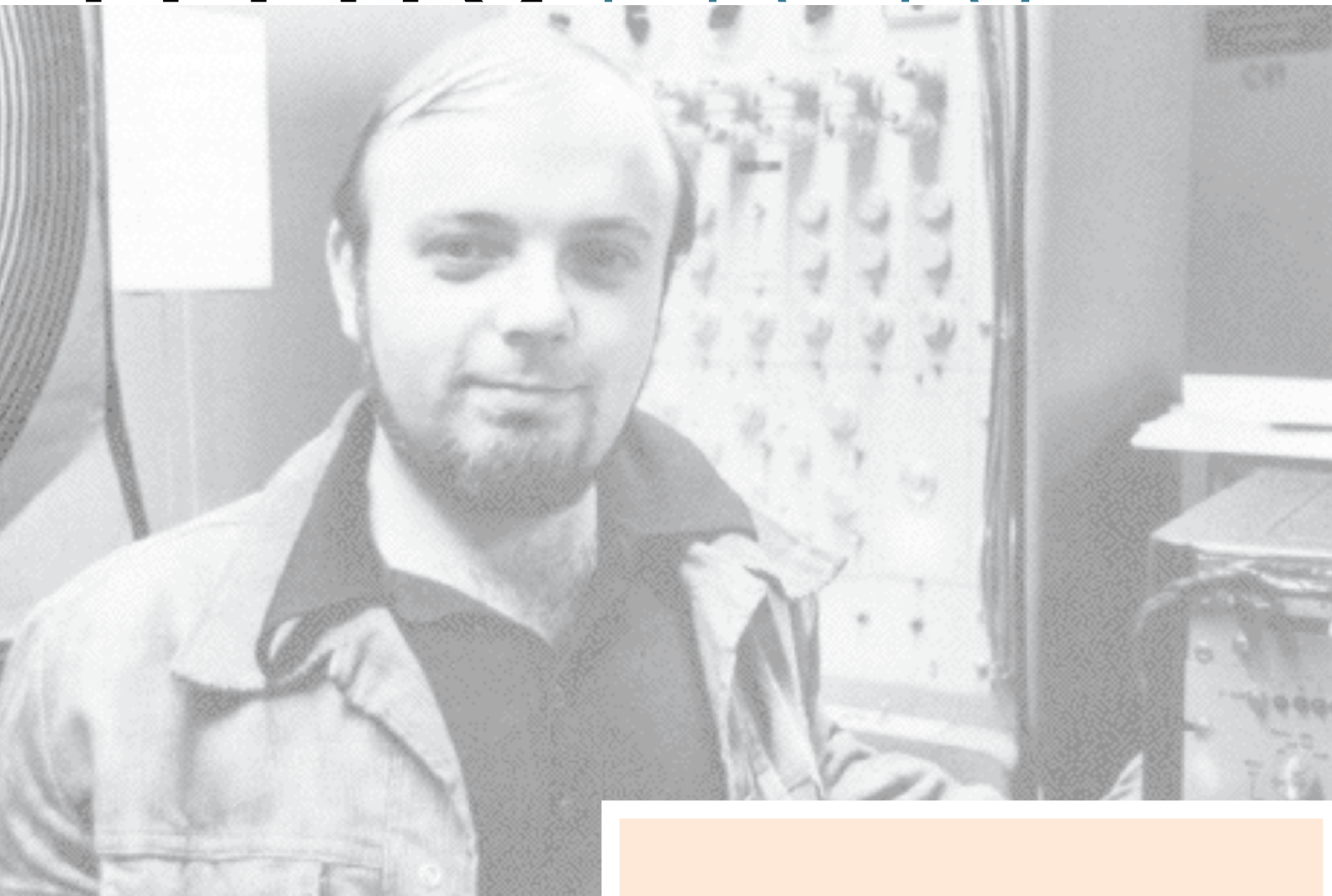


The Bulletin of the
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Volume 9
Issue 3



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From the Editor's Desk

The Bulletin of the
Parapsychological
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| by ETZEL CARDEÑA,
CERCAP, Lund University

This is my last issue as editor of *Mindfield*. Years ago, and before I knew I would be offered the editorship of the *Journal of Parapsychology*, I approached the then PA Board of Editors to let them know that they should start planning for a new editor because I do not think it is healthy for any organization to have the same person in the same unelected position for many years, as the PA has had. My goal for *Mindfield* was to transform a newsletter into a bulletin with not only

news but also substantial features, and having just made an index of the first nine volumes confirms that this was accomplished. These issues include the valuable bibliographies by Gerd Hövelmann, very important articles by Beloff, Braud, Horton, Roe and many other leaders in the field; reflections by Gauld, Carpenter, Targ, Tart, and other foundational figures; scholarly historical essays by Alvarado and others, and tributes to important people who died during the last 9 years. I have already cited some of the articles in *Mindfield* in peer-reviewed papers and hope that others will do the same. *Mindfield* conveyed important ideas from its first issue, but it took some volumes for Nikolaos Koumartzis to fully display his inventiveness in striking designs. I thank profusely all contributors, with special thanks to Carlos Alvarado and Gerd Hövelmann for being an intrinsic part of the bulletin throughout, and to Nikolaos Koumartzis for the layout. *Mindfield* is now in the hands of a troika, Renaud Evrard, Nikolaos

Koumartzis, and Annalisa Ventola, to whom I wish good luck.

Index of the First Nine Volumes of *Mindfield*

For general information and archival purposes here is an index of the 9 volumes I edited, arranged thematically and by volume and issue number, but not including my or the Presidents' general columns, reviews of past conventions and reports, or abstracts from other publications:

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For those interested in purchas-
ing hard copies of past issues
of *Mindfield*: [http://www.
magcloud.com/browse/maga-
zine/1335812](http://www.magcloud.com/browse/magazine/1335812)

I attended the 60th Annual Con-
vention of the Parapsychological
Association in one of the cradles

of Western civilization, Athens.
Here is my review (I attended
almost all events except for the
Presidential Address, which will
be published in the forthcoming
issue of the *Journal of Parapsy-
chology*). There was a preconven-
tion, open to the public and free,
Engaging the oracle, that included
presentations by senior figures
Roger Nelson, Chris Roe, and Ma-
rio Varvoglis, discussions on para-
psychology in Greece by Marilena
Avraam-Repa and Fotini Pallikari,
and a presentation on counseling
by Erika Pratte. I did not attend
the preconvention but was told
that it drew a large audience of
Greek people. Although regretta-
bly most of them did not attend
also the PA convention itself,
which was rather sparsely attend-
ed, I think that these events help
spread the word about serious
work in the field. The papers in the
convention followed, as typically
in most conventions, an approxi-
mately normal distribution, with a
few outstanding papers, most at
a competent level, and a few that
I consider subpar. Some of the, for
me, most interesting contributions
included Peter Mulacz revelation
of how many members of the very
important philosophical “Vienna
Circle” in the 20th century were
interested or even engaged in psi
research. For those measuring
correlates of psi experiences and
phenomena, a new *Belief in the
Supernatural Scale* by Malcolm
B. Schofield and collaborators

is worth paying attention to and
will be published shortly in the
Journal of Parapsychology. Jacob
Jolij and Dick Bierman presented
supportive data for an implicit
psi effect in a stimulus detection
task. Following a previous at-
tempt at improving the technology
of ganzfeld, Mario Varvoglis and
coauthors described a new immer-
sive display system, the *Selfield*,
along with data from a study in
which, again, serious long-term
meditators seemed to exhibit psi
effect but not other groups. Ed
May spoke about the imminent
release of the 4-volume release of
the *Star Gate* archives, which he
describes in this issue of *Mindfield*.
Adrian Ryan made a case for psi
researchers sending their data to
the data depository he has devel-
oped under the auspices of the
SPR, found here [https://www.spr.
ac.uk/news/psi-open-data](https://www.spr.ac.uk/news/psi-open-data). And
there was a tribute to the inesti-
mable personal and professional
contributions of Gerd Hövelmann.

Various members of the au-
dience commented during the
general meeting about how
difficult it was to follow record-
ed PPT presentations, and some
of us were critical about papers
on effects of drugs which did not
refer to essential work done on
the area previously. Finally, the
J. B. Rhine Address by a distin-
guished Greek scientist left me
and others disappointed because
while the presenter gave a coher-
ent presentation of the idealist

perspective in philosophy, he did not make links to parapsychology even when asked about it. To end the report on a better note, the hotel staff was extremely helpful to me when dealing with some malfeasance by the metro system, the food provided by the hotel was definitely better than what I expect from conventions, there was a live performance with ancient Greek instruments, *Pythia*, which included a CD that can be ordered from Nikolaos Koumartzis, and seeing the lighted Acropolis at night from the hotel's balcony was a treat of its own.

During the convention the PA announced the winner of the 2017 Outstanding Career Award (Carlos S. Alvarado), 2017 Outstanding Contribution Award (Stephan A. Schwartz), and 2017 Outstanding Student Award (David Marcusson-Clavertz). Congratulations to them! I am especially proud of David because he is my erstwhile doctoral student and continuing collaborator.

The PA also selected these as the 2017 Parapsychological Association Book Award winners:

Woollacott, Marjorie (2017). *Infinite awareness: The awakening of a scientific mind*.

Kean, Leslie (2017). *Surviving death: A journalist investigates evidence for an afterlife*.

Evrard, Renaud (2017). *Enquête sur 150 ans de parapsychologie en France*.

Congratulations to the authors!

As I write this editorial, on September 26th, I had not yet received, despite repeated requests, the official PA reports nor had the winners of the elections been announced, thus I could not commission the incoming president to write a column for this issue, nor can I include the PA reports. During the business meeting a circa 4,000 US dollars deficit (since I do not have the report I cannot give the specific figure) was reported. I am alarmed at the repeated PA deficits for some years now, coupled to added new expenses. When I mentioned this during the business meeting the Board members replied that it would just take a few more new members to make this deficit up, which I consider an inappropriate answer. I told the Board that at this pace of deficits the PA will cease to exist in 7-8 years. The Board members responded that they were taking the matter seriously and had been discussing it, but when I asked for specifics, to quote Lewis Carroll's *The Walrus and the Carpenter*, "answer came there none." This brings me to a more general issue that I became aware of when I served as PA Vice-President and then President, which is that some members of the Board of Directors seem to run for election merely to add a line to their CVs but do not commit themselves to working hard for the organization and adopt all the responsibilities that running it en-

tail. So let me make a very serious call to all of the incoming Board members to treat their posts, and the financial deficits, with all the earnestness they deserve.

Mindfield has received the following:

Cornillier, Pierre-Émile (2017).

The survival of the soul and its evolution after death. Hove, UK: White Crow. A well-known French academic artist writes about his experiments with a model who exhibited mediumistic abilities.

Koumartzis, Nikolaos (2017).

(2017). *A glimpse of history: Psychical research in Greece during the 1930s*. Athens, Greece: Daidaleos. A booklet with photographs focusing on the flowering of psi research in Greece during the 1930s. An English short version of ...

Koumartzis, Nikolaos (2017).

(2017). *Angelos Tanagras. The lost diary*. A study of probably the most influential figure in parapsychology in Greece during the modern era, profusely illustrated (in Greek). Athens, Greece: Daidaleos.

Luke, David (2017). *Otherworlds:*

Psychedelics and exceptional human experience. London, UK: Muswell Hill Press. The foremost world expert in the rela-

tion of psychedelics and psi and other exceptional/anomalous experiences provides a thorough coverage of the topic.

White, John (2017). *The psychic and the spiritual: What is the difference?* Hove, UK: White Crow. The question in the title is definitely pertinent and John White has published many books on these topics, including two important edited books on states of consciousness a few decades ago.

Here is a link to a most entertaining series of programs, often based on actual psi research and anecdotal accounts, and with a melancholy musical motif. It is well-worth watching! <http://one-step-beyond.info/>

And there has been a lot of discussion in a closed psi list about a philosopher whose book got a PA award a couple of years ago and has been involved in very controversial political activities. Robert McLuhan provides the context of this issue in <http://monkeywah.typepad.com/paranormalia/2016/10/psi-and-the-far-right.html>

My final *Mindfield* is bountiful as I needed to include all the materials I had. I will start with the *pièce de résistance*, a reprinting of Charles Honorton's 1975 Presidential Address (thanks to Ed May for sending me a Word version), where he provides an overview of the field and its criticisms, as trenchant and fresh as if penned

yesterday. It also underlines the importance of alterations of consciousness in psi performance, a theme central to my own work. Reading older pieces of brilliant minds like those of Chuck Honorton or William Braud should be an entry requirement for anyone wanting to do work in this area. In his guide to the SPR Encyclopedia entries on survival, Carlos Alvarado shows his profound historical grasp of all things psi and the extraordinary ongoing achievement of the SPR online encyclopedia shepherded by Robert McLuhan. I hope that future *Mindfields* will consider assigning similar summaries. Edwin May provides a good introduction to the 4 forthcoming volumes on the Star Gate landmark project. From the Netherlands, Maurice van Luijelaar describes a couple of fascinating ostensible precognitions of air crashes (similar to Mary S. Stowell's cases, published some years ago), exemplifying how much we need thorough accounts of ostensible psi phenomena as they occur in "real life."

