank-order judging with five choices is (3.0 - average rank) / √2. Therefore, small, medium, and large effect sizes (0.2, 0.5 and 0.8) correspond to average ranks of 2.72, 2.29 and 1.87, respectively. Notice that the largest effect size possible using this method is 1.4, which would result if every remote viewing achieved a first place ranking.

2.3 Methodological Issues

One of the challenges in designing a good experiment in any area of science is to close the loopholes that would allow explanations other than the intended one to account for the results.

There are a number of places in a remote-viewing experiment where information could be conveyed by normal means if proper precautions are not taken. The early SRI eperiments suffered from some of those problems, but the later SRI experiments and the SAIC work were done with reasonable methodological rigor, with some exceptions noted in the detailed descriptions of the SAIC experiments in Appendix B.

The following list of methodological issues shows the variety of concerns that must be addressed. It should be obvious that a well-designed experiment requires careful thought and planning:

1. No one who has knowledge of the specific target should have any contact with the viewer until after the response has been safely secured.
2. No one who has knowledge of the specific target or even of whether or not the session was successful should have any contact with the judge until after the judging has been completed.
3. No one who has knowledge of the specific target should have access to the response until after the judging has been completed.
4. Targets and decoys used in judging should be selected using a well-tested randomization device.
5. Duplicate sets of target photographs should be used, one during the experiment and one during the judging, so that no cues (such as fingerprints) can be inserted onto the target that would help the judge recognize it.
6. The criterion for stopping an experiment should be defined in advance so that it is not called to a halt when the results just happen to be favorable. Generally, that means specifying the number of trials in advance, but some statistical procedures require or allow other stopping rules. The important point is that the rule be defined in advance in such a way that there is no ambiguity about when to stop.
7. Reasons, if any, for excluding data must be defined in advance and followed consistently, and the reasons should not be dependent on the data. For example, a rule specifying that a trial could be

aborted if the viewer felt ill would be legitimate, but only if the trial was aborted before anyone involved in that decision knew the correct target.

8. Statistical analyses to be used must be planned before collecting the data so that a method most favorable to the data is not selected post hoc. If multiple methods of analysis are used, the corresponding conclusions must recognize that fact.

2.4 Prima Facie Evidence

According to *Webster's Dictionary*, in law prima facie evidence is "evidence having such a degree of probability that it must prevail unless the contrary be proved." There are a few examples of applied, nonlaboratory remote viewings provided to the review team that would seem to meet that criterion for evidence. These are examples in which the sponsor or another government client asked for a single remote viewing of a site, known to the requestor in real time or in the future, and the viewer provided details far beyond what could be taken as a reasonable guess. Two such examples are given by May (1995) in which it appears that the results were so striking that they far exceed the phenomenon as observed in the laboratory. Using a post hoc analysis May concluded that in one of the cases the remote viewer was able to describe a microwave generator with 80% accuracy, and that of what he said almost 70% of it was reliable. Laboratory remote viewings rarely show that level of correspondence.

Notice that standard statistical methods cannot be used in these cases because there is no standard for probabilistic comparison. But evidence gained from applied remote viewing cannot be dismissed as inconsequential just because we cannot assign specific probabilities to the results. It is most important to ascertain whether or not the information was achievable in other standard ways. In Section 3 an example is given in which a remote viewer allegedly gave codewords from a secret facility that he should not have even known existed. Suppose the sponsors could be absolutely certain that the viewer could not have known about those codewords through normal means. Then even if we cannot assign an exact probability to the fact that he guessed them correctly, we can agree that it would be very small. That would seem to constitute prima facie evidence unless an alternative explanation could be found. Similarly, the viewer who described that microwave generator allegedly knew only that the target was a technical site in the United States. Yet he drew and described the microwave generator, including its function, its approximate size, how it was housed, and the fact that it had "a beam divergence angle of 30 degrees" (May, 1995, p. 15).

Anecdotal reports of psychic functioning suffer from a similar problem in terms of their usefulness as proof. They have the additional difficulty that the "response" isn't even well defined in advance, unlike applied remote viewing, where the viewer provides a fixed set of information on request. For instance, if a few people each night happen to dream of plane crashes, then some will obviously do so on the night before a major plane crash. Those individuals may interpret the coincidental timing as meaningful. This is undoubtedly the reason many people think the reality of psychic functioning is a matter of belief rather than science, since they are more familiar with the provocative anecdotes than with the laboratory evidence.

3. THE SRI ERA

3.1 Early Operational Successes and Evaluation

According to Puthoff and Targ (1975), the scientific research endeavor at SRI may never have been supported had it not been for three apparent operational successes in the early days of the program. These are detailed by Puthoff and Targ (1975), although the level of the matches is not clearly delineated.

In one of the apparent successes, concerning the "West Virginia Site," two remote viewers purportedly identified an underground secret facility. One of them apparently named codewords and personnel in this facility accurately enough to set off a security investigation to determine how that information could have been leaked. Given only the coordinates of the site, the viewer first described the aboveground terrain, and he then proceeded to describe details of the hidden underground site.

The same viewer then claimed that he could describe a similar Communist Bloc site and proceeded to do so for a site in the Urals. According to Puthoff and Targ, "the two reports for the West Virginia Site, and the report for the Urals Site were verified by personnel in the sponsor organization as being substantially correct" (p. 8).

The third reported operational success concerned an accurate description of a large crane and other information at a site in Semipalatinsk, USSR. Again the viewer was provided with only the geographic coordinates of the site and was asked to describe what was there.

Although some of the information in these examples was verified to be highly acurate, the *evaluation* of operational work remains difficult, in part because there is no chance baseline for comparison (as there is in controlled experiments) and in part because of differing expectations of different evaluators. For example, a government official who reviewed the Semipalatinsk work concluded that there was no way the remote viewer could have drawn the large gantry crane unless "he actually saw it through remote viewing, or he was informed of what to draw by someone knowledgeable of [the site]." Yet that same analyst concluded that "the remote viewing of [the site] by subject S1 proved to be unsuccessful" because "the only positive evidence of the rail-mounted gantry crane was far outweighed by the large amount of negative evidence noted in the body of this analysis." In other words, the analyst had the expectation that in order to be "successful" a remote viewing should contain accurate information only.

Another problem with evaluating this operational work is that there is no way to know with certainty that the subject did not speak with someone who had knowledge of the site, however unlikely that possibility may appear. Finally, we do not know to what degree the results in the reports were selectively chosen because they were correct. These problems can all be avoided with well-designed controlled experiments.

3.2 The Early Scientific Effort at SRI

During 1974 and early 1975, a number of controlled experiments were conducted to see whether various types of target material could be successfully described with remote viewing. The results reported by Puthoff and Targ (1975) indicated success with a wide range of material, from "technical"

targets, such as a xerox machine, to natural settings, such as a swimming pool. These and some of the subsequent experiments, however, were criticized on statistical and methodological grounds. I briefly describe one series of experiments and criticisms of it to show the kinds of problems that existed in the early scientific effort.

The largest series during the 1973 to 1975 time period involved remote viewing of natural sites. Sites were randomly selected for each trial from a set of 100 possibilities. They were selected "without replacement,"meaning that sites were not reused once they had been selected. The series included eight viewers, including two supplied by the sponsor. Many of the descriptions showed a high degree of subjective correspondence, and the overall statistial results were quite striking for most of the viewers.

Critics attacked these experiments on a number of issues, including the selection of sites without replacement and the statistical scoring method used. The results were scored by having a blind judge attempt to match the target material with the transcripts of the responses. A large fraction of the matches were successful. But critics noted that some successful matching could be attained just from cues contained in the transcriptsof the material, such as instances when a subject mentioned in one session what the target had been in the previous session. Because sites were selected without replacement, knowing what the answer was on one day would exclude that target site from being the answer on any other day. There was no way to determine the extent to which these problems influenced the results. The criticisms of these and subsequent experiments, although perhaps unwelcome at the time, have resulted in substantially improved methodology in these experiments.

3.3 An Overall Analysis of the SRI Experiments: 1973-1988

In 1988 an analysis was made of all of the experiments conducted at SRI from 1973 until that time (May et al., 1988). The analysis was based on all 154 experiments conducted during that era, consisting of over 26,000 individual trials. Of those, almost 20,000 were of the forced-choice type, and just over 1000 were laboratory remote viewings. There was a total of 227 subjects in all experiments.

The statistical results were so overwhelming that results that extreme or more would occur only about once in every 1020 such instances if chance alone were the explanation (i.e. the *p* value was less than 10-20). Obviously some explanation other than chance must be found. Psychic functioning may not be the only possibility, especially since some of the earlier work contained methodological problems. However, the fact that the same level of functioning continued to hold in the later experiments, which did not contain those flaws, lends support to the idea that the methodological problems cannot account for the results. In fact, there was a talented group of subjects (labeled G1 in that report) for whom the effects were stronger than for the group at large. According to May, the majority of experiments with that group were conducted later in the program, when the methodology had been substantially improved.

In addition to the statistical results, a number of other questions and patterns were examined. A summary of the results revealed the following:

1. "Free-response" remote viewing, in which subjects describe a target, was much more successful than "forced-choice" experiments, in which subjects were asked to choose from a small set of possibilities.

2. There were six selected individuals whose performance far exceeded that of unselected subjects. The fact that these same selected individuals consistently performed better than others under a variety of protocols provides a type of replicability that helps substantiate the validity of the results. If methodological problems were responsible for the results, they should not have affected this group differently from others.
3. Mass-screening efforts found that about 1% of those who volunteered to be tested were consistently successful at remote viewing. This indicates that remote viewing is an ability that differs across individuals, much like athletic ability or musical talent. (Results of mass-screenings were not included in the formal analysis because the conditions were not well controlled, but the subsequent data from subjects found during mass screening were included.)
4. Neither practice nor a variety of training techniques consistently worked to improve remote-viewing ability. It appears that it is easier to find than to train good remote viewers.
5. It is not clear whether feedback (showing the subject the right answer) is necessary, but it does appear to provide a psychological boost that may increase performance.
6. Distance between the target and the subject does not seem to impact the quality of the remote viewing.
7. Electromagnetic shielding does not appear to inhibit performance.
8. There is compelling evidence that precognition, in which the target is selected after the subject has given the description, is also successful.
9. There is not evidence to support anomalous perturbation (psychokinesis)--that is, physical interaction with the environment by psychic means.

3.4 Consistency with Other Laboratories in the Same Era

One of the hallmarks of a real phenomenon is that its magnitude is replicable by various researchers working under similar conditions. The results of the overall SRI analysis are consistent with results of similar experiments in other laboratories. For instance, an overview of forced-choice precognition experiments (Honorton & Ferrari, 1989) found an average effect size per experimenter of 0.033, whereas all forced-choice experiments at SRI resulted in a similar effect size of 0.052. The comparison is not ideal because the SRI forced-choice experiments were not necessarily precognitive, and they used different types of target material than the standard card-guessing experiments.

Methodologically sound remote viewing has not been undertaken at other laboratories, but a similar regime called the ganzfeld (described in more detail in Section 5) has been similarly successful. The largest collection of ganzfeld experiments was conducted from 1983 to 1989 at the Psychophysical Research Laboratories in Princeton, NJ. Those experiments were also reported by separating novices from experienced subjects. The overall effect size for novice remote viewing at SRI was 0.164, and the effect size for novices in the ganzfeld at PRL was a very similar 0.17. For experienced remote viewers at SRI the overall effect size was 0.385; for experienced viewers in the ganzfeld experiments it was 0.35. These consistent results across laboratories help refute the idea that the successful experiments at any one lab are the result of fraud, sloppy protocols, or some methodological problem, and they also provide an indication of what can be expected in future experiments.

4. THE SAIC ERA

4.1 An Overview

The review team decided to focus more intensively on the experiments conducted at Science Applications International Corporation (SAIC), because they provide a manageable yet varied set to examine in detail. They were guided by a Scientific Oversight Committee consisting of experts in a variety of disciplines, including a winner of the Nobel Prize in Physics, internationally known professors of statistics, psychology, neuroscience and astronomy, and a medical doctor who is a retired U.S. Army Major General. Further, we have access to the details for the full set of SAIC experiments, but not for those conducted at SRI. Whatever detail may be missing from the written reports are obtainable from the principal investigator, Edwin May, to whom we have been given unlimited access.

In a memorandum dated July 25, 1995, May listed the set of experiments conducted by SAIC. There were 10 experiments, all designed to answer questions about psychic functioning raised by the work at SRI and other laboratories, rather than just to provide additional proof of its existence. Some of the experiments were of a format similar to the remote-viewing experiments conducted at SRI, and we can examine those to see whether they replicated the SRI results. We will also examine what new knowledge can be gained from the results of the SAIC work.

4.2 The 10 Experiments

Of the 10 experiments done at SAIC, six of them involved remote viewing and four did not. Rather than list the details in the body of this report, I give a brief description of the experiments in Appendix B. What follows is a discussion of the methodology and results for the experiments as a whole. Because of the fundamental differences between remote viewing and the other types of experiments, we discuss them separately.

In the memorandum of July 25, 1995, May provided the review team with details of the 10 experiments, including a short title, number of trials, effect size, and overall *p* value for each. His list was in chronological sequence. It is reproduced in Table 1, using his numbering system, with the experiments categorized by type and then sequentially within type. The effect size estimates are based on a limited number of trials, and thus they are augmented with an interval to show the probable range of the true effect (e.g., 0.124 ± 0.071 indicates a range from 0.053 to 0.195). Remember that an effect size of 0 represents chance, and a positive effect size indicates positive results.

4.3 Assessing the Remote-Viewing Experiments by Homogeneous Sets of Sessions

Although Table 1 provides an overall assessment of the results of each experiment, it does so at the expense of information about variability among viewers and types of targets. In terms of understanding the phenomenon, it is important to break the results down into units that are as homogeneous as possible in terms of procedure, individual viewer, and type of target. This is also important in order to assess the impact of any potential methodological problems. For example, in one pilot experiment (E6, AC in Lucid Dreams) viewers were permitted to take the targets home with them in sealed envelopes.

Table 2 presents the effect size results at the most homogeneous level possible based on the information provided. For descriptions of the experiments, refer to Appendix B. Overall effect sizes for each viewer and total effect sizes for each experiment are weighted according to the number of trials, so that each trial receives equal weight.

TABLE 1

SAIC EXPERIMENTS LISTED BY DR. EDWIN MAY

Experiment	Title	Trials	Effect Size	*p* value
		Remote-Viewing Experiments		
1	Target dependencies	200	0.124 ± 0.071	.040
4	AC with binary coding	40	-0.067 ± 0.158	.664
5	AC lucid dreams, base	24	0.088 ± 0.204	.333
6	AC lucid dreams, pilot	21	0.368 ± 0.218	.046
9	ERDa AC behavior	70	0.303 ± 0.120	.006
10	Entropy II	90	0.550 ± 0.105	9.1 x 10-8
		Other Experiments		
2	AC of binary targets	300	0.123 ± 0.058	.017
3	MEG replication	12.000sb	MCE	MCE
7	Remote observation	48	0.361 ± 0.144	.006
8	ERD EEG investigation	7,000s	MCE	MCE

aERD = event related desynchronizations.
bs = stimuli in physiological experiments.

4.4 Consistency and Replicability of the Remote-Viewing Results

One of the most important hallmarks of science is replicability. A phenomenon with statistical variability, whether it is scoring home runs in baseball, curing a disease with chemotherapy, or observing psychic functioning, should exhibit about the same level of success in the long run, over repeated experiments of a similar nature. The remote-viewing experiments are no exception. Remember that such events should not replicate with any degree of precision in the short run because of statistical variability, just as we would not expect to always get five heads and five tails if we flip a coin 10 times, or to see the same batting averages in every game.

TABLE 2
INDIVIDUAL EFFECT SIZES

Experiment	Expert remote viewers					Viewer unknown/ other	Total
	009	131	372	389	518		
			Static targets (photographs)				
E1: Static	0.424	-0.071	0.424	0.177	0.283	n.a.	0.247
E9	0.432	n.a.	0.354	0.177	n.a.	n.a.	0.303
E10: Static	0.566	n.a.	0.801	-0.071	0.778	n.a.	0.550
E5a	n.a.	n.a.	n.a.	n.a.	n.a.	0.088	0.088
E6b	n.a.	n.a.	n.a.	n.a.	n.a.	0.370	0.370
E4c	-0.112	n.a.	0	0.112	n.a.	-0.559	-0.067
			Dynamic targets (video film clips)				
E1: Dynamic	0	0.354	-0.283	0	-0.071	n.a.	0.000
E10: Dynamic	0.919	n.a.	0.754	0	0.424	n.a.	0.550
Overall	0.352	0.141	0.340	0.090	0.271	n.a.	

[a]Experiment 5 did not include any expert viewers.
[b]Experiment 6 included four expert viewers, but separate results were not provided.
[c]Experiment 4 used a specially designed target set and only four choices in judging.

The analysis of SRI experiments conducted in 1988 singled out the laboratory remote-viewing sessions performed by six "expert" remote viewers (numbers 002, 009, 131, 372, 414, and 504). These six individuals contributed 196 sessions. The resulting effect size was 0.385 (May et al., 1988, p. 13). The SRI analysis does not include information individually by viewer, nor does it include information about how many of the 196 sessions used static versus dynamic targets. One report provided to the review team (May, Lantz, & Piantineda, 1994) included an additional experiment conducted after the 1988 review was performed, in which viewer 009 participated with 40 sessions. The effect size for viewer 009 for those sessions was 0.363. None of the other five SRI experts were participants.

The same identifying numbers for subjects were used at SAIC, and so we can compare the performance for these individuals at SRI and SAIC. Of the six, three were specifically mentioned as participating in the SAIC remote-viewing experiments. As can be seen in Table 2, viewers 009, 131, and 372 all participated in Experiment 1, and viewers 009 and 372 participated in Experiments 4, 9, and 10 as well.

The overall effect sizes for two of the three viewers (009 and 372) were very close to the SRI effect size of 0.385 for these subjects, at 0.35 and 0.34, respectively, and the 0.35 effect size for viewer 009 was very similar to his 0.363 effect size in the report by May, Lantz, and Piantineda (1994). Therefore, we see a repeated and, more importantly, hopefully repeatable level of functioning above chance for these individuals. An effect of this size should be reliable enough to be sustained in any properly conducted experiment with enough trials to obtain the long run statistical replicability requiered to rule out chance.

It is also important to notice that viewers 009 and 372 did well on the same experiments and poorly on the same experiments. In fact, the correlation between their effect sizes across experiments is .901, which is very close to a perfect correlation of 1.0. This kind of consistency warrants investigation to determine whether it is the nature of the experiments, a statistical fluke, or some methodological problems that led these two individuals to perform so closely to one another. If methodological problems are responsible, then they must be subtle indeed because the methodology was similar for many of the experiments, yet the results were not. For instance, procedures for the sessions with static and dynamic targets in Experiment 1 were almost identical to each other, yet the dynamic targets did not produce evidence of psychic functioning (p = .50) and the static targets did (p = .0073). Therefore, a methodological problem would have had to affect results differentially for the two types of targets, even though the assignment of target type was random across sessions.

4.5 Methodological Issues in the Remote-Viewing Experiments at SAIC

As noted in Section 2.3, there are a number of methodological considerations needed to perform a careful remote-viewing experiment. Information necessary to determine how well each of these were addressed is generally available in the reports, but in some instances I consulted May for additional information. As an example of how the methodological issues in Section 2.3 were addressed, an explanation will be provided for Experiment 1.

In this experiment the viewers all worked from their homes (in New York, Kansas, California, and Virginia). Nevin Lantz, who resided in Pennsylvania, was the principal investigator. After each session, viewers faxed their response to Lantz and mailed the original to SAIC. Upon receipt of the fax, Lantz mailed the correct answer to the viewer. The viewers were supposed to mail their original responses to SAIC immediately, after faxing them to Lantz. According to May, the faxed versions were later compared with the originals to make sure the originals were sent without any changes. The other methodological issues in Section 2.3 were handled as follows:

1. *No one who has knowledge of the specific target should have any contact with the viewer until after the response has been safely secured.*

No one involved with the experiment had any contact with the viewers, since they were not in the vicinity of either SAIC or Lantz's home in Pennsylvania.

2. *No one who has knowledge of the specific target or even of whether or not the session was successful should have any contact with the judge until after that task has been completed.*

Lantz and the individual viewers were the only ones who knew the correct answers, but according to May, they did not have any contact with the judge during the period of this experiment.

3. *No one who has knowledge of the specific target should have access to the response until after the judging has been completed.*

Again, since only the viewers and Lantz knew the correct target, and since the responses were mailed to SAIC by the viewers before they received the answers, this condition appears to have been met.