Two éminences grises, Daryl Bem and K. Ramakrishna Rao, offer summaries of their careers in the field. This issue also has abstracts in English of the latest *Zeitschrift für Anomalistik* (thanks to Gerhard Mayer for sending them), with two entries by my dear friend Gerd Hövelmann, who accompanies me thus all the way to the end of my tenure as *Mind-*

field editor. And I have the sad task of honoring those preceding us, in this case two women with very different but valuable contributions to the field, Jean Burns and Inge Strauch.

As we were coming to press news came out that Yvonne Duplessis has died. There was not enough time to commission a proper obituary but Renaud Evrard sent me the following lines:

"Have you ever considered still running a parapsychology lab in your basement before your death? Or, being more than a hundred-years-old, urging your guests to test their parapsychological and dermo-optical faculties? Yvonne Duplessis cultivated a passionate curiosity for human sensitivity for more decades than that the life of the average human. She joined the light on September 21, 2017, at the age of 105, and probably took advantage of it to analyze the infinite diversity of colors that she loved so much. (To discover some element of her work, you can find my portrait of this "grande dame" of French parapsychology in the *Mindfield* 5(3) issue)."

Adiós, goodbye, hej då, au revoir, auf wiedersehen, ciao!

Has Science Developed the Competence to Confront Claims of the Paranormal?

PA Presidential Address, 1975, by Charles Honorton



Charles Honorton in his research laboratory at Maimonides Medical Center, Brooklyn, NY

Explaining their decision to publish a preliminary report on “remote perception” experiments performed at Stanford Research Institute in the fall of 1974, the editors of *Nature* openly posed the issue whether science has yet developed the competence to confront claims of the paranormal. The editors of *Nature* deserve a great deal of credit for

having the courage to raise this question, and in order to explore the issue further, I think it will be helpful to examine both “establishment” science and “paranormal” science.

Establishment Science

It is appropriate to begin with establishment science, the scientific

orthodoxy, or “mainstream,” since this is what defines and limits the scope of factual knowledge. Through its publication practices, establishment science controls the dissemination of research findings. Through the top-level policies of its executive councils, establishment science controls the disposition of research funds. And through our educational institutions, establishment science teaches what is currently known and/or believed about the nature of reality. Has establishment science developed the competence to confront claims of the paranormal?

Before pursuing this question further, I think it is important to recognize that despite the impressive accomplishments of modern science and its applied technologies, science has yet to come to grips with some of the fundamental problems underlying much of what it currently regards as normal. Among the still unanswered questions of normal science are these: What is the source of

power of the atomic nucleus? How is biochemistry translated into consciousness? Where is memory? These and similar questions tend to be ignored, but are kept alive by our truly great scientists, men like Eddington, Eccles, Wigner, and Wheeler.

Those of you who are familiar with the history of paranormal research will, I think, agree that establishment science competently confronted claims of the paranormal during the period we now call the “ESP Controversy” of the 1930’s. During the five-year period following publication of J. B. Rhine’s *Extra-Sensory Perception* in 1934, the scientific community responded as it should to any claim of new discovery, by disseminating both positive and negative research findings, by careful scrutiny of the experimental and evaluative techniques, and by encouraging fresh replication efforts.

During this period, there were approximately 60 critical articles by 40 authors, published primarily in the American psychological literature. Fifty experimental studies were reported during this period, two-thirds of which represented independent replication efforts by other laboratories of the Duke University work. The critical issues raised during this period were, for the most part, legitimate ones, and the experimentalists were quick to modify their procedures to accommodate valid criticism.

By 1940 there was, if not a gen-

eral consensus on the reality of ESP, at least a general consensus on what constituted a good ESP experiment. Yet despite the adequacy of many of the experimental studies, conceded even by the leading critics of the period, and despite the continued accumulation of new experimental confirmations, the active confrontation between establishment science and claims of the paranormal went into hibernation for a decade and a half. Between 1940 and the mid-1950’s, very little discussion of paranormal claims appeared outside the parapsychological specialty journals. Virtually no funding was available for research and graduate students who wanted their degrees were strongly discouraged from pursuing parapsychological topics. Aside from occasional textbook and lecture references to obsolete criticisms of the early Duke work, establishment science, during this period, sought to ignore rather than to confront claims of the paranormal.

In the 1950’s, the two leading interdisciplinary journals of science published speculative attacks on paranormal research. *Nature* carried what in itself was a “paranormal” claim, in the form of Spencer Brown’s attack on probability theory (Brown, 1953). *Science* gave special prominence to George Price’s article on *Science and the Supernatural* (Price, 1955). Price’s confrontation with the paranormal began with the

By 1940 there was, if not a general consensus on the reality of ESP, at least a general consensus on what constituted a good ESP experiment.

assumption that ESP and other paranormal claims are impossible. While conceding the methodological and statistical adequacy of many of the experiments purporting to demonstrate ESP, Price argued that since ESP is impossible, experimental evidence that cannot otherwise be explained away should be regarded instead as evidence of experimenter incompetence or dishonesty. Seventeen years later, in a cryptic *Apology to Rhine and Soal*, Price (1972) retracted these allegations.

Our current situation shows definite signs that establishment science is once again attempting to actively confront claims of the paranormal. There are numerous indications of this, but I will discuss just two. While federal grant support for parapsychological research is still virtually nonexistent in comparison with more conventional research on, say, military frisbee design, a beginning has been made in the support

of a few projects. There is at least now some inclination on the part of funding agencies to consider individual proposals on the basis of merit, past accomplishment, and the likelihood of continued success.

Aside from providing financial support for new research, the greatest contribution establishment science can make toward the resolution of controversy over *any* new claim, I believe, is to allow the dissemination of research findings, both positive and negative. In this regard, the current confrontation between establishment science and claims of the paranormal must be considered to be highly ambivalent.

On the positive side, symposia on parapsychological methods and research findings are becoming a regular feature on the annual programs of a number of scientific societies, including the American Psychological Association and the American Association for the Advancement of Science. The affiliation of the Parapsychological Association with the AAAS in 1969 has provided an important new forum for the dissemination and discussion of parapsychological findings, primarily through symposia at AAAS annual meetings.

Yet while the AAAS encourages us to sponsor symposia at its annual meetings, its journal, *Science*, continues to suppress the interdisciplinary dissemination of our research findings. Even since

On the positive side, symposia on parapsychological methods and research findings are becoming a regular feature on the annual programs of a number of scientific societies, including the American Psychological Association and the American Association for the Advancement of Science.

our admission into the AAAS, *Science* has published only reports of negative findings. Since *Science* is a highly selective journal, accepting only about 20 percent of the reports submitted to it for consideration, the publication of negative findings indicates that the editors of *Science* regard the problem-area to be one of some importance. If this is so, why has *Science* consistently rejected

competent experimental reports with positive findings?

There is no more effective means through which to answer *Nature's* editorial challenge than to allow interdisciplinary consideration of both positive and negative findings in this area. This would allow the interdisciplinary scientific community to know just exactly what is being claimed about the paranormal; to assess for itself the degree of competence and the level of development; and to stimulate new independent replication efforts.

Yet, despite encouraging indications to the contrary, the current status of the situation is still, I think, subject to the constraints imposed, in a well-known quote from the physicist, Max Planck, by what I shall call "Planck's Second Constant": "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it" (Planck, 1949). For the moment, I think we must conclude that while *Nature* has developed the competence to confront claims of the paranormal, *Science* hasn't.

Paranormal Science

Has parapsychology developed the competence to confront claims of the paranormal? Not as much

	Number of Experiments	No. Reporting Statistical Significance ($p \leq .01$)	Percent Significant
Duke Group	17	15	61
All Other Laboratories	33	20	61
Total	50	35	70
*Chi square (Duke vs. Other \times Significant vs. Nonsignificant) = 1.70 (df = 1; nonsignificant).			

Table 1. All English-language ESP experiments involving statistical assessment of data, 1934-1939 inclusive*.

as we would all like, but much more than anyone has a right to expect given the level of support, the degree of irrational prejudice, and the small number of competent investigators who have been able to sustain themselves in this field. In fact, I suggest to you that some of our paranormal claims have much more support behind them than many of the more widely accepted claims of normal science.

Let us examine the replication status of findings in parapsychology and other areas of behavioral research. Almost all of the discussion of this important topic has occurred without the necessary bookkeeping. First, let us return

to the card-guessing experiments of the 1930's and examine the replicability of that work. The central claim upon which ESP was based during this period was formulated as follows: "Is it possible repeatedly to obtain results that are statistically significant when subjects are tested for knowledge of (or reaction to) external stimuli (unknown and uninferable to the subject) under conditions that safely exclude the recognized sensory processes?" (Rhine et al., 1940, p. 15).

Even among parapsychologists there is a rather widespread belief that most of the independent replications of the early Duke work were nonconfirmatory and

I suspect this may be especially true among those of us who were not around in the 1930's (which, incidentally, accounts for about three-fourths of the participants at this convention). In fact, I was surprised myself to find that this wasn't so when I undertook a review of all of the English-language ESP experiments reported during the period between 1934 and 1939 (Honorton, 1975a). The results of this survey, in terms of replication, are shown in Table 1.

This work involved a database of approximately 3.3 million individual trials. As Table 1 indicates, 61 percent of the independent replications of the Duke work were statistically significant. This is 60 times

the proportion of significant studies we would expect if the significant results were due to chance error. Of course, there is also experimental error and some of these studies left much to be desired in terms of methodology. Yet on the basis of my own study of this literature, I concluded that at least 33 of these 50 studies were methodologically adequate on the basis of the experimental reports.

The next question, then, is this: Were all of these laboratories suppressing mountains of nonsignificant data?

I think this is very unlikely, judging from the temper of the times, but it is not necessary to rest our case on this. It is legitimate to ask what volume of research data could realistically have been generated during this period. It is important to recall, in considering this question, that in the 1930's there were only two funded research laboratories in parapsychology, one at Duke University and the other at Stanford University. (I believe the Stanford endowment still exists, although the Stanford officials have consistently allowed it to be used for other purposes.) It is also important to bear in mind that the volume of *reported* work during this period was 3.3 million individual trials and that it takes time to shuffle ESP cards and to record subjects' guesses.

About six months ago, my friend and colleague, Edwin May, opened

a suitcase, which he had carried all around India. Inside was an electronic random number generator. Just to illustrate what a little technology will do for an impoverished research area, this instrument is the third or fourth generation version of an instrument that was introduced in parapsychology only five years ago by Helmut Schmidt. It looked to me more like something one would find at Mission Control in Houston than in a parapsychological research laboratory in deepest Brooklyn. Ed explained the myriad of controls that decorate the front panel. "These knobs," he said, "allow us to ask 'physics-type' questions about PK. This one allows us to adjust the trial rate from one trial every ten seconds to a million trials in ten seconds....in millisecond increments, of course," he added with a smile.

Eager to play, we decided to run a control series of random checks. Several minutes and millions of trials later, I suddenly realized that we had just collected more trials in those few minutes than had been reported during the entire 60-year period between 1880 and 1939.

Even if we assume a position of extreme conservatism and allow for the possibility that for each published study between 1934 and 1939 there were five studies that were not reported and were nonconfirmatory (that is, 250 unpublished studies with 16.5 million nonsignificant trials), the rate of

replication of the ESP hypothesis would still be highly significant.

Replication in Parapsychology and Other Areas

It has become clear to me that the replication status of parapsychological findings must be viewed within the larger perspective of replication in other areas of research. Last year at Maimonides we completed a two-year dream research project, which was supported by the National Institute of Mental Health (Honorton & Ullman, 1975). This project was designed to replicate and extend our earlier telepathic dream work as well as to replicate certain nonparapsychological findings by another investigator. This investigator, while still analyzing data from a project begun six years earlier, had published several preliminary accounts of findings based on general impressions of his data. His tentative conclusions had gained fairly widespread acceptance, despite the exploratory character of his initial reports. To make a long story short, we not only failed to replicate our own telepathic dream findings, we also failed to replicate three of the four nonparanormal claims of this other investigator.

Recently, a number of us have begun exploring the possible role of mental imagery as a mediator of psi (i.e., paranormal information

Two dozen research laboratories are represented on the program of this year's convention. There are 48 experimental reports with findings based at least in part on significance tests.

flow). Six reports have appeared thus far relating ESP performance to one very popular scale, which purports to measure the vividness of imagery. While three of these studies showed some relationship between psi and imagery, as defined by this scale, the direction of the relationship has shown a disconcerting tendency to vary from study to study. We might be tempted to ascribe such lack of consistency to that perennial scapegoat, the "elusiveness of psi," were it not for the fact that similar reversals between this scale and a variety of nonparanormal performance measures seem to be more the rule than the exception (Honorton, 1975b).

I could go on, but I know that many of you can provide similar instances of how the findings of

more established–nonparanormal–fields cannot be relied upon. My point is simply this: the problem of replication did not begin and does not end with parapsychology. In fact, parapsychology may actually be in a better position vis-à-vis replication than many other areas of behavioral research.