4. *Targets and decoys used in judging should be selected using a well-tested randomization device.*

This has been standard practice at both SRI and SAIC.

5. *Duplicate sets of target photographs should be used, one during the experiment and one during the judging, so that no cues (such as fingerprints) can be inserted onto the target that would help the judge recognize it.*

This was done; Lantz maintained the set used during the experiment, and the set used for judging was kept at SAIC in California.

6. *The criterion for stopping an experiment should be defined in advance so that it is not called to a halt when the results just happen to be favorable. Generally, that means specifying the number of trials in advance, but some statistical procedures require other stopping rules. The important point is that the rule be defined in advance in such a way that there is no ambiguity about when to stop.*

In advance it was decided that each viewer would contribute 40 trials, 10 under each of four conditions (all combinations of sender/no sender and static/dynamic). All sessions were completed.

7. *Reasons, if any, for excluding data must be defined in advance and followed consistently, and the reasons should not be dependent on the data. For example, a rule specifying that a trial could be aborted if the viewer felt ill would be legitimate, but only if the trial was aborted before anyone involved in that decision knew the correct target.*

No such reasons were given, nor was there any mention of any sessions being aborted or discarded.

8. *Statistical analyses to be used muyst be planned before collecting the data so that a method most favorable to the data is not selected post hoc. If multiple methods of analysis are used, the corresponding conclusions must recognize that fact.*

The standard rank-order judging had been planned, with results reported separately for each of the four conditions in the experiment for each viewer. Thus 20 effect sizes were reported, four for each of the five viewers.

4.6 Was Anything Learned at SAIC?

4.6.1 Target selection. In addition to the question of whether psychic functioning is possible, the experiments at SAIC were designed to explore a number of hypotheses. Experiments 1 and 10 were

both designed to see whether there is a relationship between the "change in visual entropy" in the targets and the remote-viewing performance.

Each of the five sense with which we are familiar is a change detector. Our vision is most readily drawn to something that is moving, and in fact if our eyes are kept completely still, we cease to see at all. Similarly, we hear because of moving air, and our attention is drawn to sudden changes in sound levels. Other senses behave similarly. Thus, it is reasonable that if there really is a "psychic sense," then it would follow that same pattern.

Experiments 1 and 10 were designed to test whether remote-viewing performance would be related to a particular type of change in the target material, namely, the "change in visual entropy." A target with a high degree of change would be one in which the colors changed considerably throughout the target. A detailed explanation can be found in the SAIC reports of this experiment or in May, Spottiswoode, and James (1994). There was indeed a correlation between the change in entropy in the target and the remote-viewing quality. This result was initially shown in Experiment 1 and replicated in Experiment 10. A simulation study matching randomly chosen targets to responses showed that this was unlikely to be an artifact of target complexity or other features.

It is worth speculating on what this might mean for determining how psychic functioning works. Physicists are currently grappling with the concept of time, and precognition may in fact be consistent with current understanding. Perhaps it is the case that we do have a psychic sense, much like our other senses, and that it works by scanning the future for possibilities of major change much as our eyes scan the environment for visual change and our ears are responsive to auditory change. That idea is consistent with anecdotal reports of precognition, which are generally concerned with events involving major life change. Laboratory remote viewing may in part work by somone directing the viewer to focus on a particular point in the future, that in which he or she receives the feedback from the experiment. It may also be the case that this same sense can scan the environment in actual time and detect change as well.

Another hypothesis put forth as SAIC was that laboratory remote-viewing experiments are most likely to be successful if the pool of potential targets is neither too narrow nor too wide in terms of the number of possible elements in the target. They called this feature the "target-pool bandwidth" and described it as the number of "differential cognitive elements." They reasoned that if the possible target set was too small, the viewer would see the entire set and be unable to distinguish that information from the psychic information. If the set was too broad, the viewer would not have any means for editing an extensive imagination.

Combining these two results would indicate that a good target set would contain targets with high change in visual entropy, but that the set would contain a moderately sized set of possibilities. The set of 100 photographs used in the later days at SRI and at SAIC may have inadvertently displayed just those properties.

4.6.2 Remote staring. Experiment 7, described in Appendix B, provided results very different from the standard remote-viewing work. That experiment was designed to test claims, made in the

former Soviet Union and by some researchers in the United States, that individuals could influence the physiology of another individual from a remote location. The study was actually two separate replications of the same experiment, and both replications were successful from a traditional statistical perspective. In other words, it appeared that the physiology of one individual was activated when he or she was being watched by someone in a distant room. If these results are indeed sound, then they may substantiate the folklore indicating that people know when they are being observed from behind.

4.6.3 Enhanced binary computer guessing. Experiment 2 was also very different from the standard remote-viewing experiments, although it was still designed to test anomalous cognition. Three subjects attempted to use a statistical enhancement technique to increase the ability to guess forced-choice targets with two choices. This clever computer experiment showed that for one subject, guessing was indeed enhanced from a raw rate of just above chance (51.6% instead of 50%) to an enhanced rate of 76%. The method was extremely inefficient, and it is difficult to imagine practical uses for this ability, if indeed it exists.

5. EXTERNAL VALIDATION: REPLICATIONS OF OTHER EXPERIMENTS

5.1 Conceptual Similarity: Ganzfeld Experiments

While remote viewing has been the primary activity at SRI and SAIC, other researchers have used a similar technique to test for anomalous cognition, called the ganzfeld. As noted in the SAIC Final Report of 29 September 1994, the ganzfeld experiments differ from remote viewing in three fundamental ways. First, a "mild altered state is used"; second, senders are usually used, so that telepathy is the primary mode; and third, the *receivers* (viewers) do their own judging just after the session, rather than having an independent judge.

The ganzfeld experiments conducted at the Psychophysical Research Laboratories (PRL) were mentioned in Section 3.4. Since the time those results were reported, other laboratories have also been conducting ganzfeld experiments. At the 1995 annual Meeting of the Parapsychological Association, three replications were reported, all published in the peer-reviewed *Proceedings* of the conference.

The ganzfeld experiments differ in the preferred method of analysis as well. Rather than using the sum of the ranks across sessions, a simple count is made of how many first place matches resulted from a series. Four rather than five choices are given, and so by chance there should be about 25% of the sessions resulting in first place matches.

5.2 Ganzfeld Results from Four Laboratories

In publishing the ganzfeld results from the Psychophysical Research Laboratories, Bem and Honorton (1994) excluded one of the studies from the general analysis for methodological reasons, and found that the remaining studies showed 106 hits out of 329 sessions, for a hit rate of 32.2% when 25% was expected by chance. The corresponding p value was .002. As mentioned earlier, the hallmark of science is replication. This result has now been replicated by three additional laboratories.

Bierman (1995) reported four series of experiments conducted at the University of Amsterdam. Overall, there were 124 sessions and 46 hits, for a hit rate of 37%. The hit rates for the four individual experiments were 34.3%, 37.5%, 40%, and 36.1%, and so the results are consistent across his four experiments.

Morris, Dalton, Delanoy, and Watt (1995) reported results of 97 sessions conducted at the University of Edinburgh in which there were 32 successes, for a hit rate of 33%. They conducted approximately equal numbers of sessions under each of three conditions. In one condition there was a known sender, and in the other two conditions it was randomly determined at the last minute (and unknown to the receiver) that there would either be a sender or not. Hit rates were 34% when there was a known sender and when there was no sender, and 28% when there was a sender but the receiver did not know whether there would be. They did discover post hoc that one experimenter was more successful than the other two at achieving successful sessions, but the result was not beyond what would be expected by chance as a post hoc observation.

Broughton and Alexander (1995) reported results from 100 sessions at the Institute for Parapsychology in North Carolina. They too found a similar hit rate, with 33 hits out of 100 sessions, or 33%.

Results from the original ganzfeld work and these three replications are summarized in Table 3, along with the SRI and SAIC remote-viewing results. The effect sizes for the ganzfeld replications are based on Cohen's *h*, which is similar in type to the effect size used for the remote-viewing data. Both effect sizes measure the number of standard deviations by which the results fall above chance, using the standard deviation for a single session.

TABLE 3
REMOTE VIEWING AND GANZFELD REPLICATIONS

Laboratory	Sessions	Hit rate	Effect size
All remote viewing at SRI	770	n.a.	0.209
All remote viewing at SAIC	445	n.a.	0.230
PRL, Princeton, NJ	329	32%	0.167
University of Amsterdam, the Netherlands	124	37%	0.261
University of Edinburgh, Scotland	97	33%	0.177
Institute for Parapsychology, NC	100	33%	0.177

5.3 Conclusions about External Replication

The results shown in Table 3 show that remote viewing has been conceptually replicated across a number of laboratories, by various experimenters and in different cultures. This is a robust effect that,

were it not in such an unusual domain, would no longer be questioned by science as a real phenomenon. It is unlikely that methodological problems could account for the remarkable consistency of results shown in Table 3.

6. IS REMOTE VIEWING USEFUL?

Even if we were all to agree that anomalous cognition is possible, there remains the question of whether it would have any practical use for government purposes. The answer to that question is beyond the scope of this report, but some speculations can be made about how to increase the usefulness.

First, it appears that anomalous cognition is to some extent possible in the general population. None of the ganzfeld experiments used exclusively selected subjects. However, it also appears that certain individuals possess more talent than others and that it is easier to find those individuals than to train people. It also appears to be the case that certain individuals are better at some tasks than others. For instance, viewer 372 at SAIC appears to have a facility with describing technical sites.

Second, if remote viewing is to be useful, the end users must be trained in what it can do and what it cannot. At our current level of understanding, remote viewing is rarely 100% accurate, and there is no reliable way to learn what is accurate and what is not. The same is probably true of most sources of intelligence data.

Third, what is useful for one purpose may not be useful for another. For instance, suppose a remote viewer could describe the setting in which a hostage is being held. That information may not be any use at all to those unfamiliar with the territory, but could be useful to those familiar with it.

7. CONCLUSIONS AND RECOMMENDATIONS

It is clear to this author that anomalous cognition is possible and has been demonstrated. This conclusion is not based on belief, but rather on commonly accepted scientific criteria. The phenomenon has been replicated in a number of forms across laboratories and cultures. The various experiments in which it has been observed have been different enough that if some subtle methodological problems can explain the results, then there would have to be a different explanation for each type of experiment, yet the impact would have to be similar across experiments and laboratories. If fraud were responsible, similarly, it would require an equivalent amount of fraud on the part of a large number of experimenters or an even larger number of subjects.

What is not so clear is that we have progressed very far in understanding the mechanism for anomalous cognition. Senders do not appear to be necessary at all; and feedback of the correct answer may or may not be necessary. Distance in time and space do not seem to be an impediment. Beyond those conclusions, we know very little.

I believe that it would be wasteful of valuable resources to continue to look for proof. No one who has examined all of the data across laboratories, taken as a collective whole, has been able to suggest methodological or statistical problems to explain the ever-increasing and consistent results to date.

Resources should be directed to the pertinent questions about how this ability works. I am confident that the questions are no more elusive than any other questions in science dealing with small to medium sized effects, and that if appropriate resources are targeted to appropriate questions, we can have answers within the next decade.

APPENDIX A
EFFECT SIZE MEASURE USED WITH RANK-ORDER JUDGING

In general, effect sizes measure the number of standard deviations the true population value of interest falls from the value that would be true if chance alone were at work. The standard deviation used is for one subject, trial, and so forth, rather than being the standard error of the sample statistic used in the hypothesis test.

In rank-order judging, let *R* be the rank for one trial. If the number of possible choices is N, then we find:

$E(R) = (N + 1)/2$

and

$Var(R) = (N^2 - 1)/12.$

Therefore, when $N = 5$, we find $E(Ri) = 3$ and $Var(Ri) = 2$. The effect size is therefore:

$Effect\ Size = (3.0 - Average\ Rank)/\sqrt{2}.$

APPENDIX B
A BRIEF DESCRIPTION OF THE SAIC EXPERIMENTS

Experiments Involving Remote Viewing

There were six experiments involving remote viewing, done for a variety of purposes.

Experiment 1: Target and Sender Dependencies

Purpose. This experiment was designed to test whether a sender is necessary for successful remote viewing and whether dynamic targets, consisting of short video clips, would result in more successful remote viewing than the standard photographs used in most of the SRI experiments.

Method: Five experienced remote viewers participated, three of whom (numbers 009, 131, and 372) were included in the experienced group at SRI; their identification numbers were carried over to the SAIC experiments. Each viewer worked from his or her home and faxed the results of the sessions to the principal investigator, Nevin Lantz, located in Pennsylvania. Whether the target was static or dynamic and whether there was a sender was randomly determined and unknown to the viewer. Upon receiving the fax of the response, Lanz mailed the correct answer to the viewer. The original response was sent to SAIC in California, where the results were judged by an analyst blind to the correct target. Standard rank-order judging was used.

Since it is not explicitly stated, I asked May what measures were taken to make sure the viewer actually mailed the original response to SAIC *before* receiving the correct answer in the mail. He said that the original faxed responses were compared with the responses received by SAIC to make sure they were the same, and they all were.

Results. Each viewer contributed 10 trials under each of the four possible onditions (sender/no sender and static/dynamic target), for a total of 40 trials per viewer. There was a moderate difference (effect size = 0.121, p = .08) between the static and dynamic targets, with the traditional photographs faring better than the dynamic video clips. There was no noticeable difference based on whether a sender was involved, supporting the same onclusion reached in the overall analysis of the SRI work. Combined over all conditions and all viewers, the effect size was 0.124 (p = .04); for the static targets alone it was 0.248 (exact p = .0073), and for the dynamic targets it was 0.00 (p = .50).

Discussion: The SAIC staff speculated that the dynamic targets were not successful because the possibilities were too broad. They chose a new set of dynamic targets to be more similar to the static targets and performed another experiment the following year to compare the static targets with the more similar set of dynamic ones. That experiment is described below (Experiment 10).

Experiment 4: Enhancing Detection of AC with Binary Coding

Purpose: This experiment was designed to see whether remote viewing could be used to develop a message-sending capability by focusing on the presence or absence of five specific features of a target. The target set was constructed in packets of four, with possible combinations of the absence (0) or presence (1) of each of the five features chosen to correspond to the numbers 00000, 01110, 10101, and 11011. This is standard practice in information theory when trying to send a two-digit number (00, 01, 10, or 11); the remaining three bits are used for "error correction." Different sets of five features were used for each of 10 target packs.

Method: Five viewers each contributed eight trials, but the same eight targets were used for all five viewers. There was no sender, and viewers were told that each target would be in a fixed location for one week. They were to spend 15 minutes trying to draw the target, then fax their responses to SAIC in California. The results were blind-judged, and the binary features were coded both by the viewers and by an independent analyst.

Results: The results were unsuccessful in showing any evidence of psychic functioning. Neither standard rank-order judging nor analysis based on the binary guesses showed any promise that this method works to send messages.

Experiment 5: AC in Lucid Dreams (Baseline)

Purpose: Despite its name, this experiment did not involve lucid dreaming. Instead, it was used to test three novice remote viewers who were to participate in an experiment involving remote viewing while dreaming. This baseline experiment was designed to see whether these individuals would be successful at standard laboratory remote viewing.

Method: For this baseline experiment, each of the three viewers contributed eight trials using a standard protocol common in the SRI era. For each trial, a target was randomly chosen from the set of 100 photograph targets used at SRI and SAIC. The target was placed on a table (no sender was used) while the viewer, in another room, was asked to provide a description. The response was later blind-judged by comparing it to the target and four decoys and providing a rank ordering of the five choices.

Results: Of the three novice viewers, one obtained a promising effect size of 0.265, although the result was not statistically significant due to the small number of trials (eight). Individual results were not provided for the other two viewers, but the overall effect size was reported as 0.088 for the three viewers.

Experiment 6: AC in Lucid Dreams (Pilot)

Purpose: A lucid dream is a dream in which one becomes aware that he or she is dreaming and can control subsequent events in the dream. This ability has apparently been successfully trained by Stephen LaBerge of the Lucidity Institute. He was the principal investigator for this experiment. The experiment was designed to see whether remote viewing could be successfully employed while the viewer was having a lucid dream.

Method: Seven remote viewers were used; four were experienced SAIC remote viewers, and three were experienced lucid dreamers from the Lucidity Institute. The latter three were the novice viewers used in Experiment 5. The experienced SAIC remote viewers were given training in lucid dreaming. The number of trials contributed by each viewer could not be fixed in advance because of the difficulty of attaining the lucid dream state. A total of 21 trials were conducted, with the seven viewers contributing anywhere from one to seven trials each. The report did not mention whether the stopping criterion was fixed in advance, but according to Dr. May the experiment was designed to proceed for a fixed time period and to include all sessions attained during that time period.

In contrast to well-controlled protocols, the viewers were allowed to take the target material home with them. The targets, selected from the standard pool of photographs, were sealed in opaque envelopes with covert threads to detect possible tampering (there were no indications of such tampering). Viewers were instructed to place the targets at bedside and to attempt a lucid dream in which the envelope was opened and the target viewed. Drawings and descriptions were then to be produced upon awakening.

Results: The results were blind-judged using the standard sum of ranks. Since the majority of viewers contributed only one or two trials, analysis by individual viewer would be meaningless. For the 21 trials combined, the effect size was 0.368 (p = .046). Information was not provided to differentiate the novice remote viewers from the experienced ones.

Experiment 9: ERD (Event Related Desynchronization) AC Behavior

Purpose: The remote viewing in this experiment was conducted in conjunction with measurement of brain waves using an EEG. The purpose of the experiment was to see whether EEG activity would change when the target the person was attempting to describe was briefly displayed on a computer monitor in a distant room. Details of the EEG portion will be explained as Experiment 8. Here, we summarize the remote-viewing part of the study.

Method: Three experienced remote viewers (numbers 009, 372, and 389) participated. Because of the pilot nature of the experiment, the number of trials differed for each viewer based on availability, with viewers 009, 372, and 389 contributing 18, 24, and 28 trials, respectively. Although it is not good protocol to allow an unspecified number of trials, it does not appear that this problem can explain the results of this experiment.

Results. Responses were blind-judged using standard rank-order analysis. The effect sizes for viewers 009, 372, and 389 were 0.432 (p = .033), 0.354 (p = .042) and 0.177 (p = .175), respectively. The overall effect size was .0303 (p = .006).

Experiment 10: Entropy II

Purpose: This experiment was designed as an improved version of Experiment 1. After the unsuccessful showing for the dynamic targets in Experiment 1, the SAIC team speculated that the "target-pool band-width"—defined as the number of "cognitively differentiable elements" in the target pool—might be an important factor. If the possible target material was extremely broad, viewers might have trouble filtering out extraneous noise. If the set of possibilities was too small, as in forced-choice experiments, the viewer would see all choices at once and would have trouble filtering out that knowledge. An intermediate range of possibilities, too large to be considered all at once, was predicted to be ideal. The standard photograph pool seemed to fit that range. For this experiment, a pool of dynamic targets was created with a similar "band-width." In both experiments (1 and 10) the researchers predicted that remote-viewing success would correlate with the change in visual entropy of the target, as explained earlier.

TABLE 4
RESULTS OF EXPERIMENT 10

	Static Targets			Dynamic Targets		
Viewer	Rank	Effect size	p	Rank	Effect size	p
009	2.20	0.565	.037	1.70	0.919	1.8 x 10-3
372	1.87	0.801	9.7×10^{-4}	1.93	0.754	1.8 x 10-3
389	3.10	-0.071	.589	3.00	0.000	.500
518	1.90	0.778	7.2×10^{-3}	2.40	0.424	.091
Total	2.22	0.550	1.1×10^{-5}	2.22	0.550	1.1 x 10-5

Method: Four of the five viewers from Experiment 1 were used (numbers 009, 372, 389, and 518). They each contributed equal numbers of sessions with static and dynamic targets, with the viewers blind to which trials had which type. Senders were not used, and all sessions were conducted

at SAIC in California (unlike Experiment 1, in which the viewers worked at home). Viewer 372 contributed 15 of each type, and the others each contributed 10 of each type. Standard rank-order judging was used.

Results: Table 4 shows the results for this experiment. Unlike in Experiment 1, the static and dynamic targets produced identical effect sizes, with both types producing very successful results. The combined effect size for all trials is 0.55, resulting in a *z* score of 5.22.