Let us take a brief look at the publication practices of the American Psychological Association, for example. A number of studies have been reported of APA publication practices. I will summarize just two. Sterling (1959) sampled 362 research reports published in three of the leading APA journals. He found that while 97 percent of the studies basing conclusions on significance tests reported rejection of the null or specific hypothesis, *none* of the research reports was of a replication of previously reported findings. Similarly, Bozarth & Roberts (1972) sampled 1344 research reports published in four additional journals. They found that while 94 percent reported findings based on the rejection of the null or specific hypothesis, *less than one percent* was of replications of previous findings. I do not see how we can avoid the conclusion that it is impossible to assess the validity of findings and degree of replication in many areas of psychology.

Now let us return to our own situation and examine the replication status of research findings in contemporary parapsychology.

Let us begin by examining what is being reported right here at this meeting and then step back and see how well or poorly some of our findings are holding up relative to similar earlier work.

Two dozen research laboratories are represented on the program of this year's convention. There are 48 experimental reports with findings based at least in part on significance tests. Rejection of the null or specific hypothesis is reported in 63 percent of these studies and by 17 different laboratories. Moreover, 73 percent of the studies reported at this convention are of attempted replications of previously reported findings. Of these, 43 percent report significant confirmation of earlier findings.

I will now examine in somewhat greater depth two areas that have sustained systematic research efforts over a period of years. My selection is somewhat arbitrary and reflects my own research interests. There are other areas that could serve equally well.

Internal Attention States

The first area involves the use of internally-deployed attention states, brought about by a variety of means, to detect and register paranormal information flow. We can formulate the central claim or finding in this area as follows: psi information flow is more easily detected and recognized when the

receiver is in a state of sensory relaxation and is minimally influenced by ordinary perception and proprioception.

A good case can be made for this claim on analytical grounds alone and the empirical support for it pervades nearly every aspect of our literature, from our studies of cultural practices and spontaneous experiences to our clinical case histories and experimental reports.

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Let us first examine some of the analytical considerations. For these we are indebted to the late Cambridge philosopher, C. D. Broad, whose theoretical contributions to parapsychology have not yet received the attention and recognition they clearly deserve. Broad was among the first to suggest that if paranormal phenomena occur at all, they are probably much more pervasive in our everyday lives than has traditionally been recognized. Drawing an analogy with the discovery of magnetic fields, for example, he wrote:

Had it not been for the two very contingent facts that there are lodestones and that the one element, iron, which is strongly susceptible to magnetic influences, is fairly common on earth, the existence of magnetism might have remained unsuspected to this day. Even so, it was regarded as....[an] anomaly until its connection with electricity was discovered, and we gained the power to produce strong magnetic fields at will. Yet all this, while magnetic fields had existed and had been producing effects whenever and wherever electric currents were passing. Is it not possible that natural mediums might be comparable to lodestones, that paranormal influences are as pervasive as magnetism, and that we fail to recognize this only because our knowledge and control of them are at about the same level

as were men's knowledge and control of magnetism when Gilbert wrote his treatise on the magnet? (Broad, 1953).

Broad pointed out that psi interactions need not involve conscious experience at all, that those that do are merely the tip of an iceberg, and that below the surface, psi interactions may occur as totally unconscious influences on our moods, our dispositions, our thought processes, and so on. As we have already seen at this convention and in the literature over the last several years, experimental support for this far-out notion has begun to emerge through the work of Stanford and his associates (Stanford, 1974; Stanford et al., 1975).

As for the relative rarity of *recognized* psi interactions (i.e., psi experiences), Broad's analysis of the conditions necessary for the recognition of paranormal input makes it clear that these conditions are normally seldom fulfilled. This analysis also makes it clear why psi experiences are so often associated with internal attention states.

Suppose, recasting Broad's analysis in the communications terminology proposed by Morris (1975), that the output of an information source served as an influence on a sensorially-remote receiver. In order for the receiver influence to be detected and identified with its source, all of the following conditions are necessary and must be fulfilled:

- The receiver influence must be detected. With human receivers, this means that the influence must take the form of overt behavior or conscious experience, which the receiver can and does attend to.
- The experience must be sufficiently prominent, or carry sufficient impact, to allow the receiver to distinguish it from among the many other, non-paranormal inputs, which are concurrently influencing him. In this context, normal perceptual, somatic, and cognitive influences on the receiver may constitute sources of noise.
- The experience must be stored and reported prior to receiver-source contact through normal channels, otherwise it cannot be considered evidential.
- There must be subsequent confirmation of a meaningful correspondence between the source and the receiver.

This correspondence must be sufficiently unusual or consistent over repeated efforts to eliminate chance coincidence as a reasonable explanation.

These detection criteria can account for some of the most prominent features of spontaneous paranormal experiences. The high incidence of spontaneous psi experiences occurring in dreams and other internal attention states would be expected, inasmuch as such states are associated with

deafferentation–sensorisomatic noise–reduction–and deployment of attention inward, toward mentation processes such as thoughts and images, which may serve to carry psi information, thus increasing the likelihood of detection. The utilization of imagery and other forms of mentation in the processing of environmental information has been demonstrated in studies of subliminal stimulation—which, incidentally, is also facilitated by internal attention states (Dixon, 1971).

The high incidence of paranormal experiences occurring between friends and relatives and the low incidence of occurrence between remote acquaintances and strangers would also be expected, since there is naturally a greater likelihood of *confirmation* in the former case. Unless the source and receiver know each other and are in relatively frequent contact with one another, the likelihood of confirmation is very low. Furthermore, unless the relationship between the two permits some degree of intimacy, it is unlikely that either would be sufficiently uninhibited to share unusual personal experiences.

Similarly, the high incidence of crisis cases involving situations of sudden, unexpected emotional significance would be expected, since such experiences carry more impact and are more likely to be remembered and recalled than relatively trivial mundane experiences.

The association between paranormal information acquisition and internal attention states can be traced back at least as far as the Vedic period of India and the reports of *siddhis* or paranormal powers manifest in yogic meditation. Then there are the claims of the powers of entranced medicine men and the shaman of the nontechnological societies; the “higher phenomena of hypnotism” reported by the early Mesmerists; the phenomenological descriptions of cognitive strategies employed by gifted individuals, such as Mary Craig Sinclair; and finally, the cross-cultural validation of dreaming as the state-of-choice in spontaneous psi experiences and psychotherapeutic case reports.

Now let us look at the status of controlled laboratory experimentation in this area. I have been able to find 89 experimental studies spanning a 30-year period and involving controlled investigations of psi retrieval in internal attention states brought about by dreaming, guided imagery and hypnosis, meditation, perceptual isolation, and progressive muscular relaxation (Honorton, 1977). At least two different laboratories have reported work with each of these five procedures and this work was contributed by a total of 26 different laboratories or investigator teams. For present purposes, I will examine these studies primarily from the standpoint

Procedure	No. of Experiments	No. of Experiments Significant at $p \leq .05$	No. of Significant Experiments Expected by Chance	Binomial p less than*	No. of Labs	No. of Labs Confirming
Dreaming	18	10	0.90	1×10^{-5}	4	2
Hypnosis-guided Imagery	39	19	1.95	1×10^{-5}	19	8
Meditation	11	8	0.55	1×10^{-5}	4	3
Perceptual Isolation	15	8	0.75	1×10^{-5}	7	2
Progressive Relaxation	5	5	0.25	1×10^{-5}	2	2
TOTALS	89	50	4.45	1×10^{-10}	26	17

*Exact binomial probabilities calculated on the basis of the number of studies significant at $p \leq .05$ out of the total number of studies for each procedure.

Table 2. Replication status of experimental studies involving psi retrieval during internally deployed attention state, 1945-1975.

of replicability. The question is this: How many of the studies with each procedure and overall have reported clear-cut evidence that paranormal processes were operating within the design of the experiment, and how does this number compare with what we would expect purely on the basis of chance error? For the purpose of this analysis, I am defining as “significant” only those studies in which, on the basis of overall psi scores or clearly stated prediction, the investigators rejected the null or specific hypothesis at the .05 level or lower. These results are summarized in Table 2.

The replication rate with each of these five procedures is statistically highly significant ($p \leq 10^{-5}$). Overall, of the 89 experiments, 50 reported rejection of the null or specific hypothesis. This is 10 times the number we would expect on the basis of chance error. Significant findings were reported by 17 of the 26 laboratories; that is, by two-thirds of the laboratories involved.

Over 60 percent of these studies involved free-response ESP designs, which, as you know, are much more time-consuming than shuffling ESP cards. Nevertheless, let us assume for the sake of extreme conservatism that for each published study there are five that were not reported and were not statistically significant. This would require 445 unreport-

ed, nonsignificant studies. I cannot imagine how anyone who is familiar with the amount of time these studies require and with the level of support and research output of this field, could really believe that there are anywhere near this number of unreported studies; but let us assume that there are. The rate of replication in this area would still be highly significant.

Now let us compare these data with the new work being reported here at this conference. This year there are 11 experimental reports involving psi and internal attention states, contributed by five different laboratories. Six of these experiments report rejection of the null or specific hypothesis. This gives a replication rate of 55 percent compared to the 56 percent rate for the earlier work. These significant confirmations are reported by four of the five reporting laboratories and this year's crop is 10 times greater in proportion than we would expect on the basis of chance error. This harvest would still be significant if we were to posit five nonsignificant and unreported studies for each one reported at this conference.

I cannot take time now to describe some of the more interesting secondary findings related to this area. Elsewhere I have shown that the information rate for psi studies combining free-response designs with internal attention states is several orders of magni-

tude larger than for forced-choice guessing designs (Honorton, 1975c).

Nor have I discussed the quality of methodology in these studies. These experiments all utilized standard procedures for eliminating sensory contact between source and receiver, for randomizing targets, and so on. It is doubtful that any of them would live up to the reputation for super-control enjoyed, for a time, by Soal's work with Shackleton and Stewart. But this is not necessary. Each of these studies asked a question and provided a tentative yes-no answer, which, in turn, led to another study, in-house and by another laboratory. This, after all, is how normal science operates, at least in its better moments. The experimental claim results in another experiment rather than in a post-humous attack on the integrity of an individual investigator. Soal's honesty would not now be the topic of idle speculation (Mundle, 1973; Scott & Haskell, 1973) if he had had a replicable procedure rather than a gifted subject.

The existence of a new or hitherto unrecognized natural phenomenon cannot rest upon unrepeatable experiments by isolated investigators working with unique practitioners. Gifted subjects have much to contribute to our knowledge and understanding of psi, but so far, we have largely wasted their talents.

Microdynamic Psychokinesis

The second area I have selected for special attention involves psychokinesis on microphysical systems. The central claim or finding in this area may be formulated as follows: a human observer provided with sensory feedback to an external source of randomness produced by a microphysical process can, by attending to the feedback signal, effect an influence on that microphysical process with the result of decreasing its randomness.

Order out of disorder, as a function of intention, perhaps. The history of PK, even more than that of ESP, is tainted with fraud, malobservation, and uncontrolled events occurring in suspicious circumstances. Despite the apparently good controls employed a century ago by Sir William Crookes in his investigations of D. D. Home, or the Naples sittings with Eusapia Palladino, or even the impressive credentials of certain members of the “Metal Benders’ Guild” in contemporary Great Britain, such observations have never carried great conviction outside the small circle of firsthand observers present.

Moving from the macroscopic effects associated with poltergeists, hyperactive tables, and compass-needles, into the arena of controlled experimentation, perhaps the most obvious trend has to do with the shrinking size

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of the PK target. When J. B. and Louisa Rhine salvaged PK from the darkness of séance rooms and noisy tables, they introduced light, dice, and quarter distributions of the half-set.

Yet despite the “overwhelming” evidence for PK provided by the quarter distribution data, I believe psychokinesis might have died prematurely in the late 1940’s had it not been for the work of W. E. Cox. Ed Cox nurtured PK research for nearly two decades. He replaced the dice with, among other things, BBs running down paper straw pathways, and his research provided some of the first really strong evidence, not only for the occurrence of PK, but also for the hypothesis that PK is *goal-directed*.

Helmut Schmidt has replaced Cox’s BBs with electrons and has substituted solid-state data paths for Cox’s paper straws. His electronic random generators show good randomness when run in unattended control checks and highly significant departures from randomness when human subjects attempt to exert feedback-guided influence. Schmidt has found in a series of highly ingenious experiments that the internal complexity of the instrument does not appear to affect the degree of influence, and he too concludes that PK is goal-directed (Schmidt, 1974).

Since Schmidt introduced this line of research five years ago, 16 microdynamic PK experiments have been reported, primarily by Schmidt and his collaborators. The results are summarized in Table 3.

Thirteen of these 16 studies, over 80 percent, yielded statistically significant outcomes. This is 16 times the number of significant studies we would expect on the basis of chance error. However, since data collection is so much faster in this area, let us assume for the sake of extreme conservatism that for each of these reported studies there are 10 which were not reported and which are not statistically significant. That is, let us posit 160 unreported, nonconfirmatory studies. Even if this were so, the replication rate for microdynamic PK would still be highly significant.

Now let us compare this with the new work being reported here at this conference. This year there are seven experimental studies of microdynamic PK, contributed by four different laboratories. Five of these seven studies report significant confirmation of microdynamic PK influence and these positive replications are contributed by three of the four reporting laboratories. This is a 71 percent confirmation rate, is 14 times the number we would expect by chance error, and compares nicely to the 80 percent rate for the earlier studies.

Taking Parapsychology Seriously

It is time we take parapsychology and its phenomena seriously, that we stop being defensive with people who are skeptical but are unfamiliar with the serious literature of this field, its better research, the degree of control, and advances in methodology. I believe that it is time for us to go to the journals such as *Science* and to the funding agencies in Washington and demand that they at least examine our serious literature before they reject our papers and our grant proposals. We can present a strong case for our field and it is time that we do so.

I really wanted to take this occasion to develop some speculative areas, but considering the slender basis of support that is currently sustaining this field,

Report	Experiment	p less than
Schmidt (1970)	Preliminary Series	.12
	Main Series	.00087
Schmidt & Pantas (1971)	Preliminary Series	.0124
Matas & Pantas (1971)	Series I	6.3×10^{-5}
	Series II	.0093
		.0143
Andre (1972)	Experiment I	.1067
	Experiment II	.0093
Honorton & Barksdale (1972)	Group Experiment	.034
	Individual Ss Experiment	.826
	Single Subject Experiment	3.4×10^{-6}
Schmidt (1973)	Exploratory Experiment	5.6×10^{-5}
	Confirmatory Experiment	2.1×10^{-8}
Schmidt (1975)	Experiment I	.00084
Stanford et al. (1975)	Experiment II	.0017
		.0069
Total Significance: chi square = 202, df = 32; $p < 10^{-10}$		

Table 3. Survey of all microdynamic PK experiments, 1970-1975.

I decided upon the present approach. Most of the participants at this conference are here at great personal expense, do not know whether their research will be supported six months or a year from now (if it is supported now), and cannot publish their research in any journal but those that have a circulation of less than 2500.

We must not continue merely bemoaning the fact that parapsychology is not accepted by establishment science; we ought not to feel that, despite the fact that we know our literature and what we are doing in our laboratories, there must be something wrong with the work since it is not accepted; we should not continue to play the game that eventually, after all, science is objective and our findings will eventually become accepted on their merit. I do not believe this. We have been struggling against irrational prejudice for a long time. Patience goes only so far and I think that if the situation is going to change, we are going to have to change it.

Our findings deserve better than they have received from the scientific establishment. If our work is faulty, it should be criticized, but the criticism must be substantive, not *a priori*. The scientific community has an obligation to assess, without prejudice, the serious research in this area. The only way in which this can be done is through dissemination of research reports to a wide scientific audience. As the *Nature* editors suggested,

this would have the effect, not of providing an endorsement of any claims, but rather of stimulating critical discussion and further replication. I can see no basis of justification for the refusal of journals such as *Science* to accept research reports of good quality.

Subliminal Stimulation and Biofeedback

There are some very provocative parallels between two areas of nonparanormal research and the two areas of paranormal research I have focused on in this address.

These areas of “normal” research are subliminal perception and biofeedback. I do not have time to explore either of these in great detail, but I would like at least to highlight a few points that I think are of special interest.

I think anyone who is familiar with the internal states work in parapsychology and who has read Norman Dixon’s (1971) careful survey of the subliminal perception literature must be struck by the parallels. Both subliminal and psi influences are facilitated by internal attention states, both are subject to subtle experimenter effects and situational factors, and both involve the transformation and mediation of stimulus influence through ongoing mentation processes. Perhaps the similarity is no more than skin deep, but the point I wish to emphasize now is

that we can begin to exploit these similarities to advantage in using subliminal versus psi comparisons. Those of us who have been involved in free-response psi research have not had any real basis of comparison in terms of how strong our psi mentation-target correspondences are relative to strength and quality of correspondences in a weak sensory setting. A beginning has been made in this direction (Smith, Tremmel, & Honorton, 1976), and to the extent that the same kinds of stimulus distortions and transformations occur in *both* subliminal and psi tasks, we can have greater confidence in the informational characteristics of our psi findings. In fact, I suggest that this may be a useful prototype for a new methodology for studying psi processes, one that moves us away from a strictly theoretical baseline in assessing the significance of psi results.

The implications of biofeedback for parapsychology have usually been discussed in terms of developing biofeedback shortcuts to psi-conducive states. I agree that this is potentially very important, but I would also suggest that what has been referred to as the *goal-directedness* of psychokinesis is found also in biofeedback in the form of *passive volition*. Robert Thouless (1951) described the mind-set associated with his own success in PK dice experiments in the late 1940’s as “effortless intention to succeed.” This de-

scription would immediately be recognized by contemporary biofeedback researchers as passive volition. Elmer Green (1973-4) has described passive volition with an analogy to farming. He says, "A farmer (a) desires a crop, (b) plants the seed, (c) allows nature to take its course, and (d) reaps."

We are puzzled by the fact that a subject can psychokinetically influence a process, which he knows nothing about solely on the basis of intention to succeed and the guidance of a peripheral feedback display. Yet this is exactly what occurs in biofeedback. Let us take the example of single motor unit control. The motor unit is the functional unit of striated muscle. It consists of the nerve cell body, located in the spinal cord, its axon, terminal branches, and all the muscle fibers supplied by these branches. A single spinal motor neuron may supply anywhere from a half-dozen to several hundred muscle fibers. It is now known, chiefly through the work of John Basmajian (1972) that a human observer can learn to isolate and control single motor units using sensory feedback techniques. This despite the fact that most subjects do not even know what cells look like or where they are located. Here is Basmajian's (1969) summary of his results:

"...normal human beings can quickly, in a matter of 15 or 20 minutes, be trained to isolate only one motor unit from the popula-

Has science developed the competence to confront claims of the paranormal? I believe the most satisfactory answer to Nature's question is this: we have begun to develop competent approaches to the paranormal. We have developed at least a few approaches that work well enough to allow us to build upon them.

tion of perhaps 100 or 200 which are within an area of pick-up of an electrode pair. They can suppress all of the units, fire single units, they can manipulate those units, turn them on and off easily, they can suppress the one they started with, pick up another one, train it, suppress it, turn to a third and then, on command, they can respond with signals from the unit that you choose for them to respond with."

I think it is safe to say that ten years ago the voluntary control of a single cell would almost certainly have been regarded, along with psychokinesis, as a paranormal

claim. And perhaps it is. Basmajian and a number of other biofeedback researchers have resurrected the concept of volition as a metaforce to account for this type of control. Is it possible that PK has always been closer to us than we've realized? This is, for the present, farfetched speculation. We now have the methodology and the beginnings of a technology, however, to begin asking such questions on an empirical basis.

Normalization of the Paranormal

Has science developed the competence to confront claims of the paranormal? I believe the most satisfactory answer to *Nature's* question is this: we have begun to develop competent approaches to the paranormal. We have developed at least a few approaches that work well enough to allow us to build upon them. The job will not be done until we have succeeded in eliminating the "para-" and have normalized these phenomena. This will require further articulation of the positive attributes and antecedent conditions of psi, as well as the determination of the role of psi in our normal life experience. We are beginning to make progress in this direction, on several fronts, and I have tried to indicate a few of these.

I believe we should give up our "para-" terms, even the one that serves to identify this Association. Psi phenomena are as

relevant to physics and, I suspect, to neurophysiology, as they are to psychology. We are studying psychophysical interactions, and I would suggest that the term *psychophysics* more properly identifies the range of activities we have called parapsychological.

My own inclination, which is provisional and subject to modification as a result of new data, is that we are, along with physicists and neurophysiologists, on the threshold of a new scientific enterprise, that we are beginning to do some serious psychophysical reality-testing, perhaps to erect a bridge connecting the perennial dualities of mind and matter, physics and psychology. We are, I believe, dealing on an empirical basis with what Eddington called “mind-stuff” and what we have found thus far supports the notion that mind is a real force in nature. I suggest that in the years ahead it will be useful for us to reconsider the type of theory proposed in various forms by Frederic Myers, Henri Bergson, and Sir John Eccles, that the brain is a *transmitter* of mind rather than its *generator*, and that mind manifests through the brain by psychokinetic influence on neural tissue.

Observation and Participation in Science

In closing, I would like to speak briefly about one other area of psi research, the role of the exper-

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imenter. Experimenter effects represent a sort of skeleton in our collective closet. This is a topic we discuss not infrequently in private, but seldom in public. We are concerned with the possibility that we ourselves, as experimenters may, to some extent, be the source of the psi influences we observe in our laboratories.

Of course, there are different kinds of experimenter effects: from experimenter fraud on one end of the scale (as we learned last summer) to experimenter psi on the other end of the scale. I think it is interesting to note that many of the successful experimenters in

psi research have also been successful subjects. Both J. B. Rhine and Robert Thouless were able to demonstrate PK effects with dice. The French pioneer in the cognitive study of psi, René Warcollier, was successful as a percipient in telepathy-oriented drawing experiments, and so on. Among our currently active experimenters, William Braud was one of the most successful receivers in the Braud's work on psi and progressive relaxation. Rex Stanford was successful in an EEG/telepathy-oriented study with Ian Stevenson. Robert Morris showed that it is possible to gain nonrandom entry points in tables of random numbers using a complex psi procedure. (Morris's studies on cognitively complex psi provide further evidence for the goal-directedness of psi.)

My own accomplishments as a subject mainly involve a series of PK experiments that I reported several years ago (Honorton & Barksdale, 1972). Helmut Schmidt lent me a manual random number generator and since I was teaching at the time, I decided to do a group experiment with my students. I formulated the hypothesis, based on Ted Serios, Kulagina, and other macro PKers, that unlike ESP, PK may require a high level of arousal and activation. I had my students alternate PK runs in which they tensed and relaxed. The results were very nice: the muscle tension runs were associated with significant psi-hit-

ting. The overall results were significant and this was due to the contribution of the muscle tension condition. I returned to the laboratory very pleased with myself for having confirmed my hypothesis. I then set Warren Barksdale to the task of replicating this experiment with 20 individual subjects, following the same procedure for relaxation and tension. His experiment yielded only chance results. We then decided to run a third experiment in which I would be the solo subject and Warren would be the experimenter.

I tensed and relaxed, and we alternated which of the two display lights was target for an equal number of runs, and so on. The muscle tension runs were about 3.5 standard deviations above chance and the relaxation runs were about 3.5 standard deviations below chance. Again, my hypothesis had been confirmed. The question that now arose in my mind, however, was who were, or was, the subject in that first successful experiment?

Recently, my very good friend John Stump really threw me a curve. When my study dealing with another type of experimenter effect, involving the demeanor of the experimenter as an influence on subjects' psi performance, appeared (Honorton, Ramsey, & Cabibbo, 1975), John examined the control data. In this study, there were two samples of "control random checks." These were

run by me, manually depressing the random generator response buttons and attempting not to exert a psi influence on the instrument. Sample A was collected before the experimental data and Sample B was collected after the experimental data. Taken separately, each sample provided good randomness. What John Stump found, however, was that looking at the difference between the two samples, there was a significant difference for each of the four analyses. Since this was a two-choice generator, we looked for sequence effects: how many times did "red" follow "red"; how many times did "red" follow "green"; "green" follow "green"; "green" follow "red"? In each of these four cases, a nonsignificant positive or negative deviation in Sample A was followed by a nonsignificant deviation in the opposite direction in Sample B. The overall result of this was associated with a probability of 10^{-5} . The experimental hypothesis itself was only confirmed at the .001 level.

John Stump went a step further. He used this post hoc analysis as a pilot study. His confirmation consisted of going back to an earlier study, which I had reported with Malcolm Bessent as subject (Honorton, 1971). Again there were pre- and post-experimental random checks; and again the randomness was good within each sample, but with deviations in opposite directions, so that the

difference between pre- and post-test deviations was again significant ($p = .002$).

It appears that John Stump has confirmed Jule Eisenbud's hypothesis about what he has colorfully termed the "individual mind-prints" of the experimenter. I frankly do not know what to do about this problem and I am sure that most of you don't either. But we had better start giving serious consideration to these experimenter psi effects and, to show at least that we are not alone, I will close now with a comment from the distinguished Princeton physicist, John Archibald Wheeler, which I think is pertinent to this:

Insofar as we've learned to understand the quantum principle, it's the small tip of an iceberg that tells us that the momentum and the position of an electron are not qualities that exist independently of us, but depend upon our consciously making a decision to measure the position and the momentum in order to bring these features into evidence. I think that through our own act of consciously choosing and posing questions about the universe we bring about in some measure what we see taking place before us. Therefore, I think the word 'observing' is inadequate. A better word is 'participation.' We are going to come to appreciate that the universe itself in some strange way depends upon our being here for its properties (Wheeler, 1973, p. 32).