The Other Experiments at SAIC

There were four additional experiments at SAIC, not involving remote viewing. Two of them (Experiments 3 and 8) involved trying to measure brain activity related to psychic functioning and will be described briefly. Experiment 3 used a magnetoencephalograph (MEG) to attempt to detect anomalous signals in the brain when a remote stimulus was present. Due to the background noise in the brain measurements and the expected strength of the signal, the experimenters realized too late that they would not be able to detect a signal even if it existed. Experiment 8 utilized an EEG to try to detect the interruption of alpha waves when a remote-viewing target was briefly displayed on a computer monitor in another room. The area of the brain tested was that corresponding to visual stimuli. No significant change in alpha was seen.

The remaining two experiments were replications of previous work measuring psychic functioning in areas other than remote viewing. They will be described in detail.

Experiment 2: AC of Binary Targets

Purpose: This experiment attempted to replicate and enhance random number generator experiments conducted at SRI. In these types of experiments a computer randomly selects one of two choices to be the target, denoted as 0 or 1. The internal workings of the computer then rapidly oscillate between 0 and 1, and the subject pushes a mouse button when he or she thinks the internal choice matches the target choice. This process is repeated over many trials. The computer tabulates the results, and the experiment is a success if the subject guesses the correct answer more often than would be expected by chance. The purpose is to see whether humans can correctly guess the computer-selected binary targets and, hopefully by extension, correctly solve binary choice problems in real situations. If that were to be the case, then real problems could be posed as binary ones (e.g., is the lost child still in this city or not?) to narrow down possibilities.

Method: This SAIC experiment was designed to enhance the accuracy of binary guessing by using a statistical technique called sequential analysis. Rather than just give one guess for each decision, the subject continues to guess until the computer ascertains that a decision has been reached. The computer keeps track of the number of times 0 and 1 have each been guessed and announces a decision when one of the choices has clearly won out over the other, or when it is clear that it is essentially an ongoing tie. In the latter case, no decision is recorded. Three subjects participated (numbers 007, 083, and 531) in this experiment. Subject 531 had been successful in similar experiments at SRI.

Results: Using this method for enhancing the accuracy of the guesses, subject 531, who had been successful in previous similar experiments, was able to achieve 76 correct answers out of 100 tries. This remarkable level of scoring for this type of experiment resulted in an effect size of 0.520 and a *z* score of 5.20. The other two subjects did not differ from chance results, with 44 and 49 correct decisions out of 100 or 101. (One subject accidentally contributed an additional trial.)

Although the result for subject 531 is remarkably successful, it does not represent a very efficient method of obtaining the decision. To reach the 100 decisions required a total of 21,337 guesses, that is, over 200 guesses for each decision. Of the individual guesses, only 51.6% were correct, for an effect size of 0.032, similar to other forced-choice experiments. Due to the large number of guesses, the corresponding *z* score was 4.65. Combined over all three subjects, 56% of the 301 decisions were correct, and the effect size was 0.123. The combined results were still statistically significant, with $p = .017$, as shown in Table 1.

Experiment 7: Remote Observation

Purpose: It is often reported anecdotally that people know when they are being watched. Two experiments were conducted at SAIC to determine whether these anecdotes could be supported by a change in physiology when someone is being observed from a distance. The experimental design was essentially the same for the two experiments. This work was a conceptual replication of results reported by researchers in the former Soviet Union, the United States, and Scotland. The experiments in the former Soviet Union were interpreted to mean that the physiology of the recipient was being manipulated by the sender, an effect that if real could have frightening consequences.

Method: The "observee" was seated in a room with a video camera focused on him or her and with galvanic skin response measurements being recorded. In a distant room the "observer" attempted to influence the physiology of the observee at randomly spaced time intervals. During those time intervals, an image of the observee appeared on a computer monitor watched by the observer. During "control" periods, the video camera remained focused on the observee, but the computer monitor did not display his or her image to the observer. There were 16 "influence" periods randomly interspersed with 16 "control" periods, each of 30 seconds, with blank periods of 0 to 5 seconds inserted to rule out patterns in physiology.

Result: To determine whether the galvanic skin response of the observees was activated while they were being watched, the response during the control periods was compared with the response during the "influence" periods for each subject. The results were then averaged across subjects. In both experiments, there was greater activity during the periods of being watched than there was during the control periods. The results were statistically significant in each case ($p = .036$ and .014), and the effect sizes were similar, at 0.39 and 0.49. As preplanned, the results were combined, yielding an effect size of 0.39 ($p = .005$). As an interesting post hoc observation, it was noted that the effect was substantially stronger when the observer and observee were of opposite sexes than when they were of the same sex.

Discussion: This experiment differs from the others conducted at SAIC since it involves interaction between two people rather than one person ascertaining information about the environment or the

future. It raises substantially more questions than it answers, since the mechanism for the shift in physiology is unknown. Possibilities range from the idea that the observee was able to know when the computer in the distant room was displaying his or her image (not unlike remote viewing) to the possibility that the observer actually did influence the physiology of the observee. Further eperimentation as well as a review of similar past experiments may be able to shed light on this important question.

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Une Évaluation des Preuves du Fonctionnement Psi

[Dans ce numéro du journal, nous sommes heureux de publier des évaluations des recherches sur le fonctionnement psychique sponsorisées par le gouvernement, en particulier la vision à distance, conduite au SRI International and Science Applications International Corporation (SAIC) au cours des trois dernières décennies. Ces évaluations ont été diligentées par l'American Institutes for Research (AIR) à la requête du Congrès et de la Central Intelligence Agency. Le but était d'évaluer la validité du fonctionnement psychique et ses potentielles applications afin de déterminer si la recherche pourrait continuer à recevoir des financements gouvernementaux. L'AIR, à son tour, a formé un panel consistant d'abord du Dr. Jessica Utts (un chercheur de pointe dans la recherche psi) et du Dr. Ray Hyman (un critique de pointe sur la parapsychologie) pour réaliser ces évaluations. Ce sont leurs rapports, en plus d'une brève réponse d'Utts à Hyman, que nous publions ici. Le lecteur devra garder à l'esprit que ces évaluations n'étaient pas conçues pour rendre compte de l'ensemble des recherches psi, et les auteurs étaient limités par les instructions reçues de la part de l'AIR. Nous publions ces évaluations telles que soumises à l'AIR, avec un minimum de travail éditorial. —Ed.]

RESUME : Une recherche sur le fonctionnement psi, conduite sur une période de deux décennies, est examinée pour déterminer si le phénomène a été scientifiquement établi. Une question secondaire est celle de son utilité pour des objectifs gouvernementaux. Le premier travail examiné dans ce rapport porte sur la recherche sponsorisée par le gouvernement conduite au Stanford Research Institute (dit ensuite SRI Interinational) et au Science Applications International Corporation (SAIC).
En utilisant les standards appliqués dans d'autres zones de la science, nous concluons que le fonctionnement psi a bien été établi. Les résultats statistiques des études examinées sont bien au-delà de ce que nous pourrions attendre du hasard seul. Les arguments selon lesquels ces résultats pourraient être dus à des biais méthodologiques dans les expérimentations sont clairement réfutés. Des effets sur la magnitude similaires à ceux trouvés dans les recherches sponsorisées par le gouvernement au SRI et au SAIC ont été répliqués dans plusieurs laboratoires autour du monde. Une telle consistance ne peut pas être expliquée simplement par des affirmations non prouvées de biais ou de fraude.
La magnitude du fonctionnement psi manifestée apparaît dans la portion que les chercheurs en sciences sociales appellent un effet faible ou médium. Il est donc suffisamment fiable pour être répliqué dans des expérimentations correctement conduites, avec suffisamment d'essais pour obtenir les résultats statistiques accumulés nécessaires pour la réplication.
Nombre d'autres patterns ont été trouvés, suggéreant des façons de conduire des expérimentations plus productives et de produire des applications du fonctionnement psi. Par exemple, il ne semble pas nécessaire d'inclure un émetteur. La précognition, dans laquelle l'information pertinente n'est connue par personne avant une période future, semble très bien fonctionner. Des expérimentations récentes suggèrent que, s'il y a un sens psi, il fonctionne plutôt comme nos cinq autres sens, en détectant des changements. Les physiciens sont actuellement en train de s'échiner à comprendre le temps, et il se pourrait bien qu'existe un sens psi capable de scanner le futur pour prévenir des changements majeurs, à la manière de nos yeux scannant l'environnement pour détecter des changements visuels ou de nos oreilles pour réagir des changements soudains de son.
L'auteur recommande que des expérimentations futures se focalisent sur la compréhension du fonctionnement psi et la façon de le rendre aussi utile que possible. Il y a peu de bénéfices à poursuivre des expérimentations orientées vers la preuve, puisqu'il sera difficile d'offrir mieux à ceux qui n'acceptent pas déjà les données actuellement collectées.

Eine Bewertung des Beweismaterials Für Parapsychisches Funktionieren

[Wir freuen uns, in dieser Ausgabe des *Journal* Auswertungen der von der Regierung geförderten Forschungen zu parapsychischen Funktionsweisen, überwiegend Remote Viewing, zu veröffentlichen, die in den letzten drei Jahrzehnten von der SRI International and Science Applications International Corporation (SAIC) durchgeführt worden waren. Diese Auswertungen wurden von den American Institutes for Research (AIR) auf Wunsch des Kongresses und der Central Intelligence Agency in Auftrag gegeben. Ziel war es, die Validität der parapsychischen Funktionsweisen und ihrer möglichen Anwendungen zu bewerten, um festzustellen, ob die Forschung weiterhin staatliche Mittel erhalten sollte. Die AIR wiederum bildete ein Panel, das hauptsächlich aus Dr. Jessica Utts (einer führenden Vertreterin der Psi-Forschung) und Dr. Ray Hyman (einem führenden Kritiker der Parapsychologie) bestand, um diese Bewertungen durchzuführen. Ihre Berichte, zusammen mit einer kurzen Antwort von Utts an Hyman,

veröffentlichen wir hier. Der Leser sollte sich darüber im Klaren sein, dass diese Bewertungen nicht als umfassende Übersichtsarbeiten zur Psi-Forschung gedacht und die Autoren durch die Vorgaben von AIR eingeschränkt waren. Wir veröffentlichen die Bewertungen, wie sie bei AIR eingereicht wurden, mit minimaler Bearbeitung.- Anm. d. Red.]