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Daryl Bem

Describe your career in psi research and why did you get into it?

I am currently in the advanced Eriksonian stage of psychosocial development informally known as the “drooling-into-the-gruel” stage. In contrast to many of my similarly-situated peers in parapsychology, however, my psi-research age is quite recent, reaching back only to 1994, the year that Charles Horton and I published a review of his autoganzfeld experiments in *Psychological Bulletin*.

Two distinct paths led to my late-life interest in psi, one personal and one professional. The personal path began at age eight when I received a magic set from my aunt and uncle and became an amateur magician (aka “Daryl the Great”), performing at children’s birthday parties for \$5 per performance—more if I helped the hosts clean up after the party. As I moved into adolescence, I began to specialize in “mentalism,”

magic tricks designed to simulate psi phenomena. (Some readers of *Mindfield* may be aware that Russell Targ was also a young mentalist during that same era.)

I have maintained this interest ever since, performing both close-up and stage mentalism up through my professional years as a college professor. I was also a member of the professional “Psychic Entertainers Association” (PEA) along with psi-skeptic Ray Hyman and psi-agnostic Marcello Truzzi. (As Truzzi’s polls of the PEA membership revealed, knowing how to produce fake psi does not predict beliefs in genuine psi.)

My professional path to psi research began in college: I obtained a B. A. in physics at Reed College in 1960, and then went on to MIT to seek a Ph. D. in that field. At the time, MIT was interested in “educating the whole man”—yes, they meant man—and encouraged graduate students to have minors unrelated to their majors. MIT had an exchange agreement with Harvard (two



Daryl Bem, Cornell University

subway stops away) so that graduate students could enroll in courses offered at the other institution without additional tuition charge. The civil-rights movement was becoming an active force in the United States at that time and I was fascinated by it and the resultant changes in public opinion toward racial issues. So, I declared social psychology to be my minor field and enrolled in a Harvard

course called “The Psychology of Race Relations” taught by Professor Thomas Pettigrew. Pettigrew’s course was a life-changer for me, and I decided to switch fields as soon as feasible. The Woodrow Wilson Foundation, which was supporting my first year at MIT, gave me permission to change fields at the end of the Fall semester, taking whatever courses I needed to prepare myself for my change of field. I re-applied to graduate schools and enrolled in the Social Psychology Doctoral Program at the University of Michigan, receiving my Ph. D, three years later in 1964, specializing in public opinion and attitude change. I also became a civil-rights activist at Michigan, joining the Congress of Racial Equality (CORE). I was arrested in a peaceful demonstration in Ann Arbor the same year that Bernie Sanders was arrested in a CORE-sponsored demonstration in Chicago. I was fined \$5 for “loitering.” My mother was convinced that I would never be able to get a job after that.

My first academic job was a joint appointment in the Department of Psychology and the School of Industrial Administration at Carnegie Institute of Technology in Pittsburgh, which became Carnegie Mellon University soon thereafter. In addition to publishing in the usual academic journals, I also published a brief textbook, *Beliefs, Attitudes, and Human Affairs*, in 1970, and testified before the US Senate Judiciary Committee about how making false confessions might affect the memories

My involvement in psi research began in 1983, when parapsychological researcher Charles “Chuck” Honorton invited me to his newly-established laboratory in Princeton, New Jersey [...]

of criminal suspects. My wife, Sandra Lipsitz Bem, and I were invited to join the faculty of the Psychology Department at Stanford in 1969; and 10 years later, we joined the faculty of the Psychology Department at Cornell University. During these years, I conducted research in several areas of personality and social psychology, including group risk-taking, self-perception, the continuity of an individual’s personality through time and across situations, and the development of sexual orientation. Sandra and I were also activists in the Feminist movement during the Seventies, doing studies that showed how sex-biased job advertisements discourage women from applying for jobs for which they are well qualified. We also served as expert witnesses in legal proceedings involving cases of job discrimination.

My involvement in psi research began in 1983, when parapsychological researcher Charles “Chuck” Honorton invited me to his newly-es-

tablished laboratory in Princeton, New Jersey, to critically examine his autoganzfeld procedure from the perspective of both an experimental social psychologist and a practicing mentalist. That collaboration resulted in the publication of our 1994 *Psychological Bulletin* article: “Does psi exist” Replicable evidence for an anomalous process of information transfer.” Sadly, Honorton died of a heart attack at age 46, only 9 days before our article was accepted for publication. He had encouraged me to run an autoganzfeld study of my own at Cornell and to join the Parapsychological Association, both of which I did.

In 2000, I began my own research on precognition at Cornell, presenting progress reports at PA conventions in 2003, 2005, and 2008. The final report, “Feeling the future: Experimental evidence for anomalous retroactive influences on cognition and affect,” appeared in the *APA Journal of Personality and Social Psychology* in 2011. To encourage external replications, I made replication packages publically available; and in 2015, Patrizio Tressoldi, Thomas Rabeyron, Michael Duggan, and I published “Feeling the future: A meta-analysis of 90 experiments on the anomalous anticipation of random future events” in the open access journal *F1000Research*. The experiments in the database came from 33 laboratories in 13 countries and produced an overall effect greater than 6 sigma.

These experiments were quite

controversial and were widely discussed in both professional journals and websites. They were also publicized in the mass media and in popular science publications such as *New Scientist*, *Discovery*, *Wired*, and *Psychology Today*. Ironically, one of the most intelligent media discussions of the research emerged from my appearance on the humorous Colbert Report. Clearly the pinnacle of my career! The experiments were also incorporated into the recent discussions concerning replication issues, meta-analyses, Bayesian vs. frequentist statistics, the need for pre-registered studies, and the prevalence of Type I errors in scientific and medical literatures.

There was even a circulating rumor, based on my career as a performing mentalist, that my research was part of a strategy to embarrass the field of psychology by subsequently revealing either that my experiments were a hoax or, alternatively, that by strictly following the standard, prevailing experimental and statistical procedures of psychological research, one could demonstrate something even as far-fetched as psi.

How do you see the field now as compared to the beginning of your career?

Unlike most of my age peers in parapsychology, I did not personally experience the history of parapsychological research during the 20th

Century. In reading about that history, however, I have been impressed with the sophistication displayed by many of the earlier pioneers. I also appreciate how parapsychologists were virtually forced by skeptics to invent methodological safeguards even before more traditional disciplines had come to grips with the need for them (e.g., parapsychology's invention of the double-blind study). It seems to me that this continues today: Parapsychologists are among the first to adopt new procedures, safeguards, and statistical procedures. Many of my parapsychologist associates are among the smartest and sophisticated scholars I have met in my professional career. Not unexpectedly, they are also more open to a more radical view of reality than most of my university colleagues, with clear disagreements among them on fundamental epistemological and methodological issues. In short, I find parapsychology intellectually very exciting, redolent of my days as an undergraduate at quirky Reed College 60 years ago.

Were you to start again, what would you focus on? Where do you think the field should go?

I am still close enough to the beginning of my work in psi that I doubt I would change much in retrospect. I still find precognition the most intriguing of psi phenomena precisely

because it most challenges our intuitions and simple physical models of how it might operate. I tend to agree with those of my colleagues who believe that precognition may be the fundamental psi process, and recent conferences between physicists and psi researchers over the nature of time reversal and causality have inspired me to refresh my now-outdated understanding of quantum mechanics.

At the personal level, I am still most comfortable and competent with designing and conducting laboratory-based experiments. Similarly, my belief in the reality of psi is based on experimental data rather than on personal psi experiences of my own or informal accounts of apparent psi reported by others. But I do not advocate this as the only model for parapsychological inquiry. The success of parapsychology in both theory and application lies in the diverse phenomena it seeks to address and the methods it employs to encompass that diversity. I agree with my colleague Stephan Schwartz that the future success of parapsychology is likely to be indexed by the degree to which it ceases to be a distinct field and becomes instead a force for promoting the consideration of psi phenomena in many other disciplines.

Any regrets or other things you would like to add?

No.

The Star Gate Program

The U.S. government's military and intelligence communities funded anomalous cognition (AC; aka informational psi, remote viewing, extrasensory perception) and psychokinesis (PK) applications and research for 23 years at a total level of support of about US \$19,443 million. In the early years, it was known as a "psychoenergetics" program, in keeping with the Soviet term for the phenomena. Although small in terms of what is usually budgeted for traditional programs within these organizations, such funding represented the largest support in the history of the field of parapsychology.

The *raison d'être* of Star Gate was primarily to determine

whether the intelligence alluded to by the Soviet Bloc research in this field posed a national security threat. If so, the first and foremost question was: are such capabilities real and implementable? All aspects of the research and applied program were in support of this primary mission. Did the Star Gate program satisfy this objective? Yes, and here is why.

- Between the SRI and the remote viewing (RV) operations group at Ft. Meade, a total of 504 separate intelligence missions were tasked by a variety of agencies that required 2865 individual remote viewings to accomplish the stated missions.
- Of the 19 client agencies



| by *EDWIN C. MAY, Laboratories for Fundamental Research*

between 1973-1995, 17 were returning customers for the RV-HUMINT collection product—an 89.5% customer return rate. It seems highly unlikely that there would be such a high customer return rate if the RV-HUMINT information were not worthy of such attention.

Another way to answer this ques-

tion would be to query each tasking agency as to the mission-by-mission assessment. These data were not available. A third way to answer this question would be to provide such an assessment by outcome measures that might be contained in the CIA declassified data release. But only a small number of such assessments were released. Perhaps this is so because, in the tasking agency → collector → analyst → tasking agency cycle it is rare that the collector receives any direct feedback as to the success or failure of any given task. Until 1985 when the US Army Medical Research and Development Command issued a 5-year, \$10M contract to SRI, there was little research activity. Since the funding communities were satisfied with the intelligence product, they were mostly uninterested in how psi worked.

Some application-oriented qualitative research was conducted; that is, what are the parameters to find better remote viewers, what are the best protocols for good production, etc. Also, when the intelligence communities learned of psi activity in the former Soviet Union, other East Bloc nations, and the People's Republic of China, the Star Gate researchers were asked to determine the degree to which these claims were true.

Over the years, SRI and SAIC had a total staff of 20 full time people, nine consultants and two visiting scientists. Additionally, nearly \$700,000 USD were

awarded to subcontractors many of whom were in the psi research arena. The overall research direction, which was even specified by the Central Intelligence Agency as early as 1975, was to examine the physics of the phenomena—how is it possible for information to transcend space and time—and what are the characteristics of individuals who may have a natural gift. These investigations involved cognitive psychology, physiological, and personality variables. Often Star Gate is generally considered a CIA program; however, the CIA was responsible for only 1% of the Star Gate funding.

Uncharacteristic of such a small US Government-funded program, Star Gate captured the attention and direct involvement of very senior individuals across the government. These included, US Presidents and Vice Presidents, directors of the CIA, Defense Intelligence Agency, many elements of the Department of Defense, and members of important committees in the Senate and House of Representatives.

In a government-requested meta-analysis, the results of the SRI research from 1973-1988 were:

- Remote viewing (RV) can provide useful intelligence information.
- Laboratory and operational remote viewing show the greatest potential for practical applications.
- Experienced viewers are significantly better than the general population.
- Approximately 1% of the gen-

eral population possesses a natural remote viewing ability.

- Remote viewing ability does not degrade over time.
- At this time, there is no quantitative evidence to support a training hypothesis.
- Natural scenes are significantly better than symbols as targets for remote viewing.
- Remote viewing quality is independent of target distance and/or size.
- There is no evidence to support the idea that a psychoenergetic interaction with the physical world exists.
- Electromagnetic shielding is not effective against psychoenergetic acquisition of information.
- A potential central nervous system correlate to remote viewing has recently been identified.

For all the Star Gate years, the program was overseen by independent scientists, medical people (IRB), and policy experts to assure the quality of the work. The details of the program will be available in four large volumes called the *Star Gate Archives*. Volumes 1 and 2 of this archive contain the entire research reports in remote viewing from 1972-1995; the psychokinesis research, presented in Volume 3 of this archive, found insufficient evidence for the support of the mind-over-matter hypothesis; Volume 4 contains memorandums, letters, reports, and reviews originated by government personnel.

K. Ramakrishna Rao

Succinctly describe your career in psi research and why did you get into it?

A major part of my mature professional career has been devoted to parapsychological research. My entry into parapsychology was prompted by the manifest incompatibility between classical Indian psychological thought and the then current psychological ethos dominated by the behaviorist tradition, and my perception that parapsychology could be an appropriate bridge between the two – a bridge to connect the Brahman with behavior, as it were. However, my tryst

with parapsychology under the tutelage of J.B. Rhine brought about a kind of metamorphosis, transformation of a tender minded philosopher into a tough empirical researcher. It took me several decades to finally return to my roots.

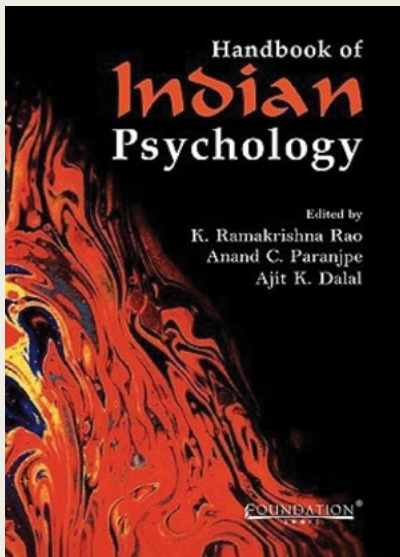
How do you see the field now as compared to the beginning of your career?

When I entered the field, I had great hopes that parapsychology would have reasonable answers to many of the riddles that puzzled and brought me into the field. After 50 years of close involvement, there is some kind of



K. Ramakrishna Rao

disillusionment. I have become increasingly conscious that we are on a dead-end road. This should not be construed, however, to mean that parapsychology has made no progress at all and



that all its past was a wasted effort. Rather, the massive data it has accumulated with much rigor and great tenacity remain data that raise many questions but provide few answers. What I am looking for are answers. They are simply not there. I need to turn elsewhere looking for reasonable explanations.

Were you to start again, what would you focus on? Where do you think the field should focus?

If I were to start again, I would first look into my own tradition. This would naturally lead me to Yoga psychology, what it can tell us about resolving the

If I were to start again, I would first look into my own tradition. This would naturally lead me to Yoga psychology, what it can tell us about resolving the above referred riddles and puzzles.

above mentioned riddles and puzzles. The goal would not be to collect evidence for psi, but to explore ways of cultivating psi so as to manifest it in our behavior more reliably. Applied psi would be my priority. We need to focus our research on how psi manifests in and guides our real lives so as to gain insights into ways of cultivating it. There is in my view little that connects the current research results with psi in real life. This needs to be bridged by more relevant and ingenious methods that judiciously bridge the divide between life and the lab.

Any regrets or anything else you would like to add?

I have no regrets for spending so much of my time pursuing parapsychological research. It helped me to grow in many ways, and to appreciate better the nuances of science and especially the nature of and problems involved in pursuing research in controversial areas. I wish to add a line of advice to those thinking about entering a field of study like parapsychology. Be fully prepared to face challenges, frustrations, and unanticipated hazards; and at the same time seek satisfaction in the fact that there is a wholesome purpose behind your difficult quest to explore the unknown and non-natural. That should give you a peculiar joy that has its own charm.

Jean Burns

| by ETZEL CARDEÑA,
CERCAP

I met Jean Burns in the 1980s in one of the wonderful conferences on shamanism organized in California by my vibrant friend Ruth-Inge Heinze. The two could not be more different, Ruth-Inge an anthropologist with a background in performance and a “no holds barred” intellectual attitude, Jean a shy and gentle soul with physics degrees from the University of California, Berkeley, and the University of Hawaii. Jean listed her interests in the PA webpage as: “parapsychology and the

relationship of psi phenomena to presently known physical laws; consciousness and free will and their relationship to presently known physical laws; thermodynamics and the nature of entropy” (<http://www.parapsych.org/users/jeanbur/profile.aspx>), in which she listed dozens of her publications. She was the associate editor of the *Journal of Consciousness* and is probably best known in parapsychology for co-editing *Psi Wars* (Alcock, Burns, & Freeman, 2003) and proposing physics models to explain psi phenomena (e.g., Burns, 2011). She also wrote a book on a topic that I do not



think has received the attention it deserves by the field, namely attempts to make psi phenomena more controllable (Burns, 1972).

Born on April 4, 1932, in Dresden, Germany, Inge Strauch (IS) studied from 1952 to 1956 psychology at the University of Freiburg im Breisgau. In the same year she became a research associate of Professor Hans Bender (1907–1991), the pioneer of academic parapsychology after WWII, who had founded in 1950 his independent “Institut für Grenzgebiete der Psychologie und Psychohygiene” (IGPP) [Institute for Border Areas of Psychology and Mental Hygiene] at Freiburg. As part of the IGPP research into “mental hygiene,” IS started with an empirical investigation into the activities of a German “spiritual healer” by determining the sociological make-up, expectations, and attitudes of a group of 650 patients who came voluntarily to see this healer at the IGPP. For this pioneering work she got her Ph. D. in 1958 (for an English overview see Strauch, 1963). During this time, IS discovered the major scientific challenge of her life, doing experimental work into dreams and dreaming in connection with altered states of consciousness. In 1961 and 1962, supported by a generous grant from the Parapsychology Foundation (PF), IS conducted EEG research on dreams at the Mount Sinai Hospital and on the neurological basis

Inge Strauch

(1932–2017), a German Pioneer of Experimental Dream Research

of psychic phenomena at the PF headquarters in collaboration with W. Grey Walter. In 1962, she spent several months at Rhine’s lab in Durham, NC, doing quantitative ESP research. IS has left an unpublished delightful diary of her private experiences at Rhine’s lab and her encounters with the social and cultural life in Southern US in the early 60s. During these years, she wrote for the German Journal of Parapsychology several reports on the early PA Conferences held in New York (Strauch 1960/61; 1962/63). Back in Germany, she continued with a “normal” academic career in psychology at Freiburg University. In 1968 she did her habilitation thesis on experimental sleep and dream research (“Investigations on Experiences during Sleep”) and became in 1970

| by *EBERHARD BAUER, IGPP, Freiburg*

a professor of psychology at the University Saarbrücken where she stayed until 1976 when the University of Zurich offered her a chair in clinical psychology. She occupied this academic position until she retired in 1999. During these years IS published, together with her students, numerous empirical studies on the phenomenology of dreams. In her book *In Search of Dreams*, written with her collaborator Barbara Meier, IS analyzed 500 REM dreams, collected in her Zurich sleep laboratory, in reference to sex differences, children’s dreams, and waking fantasies (Strauch & Meier, 1992). Of special interest for her was the longitudinal development of quality, quanti-



Inge Strauch in the role of the agent in a telepathic dream experiment (credit: Leif Geiges, c IGPP Freiburg).

ty, and habits of sleep, the subject of her last book (Strauch, 2010). On August 24, IS died in Zurich at the age of 85. She presented at two PF Conferences on alterations of consciousness two methodological papers on how to influence the content of dreams telepathically (Strauch 1970, 1978). For many years, she was also a member of the IGPP council supervising and stimulating experimental work into altered states of consciousness.

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Discussions of Survival of Death in the Psi Encyclopedia

Those of you familiar with the *Psi Encyclopedia* know that this is a free online reference work about parapsychology sponsored by the [Society for Psychical Research](#). The project, edited by Robert McLuhan, began in 2014 and it is work in progress. Due to the magnitude of information to be covered it is estimated that the main topics will be adequately covered by the year 2020 (for information about the purpose and scope of the project see <https://psi-encyclopedia.spr.ac.uk/about>).

One of the main areas represented to date, and the topic of my comments, includes some of the entries relevant to survival of death. This includes essays about phenomena, cases, mediums, and ideas. In addition to presenting information about the topics covered, I will suggest further additions with the understanding that the encyclopedia is ongoing

with current plans to enlarge it considerably.

Overview of Survival Evidence

A general introduction to the topic appears in philosopher Stephen Braude's article [Postmortem Survival](#). As stated by Braude: "There is undoubtedly no shortage of cases at least superficially suggesting postmortem survival. But for all of these there are non-survivalist explanatory options which any clear-headed appraisal of the evidence must consider seriously and which those favoring survivalist explanations must strive to rule out." Explanations that need to be considered include conventional ones such as inaccurate observation and psychological processes such as dissociation and cryptomnesia.

In addition, Braude considers psi and acknowledges the controversy that exists on this issue



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between survivalists and non-survivalists (see Braude's [The Super Psi Hypothesis](#)). One of the things I like about Braude's take is that he mentions that many who reject the living psi hypothesis arguing that it takes an unprecedented degree of high psi functioning fail to consider that "survivalists are committed to positing comparably impressive psi on the part of the deceased or the living, simply in order to explain how evidence

suggesting survival was manifested in the first place.” Although I do not pretend to solve the debate here (and I have much sympathy for the survival agency), I think points like this remind us of the complexity of the subject, which Braude explores in the rest of his essay. It is important to examine the assumptions of both camps and then focus on the best cases. Braude gives good summaries of investigations and cases, but I wish he had devoted more space to more detailed descriptions of this material. For example, a reader not familiar with the Piper or Leonard mediumship will have difficulties getting a flavor of the actual material and why it impressed researchers in the past. Other entries in the work may help with this, especially as the project develops further.

Reincarnation Cases

The most frequently discussed survival topic in the encyclopedia to date (August 2017) is that of research with children who claim to remember previous lives. A good place to start is psychiatrist Jim Tucker’s *Past Life Memories Research (Overview)*. He writes: “The children say that they remember experiencing various events from the past, and they have associated emotions that are appropriate for the deceased

individual in whose life those events occurred. Taken at face value, these cases indicate that some children remember lives they experienced in a previous incarnation. Fifty years of research has produced significant evidence that supports this.”

Other essays are about more specific topics. Tucker has an article about [Birthmarks](#). James G. Matlock has detailed entries about [Experimental Birthmarks and Birth Defects](#), [Patterns in Reincarnation Cases](#), [Replacement Reincarnation](#), and [Xenoglossy in Reincarnation Cases](#). Matlock’s contributions are among the best articles on the topic in the encyclopedia, being both open to reincarnation and critical at the same time. The latter is shown in the following comments in the xenoglossy article: “Usually it is only a few words that are spoken, and when responsiveness in conversation is attested, the level of proficiency is low. An important caution is due here, though. Although the testimony seems sound in these cases, in most of them investigators had no opportunity to observe the subject speaking, and only rarely have voice recordings been made for evaluation later. Much more work must be done before we can be confident that responsive xenoglossy is possible, but these spontaneous cases suggest that it is, and that makes the xenoglossy

of the regression cases more plausible.”

Other topics reviewed by journalist Karen Wehrstein are [Adult Past Life Memories Research](#), [People Who Knew Each Other in Past Lives](#), and [Twins Reincarnation Research](#). Erlendur Haraldsson summarizes his research in [Psychological Studies of Children Claiming Past-Life Memories](#). I believe this is one of the most interesting areas of reincarnation research. Different from most other survival (and reincarnation) research that centers on veridical phenomena to show that there may be a surviving self that can present evidence of identity, this work, of which Haraldsson is probably the leading researcher (e.g., Haraldsson, 1997), is an attempt to connect cases to psychological

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aspects of the children, expanding our understanding of the phenomena in different directions. Haraldsson writes that he found that some of the children who remembered other lives had more behavior problems (argumentative, quarrelsome) than a comparison group. Also, in a study of children from Sri Lanka he found that: "The great majority expressed general happiness with how their lives had developed. Their educational level was higher than that of their generation... They seemed to be living normal productive lives... On the whole the data showed that they had fared reasonably well in life, apparently somewhat above the average for their age group in Sri Lanka."

The topic of hypnotic regression is covered by Melvin Willin in [Past Life Regression](#). The author states: "In combination, the obscurity of the information,

realistic presentation of character and emotional identification on the part of the subject have convinced many people of the reality of reincarnation. Alternative parapsychological explanations include ESP and exposure to the memories of deceased humans. Some narratives have been traced to printed sources, suggesting the presence of cryptomnesia... There remain serious reservations about the authenticity of memories recovered under hypnosis, especially given the phenomenon of false memory syndrome."

There are several entries about reincarnation cases. A long, and useful list of cases taken from Ian Stevenson's writings is presented by Robert McLuhan in [Children Who Remember a Previous Life](#).

Also interesting is Wehrstein's [Famous Past Life Claims](#) and Matlock's [Reincarnation Accounts Pre-1900](#). These articles give a good idea of the variety of individuals that say that they remember previous lives. In addition, there are summaries of specific cases authored by Haraldsson ([Dilukshi Nissanka](#); [Nazih Al-Danaf](#); [Pretiba Gunawardana](#); [Purnima Ekanayake](#); [Thusita Silva](#); and [Wael Kiwan](#)), McLuhan ([Kumkum Verma](#); [Jagdish Chandra](#); [Ratana Wongsombat](#); and [Sunil Dutt Saxena](#)), and Wehrstein ([Imad Elawar](#); [James Leininger](#); and [Titu \[Toran Singh\]](#)).

Haraldsson's case of [Dilukshi](#)

[Nissanka](#) is one of a few cases in which there was a written record of the information the child gave before the case was formally investigated. "Before she was two years old Dilukshi Nissanka began to speak about a previous life in Peravatte in Dambulla, some 80 miles distant, which ended when she drowned in a stream. She refused to address her parents as mother and father, and requested to be taken to her previous mother." Of at least 22 statements made by her, most were verified." Twelve of the statements were completely or partially correct, 4 were wrong, and the other 6 were ambiguous.

Mediumship

To date there are only a few entries about mental mediums. They include Guy Lyon Playfair's [Chico Xavier](#), Michael Tymn's [Leonora Piper](#), and Willin's [Rosemary Brown](#). There are two articles by Tymn and Braude about Pearl Curran: [Patience Worth \(Pearl Curran\)](#) and [Patience Worth \(analysis\)](#).