ZUSAMMENFASSUNG: Die Forschung über parapsychische Funktionsweisen, die über einen Zeitraum von zwei Jahrzehnten durchgeführt wurde, wird bewertet, um festzustellen, ob das Phänomen wissenschaftlich belegt ist. Eine zweite Frage betrifft die Nützlichkeit für staatliche Zwecke. Im Mittelpunkt dieses Berichts steht die von der Regierung geförderte Forschungsarbeit am Stanford Research Institute (später bekannt als SRI International) und an der Science Applications International Corporation (SAIC). Unter Verwendung der für alle anderen Wissenschaftsbereiche geltenden Maßstäbe wird der Schluss gezogen, dass parapsychische Funktionsweisen gut etabliert sind. Die statistischen Ergebnisse der untersuchten Studien liegen weit über dem, was durch Zufall erwartet wird. Argumente, dass diese Ergebnisse auf methodische Fehler in den Experimenten zurückzuführen sein könnten, werden fundiert widerlegt. Effekte von ähnlicher Größenordnung wie bei der von der Regierung geförderten Forschung bei SRI und SAIC wurden in einer Reihe von Laboratorien auf der ganzen Welt reproduziert. Diese Konsistenz lässt sich nicht ohne weiteres aufgrund von Fehlern oder Betrug erklären.

Die Größenordnung der hier gezeigten parapsychischen Funktionsweisens scheint im Bereich dessen zu liegen, was in den Sozialwissenschaften als schwacher und mittlerer Effekt bezeichnet wird. Diese ist somit zuverlässig genug, um in ordnungsgemäß durchgeführten Experimenten repliziert zu werden, mit einer ausreichenden Anzahl von Versuchen, um die für die Replizierbarkeit erforderlichen langfristigen statistischen Ergebnisse zu erzielen.

Eine Reihe weiterer Muster wurden gefunden, die darauf hinweisen, wie man produktivere Experimente durchführen und anwendbare parapsychische Funktionsweisen erzeugen kann. So scheint beispielsweise kein Sender benötigt zu werden. Präkognition, bei der relevante Information jemandem erst in der Zukunft bekannt wird, scheint recht gut zu funktionieren. Neuere Experimente deuten darauf hin, dass ein etwaiger parapsychischer Sinn wie unsere anderen fünf Sinne funktioniert, indem er auf Veränderungen reagiert. Physiker bemühen sich derzeit um ein Verständnis von Zeit, und es kann sein, dass ein parapsychischer Sinn die Zukunft nach bedeutsamen Veränderungen durchsucht, so wie unsere Augen die Umgebung nach visuellen Veränderungen absuchen oder unsere Ohren es uns erlauben, auf plötzliche Klangveränderungen zu reagieren.

Der Autor empfiehlt, dass sich zukünftige Experimente darauf konzentrieren sollten, zu verstehen, wie parapsychische Funktionsweisen zustande kommen und wie man diese so nützlich wie möglich anwenden kann. Beweisorientierte Experimente lediglich fortzusetzen, bringt kaum noch einen Nutzen, da demjenigen, der den vorliegenden Bestand an Daten nicht akzeptiert, kaum mehr geboten werden kann.

Una Evaluación de la Evidencia de Funcionamiento Psíquico

[En este número de la revista, nos complace publicar evaluaciones de investigaciones patrocinadas por el gobierno sobre el funcionamiento psíquico, en su mayoría de visión remota (remote viewing), realizadas en SRI International y Science Applications International Corporation (SAIC) durante las úl-

timas tres décadas. Estas evaluaciones fueron encargadas por el American Institutes for Research (AIR) a solicitud del Congreso y la Agencia Central de Inteligencia Norteamericanos. El propósito fue evaluar la validez del funcionamiento psíquico y sus posibles aplicaciones como un medio para determinar si la investigación debe recibir financiamiento gubernamental continuo. El AIR, a su vez, formó un panel compuesto principalmente por la Dra. Jessica Utts (una de las principales defensoras de la investigación psi) y el Dr. Ray Hyman (un destacado crítico de la parapsicología) para llevar a cabo las evaluaciones... El lector debe tener en cuenta que estas evaluaciones no pretendían ser revisiones exhaustivas de la investigación psi y que los autores estaban limitados por las instrucciones que recibieron de AIR. Publicamos las evaluaciones tal como se enviaron a AIR, con una edición mínima. -Nota del editor]

RESUMEN: Se examina la investigación sobre el funcionamiento psíquico, realizada durante un período de dos décadas, para determinar si el fenómeno se ha establecido científicamente. Una pregunta secundaria es si es útil para propósitos gubernamentales. El trabajo principal examinado en este informe fue una investigación patrocinada por el gobierno realizada en el Stanford Research Institute (más tarde conocido como SRI International) y la Science Applications International Corporation (SAIC).

Usando los estándares aplicados a cualquier otra área de la ciencia, se concluye que el funcionamiento psíquico ha sido bien establecido. Los resultados estadísticos de los estudios examinados van mucho más allá de lo que se esperaría por casualidad. Los argumentos de que estos resultados podrían deberse a fallas metodológicas en los experimentos son ampliamente refutados. Los efectos de una magnitud similar a los encontrados en la investigación patrocinada por el gobierno en SRI y SAIC se han replicado en varios laboratorios de todo el mundo. Dicha consistencia no puede explicarse fácilmente como fallas o fraude.

La magnitud del funcionamiento psíquico exhibido parece estar en el rango de lo que los científicos sociales llaman efectos pequeño a mediano. Por lo tanto, es lo suficientemente confiable como para ser replicado en experimentos conducidos adecuadamente, con datos suficientes para lograr los resultados estadísticos a largo plazo necesarios para la replicabilidad.

Se han encontrado varios otros patrones que sugieren cómo realizar experimentos más productivos y producir un funcionamiento psíquico aplicado. Por ejemplo, no parece que se necesite alguien que envíe la información. La precognición, en la cual nadie conoce la información relevante hasta un tiempo futuro, parece funcionar bastante bien. Experimentos recientes sugieren que, si existe un sentido psíquico, funciona de manera similar a como lo hacen nuestros otros cinco sentidos, al detectar cambios. Los físicos actualmente están tratando de entender al tiempo, y puede ser que un sentido psíquico explore el futuro en busca de un cambio importante, así como nuestros ojos escanean el entorno en busca de cambios visuales o nuestros oídos nos permiten responder a cambios repentinos en el sonido.

El autor recomienda que los experimentos futuros se centren en comprender cómo trabaja el funcionamiento psíquico y en cómo hacerlo lo más útil posible. Hay pocos beneficios en los experimentos continuos diseñados para obtener confirmación, ya que hay poco más que ofrecer a quien no quiera aceptar los datos ya recopilados.

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Mind Matters: A New Scientific Era [1, 2]

Roger D. Nelson

Global Consciousness Project

I am an optimist, the sort we call incorrigible. So for those who know me, there should be no surprise at my title and theme. There is no question in my mind that what we do in our sometimes beleaguered bailiwick is research that provides insight into what we humans are. It is a difficult domain, so we don't have complete answers, but because of the difficulty, we are led to generate really good questions. They touch on the edges and frontiers of science (and we accept with good humor that some of our friends in academia prefer the term "fringe").

What is important is that as these challenging questions are answered, even tentatively, they will expand and extend scientific understanding far beyond the effect of mere filling in the blanks and refining details of the current picture. There are big open spaces in standard models, which are amazingly difficult for ordinary scientists to see. Where is consciousness and the mental realm we all experience? Mind has, surprisingly, been an unwanted stepchild in science over extended periods and has never been accorded a proper place in psychology or any other of the sciences, even the queen of sciences, physics. Why not? I think it is, simply put, too complicated, too difficult.

We have not known how to place consciousness or mind in a sound theoretical model and so have tended to ignore the very experiences that allow us to do physics and explore psychology.

Obviously the construction of a model of the world helps us to think about it more cogently, but somehow the construction and the thinking escape us—they are too basic, perhaps, too much like the water the fish never sees. But there is the experience, and when we focus on it and try to stay with it, we generally agree that there is a problem, perhaps even a hard problem, as David Chalmers says (1995).

During the Winchester convention, we had an opportunity to listen to Max Velmans, who also sees how much we are missing. He is one of the few adventurous souls in mainstream psychology who wants to build bridges and foundations, and eventually to create an architecture for mind, linking philosophy and psychology and physics, and, yes, parapsychology in some measure (Velmans, 2000). And we will hear committed people who are career parapsychologists discuss their contributions to an ever growing and increasingly sophisticated corpus of excellent research on the subtle but important extended qualities of human consciousness. Perhaps it is not after all too optimistic to expect a new era in science,

1 [Originally published in *JP, 2008, 72,* 3-17.]

2 This article is a revised version of the presidential address for the 51st Annual Convention of the Parapsychological Association, held at Winchester, England, August 13–17, 2008.

where the experiential and subjective, mind and consciousness take important roles in models that cover the territory with more generosity and accuracy.

MAINSTREAM, WHY NOT?

We find little recognition in the scientific mainstream for what we know internally is excellent work, high quality evidence that "there is some there there." Part of the reason is that we really are addressing hard problems, with much of their difficulty stemming from a deep subtlety. In parapsychology we are looking at the edges of phenomena in an ill-defined consciousness space, and thus at problems within problems—there is no theory of consciousness, much less one that can explain the extension of consciousness into realms that, while part of human experience, are so subtle that we are seldom quite sure what has happened. But for more than a century, competent people have been piling up evidence that is persuasive to any who will look at it with care and a serious, unbiased interest. The evidence from experimental parapsychology is convergent and consistent. It shows small effects and patterns that begin to look like pieces in a puzzle. When we take an overview, a general understanding looks tantalizingly near. In addition, we have some good though still metaphoric models. We can and should feel encouraged.

Our science asks for a leap from the classical thinking that still pervades the views of most scientists even in a quantum era, but we aren't alone in that. The July 2008 edition of *Scientific American* has an article on "metabolomics," something most of us have never encountered. It is about the work of Jeremy Nicholson, at the Imperial College London, studying the 1,000 varieties of intestinal flora. He suggests a future of preventative therapies for serious diseases with drugs targeting the bacteria that define our unique metabolic profile. He says, "Many microbiologists might argue this is fanciful, but you only make huge progress in science by thinking almost the unthinkable" (Wenner, 2008).

More to the point in which we are most interested, there is increasing likelihood that psi research, our scientific examination of "the unthinkable," will add value to both physics and biology. Here is an observation by Dean Radin, discussing new mainstream findings about room-temperature entanglement:

> Does entanglement exist in biological systems? Of course it does. Quantum connections never go away. They just become more difficult to detect when dealing with squishy, hot environments. So the better question is [how we can] detect it. There the answer is still very speculative, but I suspect we're doing it all the time, and at the psychological level we call the effect of biological entanglements (an emergent property of more elementary forms of entanglement) psi.