The article about Piper presents a good broad overview of this important case of mediumship. Several classic investigations are summarized, among them those of Richard Hodgson. Tymn writes about the "George Pelham" (George Pellew) communicator: "Hodgson noted that the communicator appeared to be lifelike. He

also remarked that among a total of a hundred and fifty individuals whom he arranged to sit with Piper, from the time of Pelham's first appearance to the time of writing, as many as thirty had been known to Pelham: in each case Pelham recognized them and greeted them by name, with one single exception, a woman who had been a child when Pelham knew her. Pelham did not, however, recognize, or greet by name, any of those whom he had not known. Hodgson argued that this was consistent with what would be expected if Pelham had survived death and found himself able to communicate."

An interesting contribution, and one not focusing on survival, is Everton de Oliveira Maraldi's essay about [Hélène Smith](#) (whose real name was Catherine-Élise Muller). She was mainly investigated by Théodore Flournoy, who documented and interpreted in psychological terms her communications about previous lives in India and France, as life on planet Mars. As Maraldi stated about Flournoy's work: "For him, Hélène's romances were products of a subliminal imagination, their content based on her previous memories and experiences, incubated and creatively combined in the subliminal regions of her mind." One thing I like about this article, and which I would like to see in other entries, if relevant,

is that Maraldi connects the work Flournoy conducted with this medium with what has been called "the evolving field of subliminal psychology" (Miranda, 2016, p. 29), that is, the developments about the capabilities and functions of the unconscious mind that were developing at the time, and that greatly influenced both psychology and psychical research. Furthermore, Maraldi has rightly stated, in my opinion, that: "Hélène Smith illustrates the influence that exemplary cases may have on the development of research programs, especially for its implications for the then-emerging fields of psychical research and dynamic psychiatry..."

On a different issue, it is important that cases and arguments like Flournoy's, which are not supportive of survival, also appear in the encyclopedia. The importance of this for psychical research was pointed out over a hundred years ago by James H. Hyslop (1906). Fortunately discussion of conventional explanations has not been neglected in other articles, such as those about Rosemary Brown and Leonora Piper.

Various other topics are also discussed, as in the entries about [The Cross-Correspondences](#) (Trevor Hamilton) and [Book Tests](#) (Tymn and McLuhan), and specific cases and sittings (Tymn's [The Junot Sittings \(Leonora Piper\)](#); Tymn's [Raymond](#)). The latter

refers to communications purported to come from Raymond Lodge, the son of Oliver Lodge, who died in World War I in 1915. These mediumistic communications formed the basis for the famous book *Raymond, or Life After Death* (Lodge, 1916).

Raymond was a book that fulfilled various roles. Lodge's book was an expression of the hardships of war time, and more particularly, of personal and national bereavement in England (Kollar, 2000). It was to some extent a response and a message of comfort for people to deal with the great amount of loss of life due to the war. More relevant to psychical research, *Raymond* presented evidence for veridical mediumship, and included Lodge's discussion about various aspects of the process of communication that are not discussed in the entry probably, and understandably, to keep it short. In these comments Lodge was also critical of mediumistic descriptions of spirit life in the beyond. He wrote: "I should think, myself, that they are of very varying degrees of value, and peculiarly liable to unintentional sophistication by the medium. They cannot be really satisfactory, as we have no means of bringing them to book. The difficulty is that Feda [the medium's control] encounters many sitters, and though the majority are just

inquirers, taking what comes and saying very little, one or two may be themselves full of theories, and may either intentionally or unconsciously convey them to the 'control'; who may thereafter retail them as actual information, without perhaps being sure whence they were derived. Some books, moreover, have been published of late, purporting to give information about ill-understood things in a positive and assured manner, and it is possible that the medium has read these and may be influenced by them. It will be regrettable if these books are taken as authoritative by people unable to judge of the scientific errors which are conspicuous in their more normal portions; and the books themselves seem likely to retard the development of the subject in the minds of critical persons" (Lodge, 1916, p. 192).

I am sure that other articles about specific mental mediums will appear in the near future. There is certainly not a shortage of relevant mediums, among them: Geraldine Cummins, Ermance Dufaux, Eileen J. Garrett, Gladys Osborne Leonard, Hester Travers Smith, Minnie Soule, and Rosina Thompson. There is also a need for more discussion of specific topics. Because the entries for the encyclopedia focus on the old literatures, there is a need for comments about modern mediumship research, which has been

reviewed by Beischel and Zingrone (2015). Further topics that may be added are xenoglossy via mediums, mediums that play musical instruments and that produce drawings and painting without training, and mediumistic literature.

Apparitions

The topic of apparitions only has a few entries, but the encyclopedia has three good overview articles. These are Peter Hallson's [Ghosts and Apparitions in Psi Research](#)

There is certainly not a shortage of relevant mediums, among them: Geraldine Cummins, Ermance Dufaux, Eileen J. Garrett, Gladys Osborne Leonard, Hester Travers Smith, Minnie Soule, and Rosina Thompson.

([Overview](#)), Braude's [Theories about Collective Apparitions](#), and Haraldsson's [Deathbed Visions Research](#). There is also a summary of the [Cheltenham Ghost](#), authored by the SPR, I assume McLuhan.

Other Topics

Several other topics are included with information relevant to survival of death. This includes near-death experiences (Nancy Evans Bush's, [Distressing Near-Death Experience](#); Penny Sartori's [Near-Death Experience](#)), possession (Wehrstein's [Shiva-Sumitra; Sharada](#)), terminal lucidity (Michael Nahm's [Terminal Lucidity](#)), and transplant cases (Braude's [Transplant Cases Considered as Evidence for Postmortem Survival](#)). Other relevant articles are Douglas Stokes's [Physicalism and the Soul](#) and Patricia Pearson's [Phenomena Relating to Danger, Death, Bereavement](#).

Much about survival is found in entries about various individuals. Some of them are Alvarado's [Ernesto Bozzano](#), and [Charles Richet](#), Braude's [C.J. Ducasse](#), and Tynm's [Richard Hodgson](#). With one exception (SPR's [Erlendur Haraldsson](#)) there are no entries about modern researchers. There is also material relevant about survival in my comments about a book by Bozzano: [Ernesto Bozzano's Phénomènes Psychiques au Moment de la Mort](#)

([Psychic Phenomena At the Moment of Death](#)). I hope that future articles will focus on many other important works.

Other Additions

In addition to more discussions of phenomena favorable to the survival interpretation, more articles about specific alternative explanations of the phenomena are needed. Although the topic is not ignored, it would be useful to have long entries about these explanations. An example would be an essay about cryptomnesia such as Stevenson's (1983) systematic overview, which perhaps could be reprinted in the encyclopedia. The same may be said about personation dissociative phenomena. An entry presenting a good overview of the topic of personation in hypnosis and various cases of secondary personalities would inform readers that such personalities can be very realistic, even when there is no reason to consider them to be spirits of the deceased (see Braude's entry [Mediumship and Multiple Personality](#)).

Although survival can be a polarized issue, with some positions bordering on fundamentalism, I find that the articles in the encyclopedia tend to be balanced. Even when their authors favor a particular view, they do not fail to mention the opposite argument. In this way the encyclopedia is

effective as a reference source in presenting information about the variety of existing theoretical issues about survival. Because this is a work in progress it is not hard to find topics that need to be included. They include articles about specific phenomena in particular time periods, countries or cultures, an example being Matlock's [Reincarnation Accounts Pre-1900](#). Similar entries should be written for topics such as apparitions, mediumship, and NDEs. Similarly, overviews of the writings of influential individuals about survival, such as those of J.B. Rhine and Ian Stevenson, would be welcome. Furthermore, it could be very informative to have entries comparing different approaches to the study of survival.

Concluding Remarks

I have offered various suggestions for new entries in the spirit of offering constructive suggestions for future developments, but I recognize that it is all too easy to point at omissions in a publication like the *Psi Encyclopedia* when the overall area is so multifaceted and the project is, of course, ongoing. Its editor, Robert McLuhan, is actively working to increase the number of entries by qualified individuals not only in the area of survival of death but also in the rest of parapsychology. This is a

very time consuming task and one, I believe, that is going well.

Acknowledgement

I wish to thank Nancy L. Zingrone for editorial suggestions.

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Ostensible Clairvoyant and Precognitive Experiences Related to Air Crashes

Sylvain Ephimenco, a French writer and journalist who lives in the Netherlands, reported in the newspaper *Trouw* his nocturnal impressions related to Germanwings Flight 4U 9525, that crashed March 24, 2015, in the French Alps, 100 kilometers north-west of Nice. This disaster with an Airbus A320-211, which resulted in the death of all 144 passengers and six crewmembers, was deliberately caused by co-pilot Andreas Lubitz, who had been under psychiatric treatment. Ephimenco's newspaper article appeared on March 26, two days after the crash (Ephimenco, 2015).

Ephimenco described his nocturnal impressions of the crash thus:
"It is roughly four o'clock in the

morning, the day before yesterday, when I feel a hand on my forehead and hear a voice saying to keep calm. Calm? I just escaped dead, surrounded by an inferno and bombarded by pieces of wreckage. I am going to sit at the rim of the bed and try to clear my thoughts. 'Suddenly you became very restless and loudly screamed 'oh no' says the voice next to me.

I can tell her precisely why I shouted this. I am regularly plagued by nightmares, but this one was very realistic. Furthermore this anxiety dream had two special characteristics: it contained both colors and sounds, whereas I always dream in shades of grey without hearing anything.

I was just now in Aix-en-Provence, where I have lived for years, in the company of my elder



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brother. The white room in which we stay has huge windows and a glass ceiling. This room with ideal view looks mostly like an aquarium or a control tower. Suddenly I hear from afar a humming sound. The ominous humming that approaches turns into the sound that can be produced by a defective clothes dryer. It comes closer and closer and the noise transforms in

an unbearable uproar: humming, hissing and the squealing noise that ten braking lorries at high speed would make together. A shadow darkens the glass ceiling totally. I look upstairs and shout to my brother 'it is an airplane!' I see the shape that buzzes slowly over the glass ceiling. A hulking and raving colossus with underneath the hull and at the ends of the wings flashing blue and red lights. In that moment I think very clearly: the airplane is in nosedive and is almost crashing on us. At less than hundred meters from the 'aquarium' the machine disappears with a thunderous explosion in a blaze. I shout 'oh no!' and try to find shelter behind a pillar. Time to feel a soothing hand on my forehead.

Approximately eight hours later the first messages seep in about the crashed Airbus A320 that was on its way from Barcelona to Düsseldorf. Flight 4U 9525 from Germanwings flew against a mountain. 10 minutes earlier it passed by Aix-en-Provence at an altitude of 11,500 meters. At that moment it started its fatal descent. The machine had its last radio contact with the air traffic centre of Aix at 10.30.

What do I want to say or prove with this? Nothing at all. I am not able to predict the future and I am not a paragnost, and totally not Lienneke van den Hoek who on 4 October 1992 called Schiphol to report her vision. She warned of a

crash and later that day an El Al freight airplane fell on the Bijlmer.

I just had a bad dream and wanted to get rid of a deep-rooted feeling of displeasure that haunted me since the day before yesterday, by writing."

Near the end of his story, Ephimenco mentions Dutch homemaker Lienneke van den Hoek, who predicted the crash of a Boeing 747 cargo aircraft (El Al Flight 1862), into two apartment buildings in the Bijlmermeer neighborhood of Amsterdam on October 4, 1992. This accident resulted in a total of 43 deaths according to officials: the crew of three persons including captain Itzhak Fuks, one passenger, and 39 people on the ground. The actual number of victims might have been higher because many residents lived illegally in this neighborhood. After ten years of investigating different aspects of this crash, Pierre Heijboer (1937-2014), who lived in Bijlmer and was a journalist of the newspaper *De Volkskrant*, wrote a voluminous book in which, among other things, he discussed Van den Hoek's predictions and their reception (Heijboer, 2002). In his book, Heijboer described how Van den Hoek was lying alone, reading in her bed in her Hilversum home, between 22 and 23 pm of Saturday, October 3, 1992. After about 10 minutes of reading she experienced a terrifying vision, seeing a blue and white Boeing 747 drilling through the wall, with glass,

Near the end of his story, Ephimenco mentions Dutch homemaker Lienneke van den Hoek, who predicted the crash of a Boeing 747 cargo aircraft (El Al Flight 1862), into two apartment buildings in the Bijlmermeer neighborhood of Amsterdam on October 4, 1992.

bricks and metal flying all over. After her vision ended she knew that, as before, friends of "the other world" wanted to show her things that were going to happen. When she was quiet again she saw an apparition of a man who told her: "You have to warn them Lienneke, what you just saw will happen soon, tomorrow". Getting out of bed, Lienneke went downstairs, to write down: "Schiphol" (the Amsterdam airport, 30 kilometers from Hilversum), "airplane crashes," "problem hydraulic system," "blue and white colors," "17.30," "pilot Fuck."