This quote is from personal communication, but Dean's book *Entangled Minds* gives background and deep discussion in the context of parapsychological findings (Radin, 2006).

There is an outside chance that regular academic biologists and physicists will catch up with parapsychologists in the near future. Otherwise it looks very likely that the folks who have chosen psi research as their primary focus despite (or indeed because of) the challenges will write the opening chapters in a more comprehensive scientific view. Already there is a significant shift in at least one important field, medicine, to integrate and apply alternative perspectives. Good research that takes a page from parapsychology is

being done on distant healing and other therapeutic modes that were until recently considered fantasy by academic medicine. These practices actually are ancient, yet they were almost entirely excluded from western, mainstream science in its early phases. But no longer. A few courageous and persistent individuals continued to work the problems of scientific research encompassing a broader perspective in pragmatic, outcome-oriented research showing that prayer and meditation and spiritual practices do contribute to maintaining good health and to healing when it is needed (Astin, 2000). Acupuncture effects and their travel through a real meridian system (which still is disputed by classic medicine) can actually be seen using sophisticated ultrasound measures and functional magnetic resonance imaging (Jones & Bae, 2004). There are many examples of this kind, and evidently more to come.

In consciousness research the situation is similar but without the obvious pragmatics. Again there is ancient wisdom and cross-cultural information describing both the interconnection of mind and matter and the possibilities for anomalous information transfer that we study. I think we are beginning to see the shape of the new perspectives and insights that are needed to integrate physics and biology and psychology to finally include consciousness in models of the world. Mind matters, and we who are engaged in parapsychology know that. It remains only to develop a persuasive combination of clean, clear research data and sound explanatory models.

PARTICIPATION, A SINE QUA NON

Research with all the pieces in hand remains the best evidence (for the active researcher) because it combines as closely as possible the experiential and the scientific. I will give a few examples from my work at the Princeton Engineering Anomalies Research laboratory (PEAR), where for 22 years I participated in forming effective research questions, designing the protocols and the statistical analyses, collecting the data (including data generated as a participant myself), analyzing, and ultimately interpreting the results, all in cooperation with two or three or more other bright and interested people. We all knew the importance of getting it right. We had an extraordinary opportunity to do challenging research in a setting where we could invest the time necessary to do it well and to repeat experiments with variations that could inform and improve the work. We could and did look for what matters to consciousness interacting with physical systems, and we were able to learn important constraints in our efforts to capture anomalous information transfer. At PEAR we had the luxury of expertise and resources, and we used them well because we all knew it was a precious opportunity to learn something. We did not want to waste our time or that of anybody else who might look at our work. Here are some examples of what we learned.

REMOTE PERCEPTION

One focus at the PEAR lab was a long series of experiments looking at anomalous information transfer that we called “remote perception” or “precognitive remote perception” (PRP). The paradigm is similar to the free response remote viewing work developed by Targ and Puthoff at SRI in the 1970’s (Targ & Puthoff, 1977), and related as well to the ganzfeld work developed to a high point by Honorton (Bem & Honorton, 1994; Honorton, 1990). At PEAR we focused on quantitative assessment, using a set of 30 binary questions to represent the free response—is the scene hectic or calm; is it characterized more by straight lines or curves; are there people or not? Using the resulting performance measures, we

sought to determine what the constraints and necessary conditions were for successful remote perception. We asked whether the effect was diminished by greater distances between the agent at the scene and the percipient, and whether the scores were different for perception attempted before the target was visited or after the visit, compared with on-time viewing. We studied whether people were more successful when the target was determined by volitional selection at the appointed time by the agent at the scene or by random selection from a pool. We also explored variations of the quantification process, gradually increasing the number of descriptors in the questionnaire from two, to four, to a quasi-continuous scale with nine options. I can give here only a very brief overview of the results of this program over about two decades of work, but it is examined in detail in a recent PEAR paper (Dunne & Jahn, 2003).

The most important outcome was a confirmation of the primary hypothesis: Percipients can acquire information about distant targets without normal sensory channels. The effect is subtle, but over hundreds of trials, the odds against chance explanations go to millions or hundreds of millions to one. The scores for precognitive and retrocognitive trials are similar to those for concurrent trials, with no evidence for regression over a range of several days. And distance also seems not to matter; the perception of targets at international distances is indistinguishable from relatively local targets. But some variations in the experimental conditions do have a clear effect. As we developed more refined scoring procedures, the ability to capture information about the distant targets seemed to decrease. The effort to provide more nuance and flexibility to our participants turned out to be not a boon but something of a boondoggle. Brenda Dunne, who led the PRP program, had misgivings about the quantification from the beginning because it shifted focus from experience to assessment. But it was a necessary experimental investigation which produced an answer that is important. The ability to "far see" is fragile, and its requirements must be respected. It cannot be forced into an arbitrary mold for the sake of the scientific question. Instead, we must shape our scientific approach to study anomalous perception without sacrificing the free movement of the mind that enables it.

This is a critical point for research on psi and consciousness in general, one that we should understand well enough to make it clear to outside observers, both proponents and skeptics. The core understanding is that we must respect the unique character of what we observe. The answers we obtain are in part determined by the questions we ask (a photon will be seen as a particle or a wave depending on the way we observe it). We cannot squeeze or stretch a subtle talent or an ephemeral phenomenon into any arbitrary form but must accommodate its native dimensions.

MIND-MACHINE INTERACTION

The second major experimental program at PEAR was mind-machine interactions, or MMI. We began with random event generator (REG) experiments asking participants, whom we called operators, to change the random output to higher or lower numbers, compared with baselines (Jahn, Dunne, Nelson, Dobyns, & Bradish, 1997). We had an engineering mission, which was to find out whether human consciousness in special states might affect sensitive electronic equipment. Given that context, it will be no surprise that we were dedicated to precision and accuracy, and to a thorough and wide-ranging assessment. Ultimately, we created several unique experiments addressing similar questions using electronic, mechanical, hydrodynamic, and thermodynamic systems. Some of these were so beautiful as to deserve

a place in a fine gallery or museum, but this aspect was intended to help create conditions conducive to the "impossible" tasks we set for our operators. Again, we were attempting to provide space and opportunity for creative consciousness, and support for the subtle requirements of interactions between intention and effects in the world.

All the experiments were technically sophisticated and aesthetically elegant in their design and implementation. We made a pendulum with a crystal bob on a rod of fused silica, enclosed in clear acrylic. Operators tried to change the damping rate while measurements were taken with a razor edge cutting a light beam with timing by a 50-ns clock. We made a delightful small fountain and monitored its transition from laminar to turbulent flow with photodiode arrays to see whether intention could augment or hinder the descent from order into chaos. And we built a random mechanical cascade of 9,000 plastic balls bouncing through an array of pegs into collecting bins, forming a distribution that we tried to shift to the left or right by sheer will or intention. This was a complex mechanical instrument 3 m tall, and it earned the ironic name "Murphy" after the famous law, but it served well to help us determine whether psi could change behavior on an arguably macroscopic scale. And there were more such explorations: a dual thermistor experiment asking for focused temperature changes, an interferometer displaying a shifting pattern of concentric interference fringes, a Crookes tube with a series of evanescent spheres formed by luminescent gas discharge, fluctuating iridescent patterns in a birefringent plastic lever arm. Suffice it to say that we covered a lot of ground in nearly three decades of the PEAR lab.

MMI FINDINGS

A short list of major findings in the PEAR mind-matter interaction program includes many confirmations or replications of others' work. Indeed, the PEAR REG experiments were an extension of the work of Helmut Schmidt in particular (Schmidt, 1973) to provide a completely independent assessment using the best available technology and designs. The research continued for almost three decades, so there is much informative detail. The following summary points give some notion of the span and depth of the research findings:

1. There is an effect of conscious intention on the output or behavior of random systems.
2. The anomalous effect is very small but statistically significant over many replications.
3. Depending on conditions, effect size is approximately equivalent to parts per thousand.
4. Both high and low intentions yield correlated departures from expectation.
5. Baseline trials may show reduced variance, suggesting effects of a "baseline" intention.
6. Trials conducted with the operator in local and remote locations have similar effect sizes.
7. Trials conducted with the intentional effort prior to the data collection are also successful.
8. Experiments with two operators who are a bonded pair have significantly larger effects.
9. Serial position analysis shows early trials have a large effect that decreases but recovers.
10. Anomalous effects differ in magnitude and style for individual operators.
11. About 15% of unselected operators can achieve significant overall performance.
12. Effect size and style (symmetry of intentions) transfer from REG to other experiments.
13. Experiments with a wide variety of random sources show similar effect sizes.

14. Effects appear to depend on time invested in intentional effort and may be teleological.
15. Anomalous effects depend primarily on psychological factors, not physical parameters.

GROUP CONSCIOUSNESS

In the early 1990's, as miniaturization of electronics allowed construction of small but competent physical random number sources, we developed protocols for collecting data in the field. The question was whether REGs might be affected by mere attention rather than intention, and, more generally, whether special states of consciousness might have a kind of "field" effect. A variant of the REG program was created to take data continuously and to allow marking of the beginning and end of time periods of interest. For example, we took the REG, connected to a laptop or palmtop computer, to concerts, rituals, religious ceremonies, sporting events, board meetings, and various other events that might create a state of "group consciousness." The protocol was simple: Moments or periods that we judged likely to produce coherent or resonant thoughts and emotions among the people attending the event were marked, and the data were later extracted for analysis. The prediction was for a variance increase (since there was no directional intention, either high or low deviations from expectation would indicate an anomalous effect). We looked at many kinds of events that we expected would produce group coherence, and for a control condition, we collected data in mundane contexts such as shopping centers, busy street corners, academic meetings, and so on.

These experiments were termed FieldREG studies, and over several years we accumulated more than 100 datasets from "resonant" situations and a smaller but substantial number from "mundane" locations (Nelson et al., 1996; 1998b). A number of special series were undertaken, including data collection at operas, cathedrals, and sacred sites such as temples and tombs in Egypt. In a nutshell, these experiments showed that the REG data tended to depart from expectation in those situations that were conducive to a melding of individuals into a group consciousness. We found a few categories that were especially powerful, or rather, reliable—in the FieldREG work, as in the laboratory experiments, effect sizes tend to be small, so that repetitions of essentially similar conditions are necessary to accumulate statistical significance. On the other hand, using a time-normalized yield measure (Nelson, 2006), these natural, real world situations have a somewhat larger effect size than that found in laboratory experiments. The largest or most reliable effects seem to involve ritual or some other influence that is designed to bring people to a shared state of mind. On consideration, this seems reasonable, though we had to learn by trial and error what the most conducive situations might be. We also found that the combination of collective activity with a special place could be counted on to produce structure in the random data sequence. For example, the Egypt series comprised a traveling group of people interested in ancient Egyptian spiritual practices, who intended to chant or meditate in sacred sites. That is, there was a preplanned set of resonance-producing activities in the appropriate contexts, intended as a respectful attempt to connect to the spirit of the sacred places we visited. This series is the most consistent and hence statistically robust subset of the entire FieldREG database (Nelson, 1997, Nelson, et al., 1998b).

FIELDREG FINDINGS

What did we learn from several years and over 100 formal assessments of the FieldREG question? In the PEAR database, it is possible to make a strict meta-analytic combination across data subsets, and from that to draw robust conclusions. These are supported also by independent work (Radin, et al., 1996; Bierman, 1996). In all such research, it is necessary to use operational definitions, namely, a description of what is done to create or identify the item of interest, the group consciousness. In the experiment, we ask whether there are variance changes in data collected during these times. Given that background, a short list of findings includes:

1. Changes in REG behavior correlate with special states of group consciousness.
2. Situations conducive to resonant interaction produce increased data variance.
3. Practices designed to create group unity and coherence yield larger deviations.
4. Some venues may reliably yield decreased variance, but more study is needed.
5. Mundane or chaotic situations yield only normal random data sequences.
6. We infer that group consciousness can exist and can have anomalous effects.
7. The studies tentatively suggest an information field or "consciousness field" effect.
8. The nature of the questions we ask partially determines the experimental result.
9. The potential range of FieldREG applications is broad and invites further study.

Among the several replications of FieldREG work were some that looked at events in distant locations, and some that used multiple REGs. Notable among these were Dean Radin's examination of data from 5 devices in separated locations taken during the reading of the verdict in the O. J. Simpson trial (Radin, 1997), and Roger Nelson's collection of data from 12 REGs in Europe and the US during Princess Diana's funeral (Nelson et al., 1998a). Both of these events engaged the attention of millions of people, and both showed statistically significant departures from expectation at the most critical or poignant times. These and similar probes suggested it would be valuable to have a continuous record of REG data that could monitor the world stage for indications that special states of "global consciousness" might affect our instruments in a way similar to the effects of group consciousness.

GLOBAL CONSCIOUSNESS PROJECT

We began planning and building a world-spanning network of physical REG/RNG devices in late 1997. The architecture of the network was designed to use the Internet (which was coming to maturity at that time as a world-wide web) to transmit data from remote nodes to a central server for archiving. Here is a brief description of the technology: Custom software on continuously running computers at each node collects one trial (comprising the sum of 200 bits) each second from an REG on a serial port, stores the trials on the local disk, and transmits the data to a server in Princeton in checksummed 5-min packets. Custom software on the server stores the data in permanent archives with all data synchronized using network time protocols. The result is a continuously growing swath of parallel data sequences extending from August 1998 to the present time (Nelson, 2001; Bancel & Nelson, 2008). The database is publicly available for download by anyone with an interest in checking our analyses or conducting original research.

A large and comprehensive website at http://noosphere.princeton. edu provides details of the technology and methods, a complete record of the formal hypothesis testing we have done over the years, the primary results, a growing spectrum of deeper explorations of the data, and some interpretive efforts. To date, there are over 250 rigorously vetted, prespecified events in the formal series, including tragedies and celebrations, natural- and human-caused disasters, planned and spontaneous gatherings of great numbers. The primary experiment consists of formal events that are specified in a prediction registry prior to any examination of the data. Relatively few events are selected, and the formal series comprises 1.5% of the full 10-year, 15-gigabyte database. Since we are breaking new ground in psi research, there is little or no history of similar research to guide hypothesis specification. We therefore use a general hypothesis that allows the criteria for selecting events and analysis tools to be kept deliberately free:

> Periods of collective emotional or attentional behavior in widely distributed populations will correlate with deviations from expectation in a global network of physical random event generators.

A series of replications (analyses of data corresponding to the individual global events) using this general approach allows us to maintain formal rigor while exploring a variety of occasions that bring people to a common focus. By accumulating subsets of event categories, we gain insight into psychological (or sociological) parameters that help determine the nature and magnitude of anomalous effects in the data. The approach allows considerable latitude in identifying events and constructing test statistics, but with a number of constraints. The events specified in our formal hypotheses all involve large numbers of people, geographical extension, an engaging emotional or attentional character, and they are expected to promote or entail mental coherence.

The GCP is an evolutionary development in psi research that differs qualitatively as well as quantitatively from prior research. The globally distributed network produces synchronized data in parallel sequences from dozens of physical random sources, allowing a class of investigations that includes interdevice correlations, measurement of momentary variance and covariance, assessment of distance and time as parameters, and quantitative research on the possibility that multiple random sources may augment or otherwise differentiate the response.

GCP FINDINGS

We have found that the anomalous effects typically take a different form from that observed in laboratory REG research. During 10 years of operation we have specified and analyzed 250 global events constituting our operationally defined moments of "global consciousness." The nature and scale of the database provide a number of unique opportunities and findings:

1. Globally shared thoughts and emotions can affect physical random systems.
2. When global events transpire, we find anomalous structure in the GCP data.
3. The average effect size is small, about 0.3 to 0.5 sigma, but conceptually replicable.
4. The odds against chance for the composite formal result are about 10 million to 1.

5. The anomalous effects are seen in the collective behavior of the global network.
6. Deviation (or structure) is driven by excess pairwise correlation between RNGs.
7. Distribution statistics of RNGs are unperturbed, but they correlate during events.
8. Two independent, orthogonal correlation statistics respond similarly to the formal events.
9. The orthogonal measures of network correlation are also correlated with each other.
10. There is differential response of correlation statistics to categorized subsets of events.
11. Both correlation statistics exhibit a similar distance dependence, with scale ~ 8000 km.
12. The temporal course of correlations shows GCP effects have a time scale of 1 to 2 hours.

When we compound the statistics from the several independent findings, we compute a bottom line deviation of more than 5 sigma ($Z > 5$ and p value on the order of 10-7), representing evidence for structure where there should be none in the random data. We think this carefully established anomaly relating consciousness and physical randomness bears implications for both the study of human consciousness and our understanding of the physical world.

Detailed discussion is beyond the present scope, but some comments are in order. It is essential to understand that we do not look for "spikes" in the data and then try to identify what caused them. Instead, we identify the event first and then analyze the corresponding data—we make a prediction before examining the data and then test it in the data. This process yields a replication series of proper hypothesis tests that in their aggregate constitute a test of the general hypothesis given earlier. We find that whereas we can measure deviations in data corresponding to the identified events, the database as a whole exhibits parameters consistent with statistical expectation. The significance of each of the results enumerated above, and of the composite bottom line, has been confirmed by extensive simulation using pseudo-random data and direct resampling analyses from the network database.

The discovery of two demonstrably independent statistics is important to the development of models and helps to constrain the range of possible explanations. It also helps assure that the anomalous results cannot be ascribed to data selection. The discovery that the anomalies are not simple, direct effects on individual REGs but are driven primarily by interdevice correlations is an instructive surprise. It is yet another indication of the complexity faced by psi researchers and an example of the importance of the questions asked. The range of distances over which the internode correlations are detectable is approximately 8,000 km, and weighted regressions show a marginally significant decline in effect size over global range of separations. This indicates that although the measured effect is indeed global, it is nevertheless sensitive to the geographical extent of the network and the distribution of the events. We can ask what the implications are for the widespread, albeit still tentative, idea that psi effects are fundamentally nonlocal. Finally, temporal structure is also an important feature of the GCP data. Our operationally defined global consciousness would seem to have a "moment" of an hour or two, perhaps corresponding to the much faster time-scale of human consciousness where a sensory or emotional impression can form in a small part of a second, perhaps as little as 100 milliseconds.

CONVERGENT EVIDENCE

There is a powerful general point to be made from the psi literature. Given that there are many experiments and observations of high quality showing anomalies in a wide range of disciplines, and independent findings pointing to effects of consciousness that are not accounted for in ordinary psychological or physical theories, we can say that there is excellent "convergent evidence" that consciousness interacts with physical reality. When there is just one opinion or one experimental observation on a phenomenon, it is difficult to make a case. But with more than 100 years of research by highly qualified scientists looking from different perspectives at the extended capacities and limitations of mind, we can consider whether their findings converge. I think they do, in no uncertain terms, despite, and indeed with the help of, criticisms that ultimately have strengthened the evidence. We have personal experience and observation of natural occurrences of psi. We have laboratory experiments on extrasensory perception, clairvoyance, psychometry, psychokinesis, and more. We have extensions of these efforts to learn something in the real world, some pragmatic and some purely experimental. Government and business have requested and gotten help from psi practitioners, sometimes with high profile public presence as in the Stargate program of remote viewing. Pertinent to our theme, such work may be regarded as applications of techniques and findings from controlled laboratory research (Dunne & Jahn, 2003; Targ & Puthoff, 1977). Similarly, studies of micro-psychokinesis in the laboratory have lead to field research on group consciousness attempting to confirm that special states of resonance or coherence reportedly stimulated by ritual, music, collaboration, and cooperation may have a detectable presence beyond the experiential (Nelson, et al., 1997; 1998a; 1998b).

The natural extrapolation of field research with REGs into the Global Consciousness Project is a multilevel example of convergent evidence. Not only does the GCP present an independent and completely different perspective on the question of whether mind has real presence in the world, but its application of powerful modeling and statistical techniques to search for structure in this large and complex database seeks convergent evidence internally. The result is a collection of findings that are on the one hand demonstrably independent and on the other hand complementary; they are interlocked pieces of a comprehensive picture. Again we find indicators of a real entity that is anomalous in the sense that ordinary physical models do not yet accommodate it. But this evidence converges with and extends the field studies of group consciousness and the laboratory research with individuals. The GCP results say essentially the same thing as do the results of decades of psi research in laboratories around the world, albeit in a different but very rich language. Consciousness is real. It has a role to play as a presence in the physical world. Our work as psi researchers is to go on with efforts to learn more about that presence and to make clear that the role of consciousness in the world is both real and important. In this first decade of the 21st century, it is becoming apparent that that role is critical.

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