The next day Lienneke telephoned Schiphol airport, where Ine van Veen from the information desk took the call and received her warning with all the aforementioned details. Van den Hoek emphasized her seriousness,

requested to pass on the information to somebody in command, left her telephone number, and asked to be called back. She did not dare to mention the name of the captain, “Fuck,” that she had heard in her vision (his name turned out to be Itzhak Fuks). Ine van Veen passed the information on to the airport police. But Lieneke received no return call from Schiphol airport. So a few hours later she called again, and telephone operator Karin de Jong took her call. De Jong: “In the afternoon that lady called again, apparently because she had not been called back. She told the same story she had given to Van Veen who had spread the word ... That lady predicted a calamity with a blue and white plane, caused by a fault in the hydraulic system... My colleague passed this information on to the military police and it would happen at 18.35”. De Jong answered that the information had already been passed on: “Your information is known to the persons who need to know it ” (Heijboer, 2002, p. 39).

That Sunday evening a bewildered Lieneke saw on her TV images of the terrible Amsterdam crash site and was angry that she had been unable to prevent a disaster. The plane from El Al had made a stopover at Schiphol on its way from JFK airport New York to Ben Gurion International Airport in Israel, and crashed fifteen minutes after it departed from Schiphol (it was scheduled to depart at 5.30

pm, but its departure was delayed until 6.20 pm). In his book, Heijboer describes how Van den Hoek’s warning had reached the airport police (Rijkspolitie Dienst Luchtvaart at Schiphol) but they could not prevent airplanes from taking off, because more than half of all airplanes in the airport including those of KLM, El Al, and other airliners, were blue and white. Press officer A. van der Bent from airport police (Rijkspolitie Schiphol) stated: “Afterwards we profoundly discussed if we should have acted on the information which that lady gave to us on October 4. The result was that we justly had not taken action” (Bent, in Klinken, 1993). But later, officials who were intrigued by Van den Hoek’s warning contacted her, among them the press officer of the state aviation police Alek de Jong. (Heijboer calls the press officer Alek de Jong, and he is called by journalist Leo de Ruiter “H. de Jong.” De Ruiter was the first to write about the air crash predictions that Lieneke made from her Hilversum home, under the pseudonym of Carel Elias. Lieneke van den Hoek had cooperated with De Ruiter under condition of anonymity, so instead of using her name, De Ruiter wrote “male psychic H. W. from Amsterdam”). After his visit to Lieneke, Alek de Jong thought it wise to send a professional to find out if she could contribute to the investigation of the air crash. (Heyboer, 2002).

Alek de Jong paid her a visit and he thought it wise to send a professional to find out if she could contribute to the investigation of the air crash (Elias, 1993, who mentioned the “male psychic H. W.” to maintain Van der Hoek’s anonymity). It was decided that she would be visited by psychologist Jan Smit, manager of the Nationaal Lucht- en Ruimtevaart Geneeskundig Centrum at Soesterberg (National Medical Center for Aviation and Space Travel). On November 12, 1992, Smit visited Lieneke at her home and recorded the conversation. He was impressed by credible details of the flight given by Lieneke that were only known to investigators who had seen data from the digital flight data recorder. He did not believe his ears and asked her if

He was impressed by credible details of the flight given by Lieneke that were only known to investigators who had seen data from the digital flight data recorder. He did not believe his ears and asked her if she would agree to participate in a meeting in a flight simulator at Schiphol.

she would agree to participate in a meeting in a flight simulator at Schiphol. Van den Hoek agreed and then “went into trance.” She told the astonished psychologist that on December 21, another Dutch airplane would crash in Gent, Belgium (Van Klinken, 1993). The location Lieneke predicted was wrong, but in the morning of 21 December 21, 1992, Martinair flight 495, a DC-10 that came from Schiphol, crash-landed in severe weather conditions on Faro Airport, Portugal, leaving 54 passengers and two crew members dead. Martinair is a Dutch airliner.

At first, leading investigator H. N. Wolleswinkel would not give permission for the use of a flight simulator, which would cost 6000 guilders per hour, to reconstruct the flight that ended in the Bijlmer crash. Finally he agreed because his boss, Minister of Transportation Hanja Maij-Weggen, was very interested because she had had psi experiences during childhood.

In the evening of March 18, 1993, in the hall for flight simulators at Schiphol, Lieneke met Huub Corsmit, captain of a Boeing 747, a flight engineer, aviation psychologist Jan Smit, and Leo de Ruiter, reporter of the magazine *Paravisie* whom she had requested to come along. Van den Hoek went “into a trance” and subsequently mentioned a keyboard that the flight engineer used. She furthermore described a handle near the feet of the co-pi-

lot that was utilized for bringing pedals closer so that the co-pilot could exert more force. Van den Hoek provided the correct wind direction and wind velocity for the phase of the flight that was reenacted, and illustrated actions of the crew members in their last minutes. After the session Lieneke asked the psychologist if her contribution would be of any use. He answered: “I do not know yet. We will have to see.” Smit, a member of the Christian Reformed Church told a journalist: “I came in contact with things that I want to keep at a distance henceforth” (Smit, in Klinken, 2002). He talked on TV in 2002 about the data on wind direction and wind velocity that Lieneke provided: “That made an enormous impression on me, because nobody can know this” (<https://www.youtube.com/watch?v=8RQah3ttndc>). But the reconstruction of the flight in the simulator was not successful according to press officer Henk Houtman from the Ministry of Traffic and Water Management (Houtman, in Klinken, 1993).

Several questions and uncertainties with regard to the circumstances of the Bijlmer air crash remain unsolved. Many persons living near the crash site reported physical problems and there were rumors that chemical substances were part of the cargo of the crashed El Al airplane. On April 22, 1999 the final report of a parliamentary inquiry was presented,

The story of the prediction of the Bijlmer crash by Lieneke van den Hoek almost looks too good to be true. One should consider the possibility that Heijboer might have made up essential parts of it or exaggerated them.

in which Van den Hoek is not mentioned at all. Rob van Gijzel, former member of parliament, who investigated the Bijlmer crash for years, called Heijboer’s book thorough and substantiated. Although not all assertions in the book can be proved, Heijboer provided some new evidence or earlier assumptions (Van Gijzel, in Van Engelen, 2002).

Heijboer took Lieneke van den Hoek very seriously. Once, he asked her to tell him something about himself. She said: I see a grandmother with a small boy and a sheep in his hands. Heijboer recalled he had a photo of himself with a lamb in his hands and looked for it in his home. He found a photo of himself as a boy with a sheep in his hands (Klinken, 2002). Of course a lot of persons from rural areas could have been photographed during their youth with a sheep. Pierre Heijboer, a son of a miner, was born and spent his

youth in Hoensbroek, in the Dutch province of Limburg, where sheep herding was not uncommon. So this vision could have been a calculated guess.

The story of the prediction of the Bijlmer crash by Lieneke van den Hoek almost looks too good to be true. One should consider the possibility that Heijboer might have made up essential parts of it or exaggerated them. But there is independent corroboration of some key facts. Journalist Gijs Zandbergen received confirmation from Schiphol telephonist Karin de Jong that she and her co-worker had received the warning calls the day before the air crash. Zandbergen was employed as a journalist by the newspaper *De Volkskrant*, as was Pierre Heijboer. Could they have fabricated or antedated Lieneke's phone calls? This seems unlikely, because they would have had to collaborate with the two telephonists and the airport policemen who received their messages on the same day. High ranking officials of Schiphol and official investigator H. Wolleswinkel had been informed of the calls by airport police, and decided it could be useful to engage the help of Lieneke for further investigations into the causes of the crash. They considered their sources of information credible enough to spend a considerable amount of money to hire a flight simulator for recon-

struction of the flight under the guidance of Smit.

How reasonable is the hypothesis that psychologist Jan Smit lied or had been wrong about his memories? He was a highly competent witness who, because of his affiliation to the Christian reformed church, was generally averse towards paranormal phenomena. Smit gave consistent testimony in December 1993 when he was interviewed by journalist Jan Klinken for the newspaper *Reformatisch Dagblad*, and in October 2002, on television in *Netwerk*. In the months after the two crashes Lieneke made several other predictions to the investigators. Smit was critical and remarked that she made so many predictions that half of all planes from Schiphol would have crashed if these later predictions had come true. The psychologist proved to be a person of high ethical standards, trustworthy and critical. His confirmation of Lieneke's predictions and clairvoyant perception of the correct wind direction and the wind velocity during the short flight of the crashed plane was not given lightly.

We come to a final evaluation of the strength of Van den Hoek's two air crash predictions. Notwithstanding the parapsychological relevance of the predictions, their practical usefulness was limited. If Lieneke had not predicted correctly the time of the crash, the colors, and technical defects

of the El Al plane, one could have agreed with Van Klinken's conclusion that: "The first prediction, of October, 4 could still be explained by considering it a lucky guess .. (but) the second prediction, of the Faro disaster makes the affair quaint. Although the prognosis was unsound, the date was right" (Van Klinken, p. 16).

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Anomalistik. Geschichte und wissenschaftstheoretische Grundfragen

(Anomalistics: History and Scientific Basic Questions), pp. 11-30.

Gerd H. Hövelmann (†)

In this article, an overview of the history and theoretical scientific fundamental questions of the research field of anomalistics is given. After discussing the use and demarcation of the term anomalistics, the importance of the sociologists Roger W. Westcott and Marcello Truzzi for the newly developing research field is pointed out. According to Truzzi, anomalistics basically fulfills four functions: (1) supporting the scientific assessment in the context of diverse alleged anomalies; (2) contributing to a better understanding of the process of scientific assessment; (3) developing a conceptual framework for categorizing and assessing alleged anomalies; 4) assuming

the role of a neutral party in the process of assessment by the scientific community. The last paragraphs are dedicated to the discussion of the legitimacy of anomalistics as well as its benefits for science.

Forschung zum Problem der “denkenden” Tiere. Die Fortschreibung einiger alter Fehler

(Animal “language” research. The perpetuation of some old mistakes), pp. 31-51.

Gerd H. Hövelmann (†)

In this article, different approaches to the investigation of the capacity of animals to achieve higher cognitive abilities, and particularly linguistic capacities, are presented and critically discussed. Thereby, contemporary, almost exclusively US ape language research is compared with animal language experiments conducted during the first three

by GERD H. HÖVELMANN (†)

decades of the 20th century in Germany, mainly with horses and dogs. No substantial differences can be identified with regard to the methodological quality as well as the dubiousness of the interpretation of the data. Instead of an expectable advancement or improvement, the flaws, fallacies, and other shortcomings are perpetuated, and the then mentioned reasonable criticism largely ignored. Effects of experimenter expectations on the animals' productions as well as various immunizing strategies with regard to the achievements of the animal subjects are only some of the problems that still characterize this kind of research. Accordingly, the presented findings have little meaning with regard to the assessment of higher cognitive capabilities of animals.

Remote Viewing: Eine Proof-of-Principle-Studie (Remote viewing: A proof-of-principle study), pp. 83-104.

Maximilian Müller and Marc Witmann

This is a proof-of-principle study of the remote viewing method, which is based on the original protocol of the Star Gate program financed by the American military and conducted between 1978 and 1995. In our study 36 randomly selected participants were first stimulated with a ganzfeld device and then participated in a remote viewing session. Solely by relying upon their spontaneous perceptions, they were asked to describe and draw sketches of one of six photos (the targets) enclosed in an envelope. Following this session the six photographs were openly presented and participants were asked to form a correspondence/confidence rating related to their own records. Overall, 14 of 36 participants judged the photo, which was actually in the envelope, with the highest confidence and correctly identified it. Regarding a binominal distribution and an expected value of 6, the probability for such an event happening by chance lies at $p < 0.0009$ and is therefore highly significant. Distribution of individual rank orders of the target also showed a highly significant

deviation from the expected value, therefore, we discuss whether an anomalous information transfer occurred.

Wie aus Protyposis Energie, Materie und Bewusstsein entsteht. Eine Theorie zur Erklärung des Aufbaus der Welt sowie parapsychologischer Phänomene

(How energy, matter, and consciousness develops from protyposis. A theory of explanation of the structure of the world and of parapsychological phenomena), pp. 105-123.

Stephan Krall

A physical explanation of the connection between mind and consciousness with matter has not yet been conclusively achieved. The article describes a new monistic view of the world based on the research of Thomas Görnitz following Carl Friedrich von Weizsäcker's Ur-theory. The substance on which being is based is the protyposis, which represents the totality of abstract and primarily meaningless quanta bits (AQIs). The quanta of energy and matter are formed from the protyposis that has arisen since the Big Bang. Through the interaction of photons with matter in living structures, abstract quantum information can become meaningful, because life is based on information processing, with which living

beings themselves stabilize. In highly developed brains mind can arise with consciousness. With the help of non-locality in quantum systems, the tunnel effect and the quantum-physical phenomenon of an "extended present" the phenomena of telepathy, precognition and teleportation can be explained.

Das Phänomen der "Kröte im Stein." Eine kommentierte Übersicht über den Diskurs zu einer Anomalie in Deutschland

(The phenomenon of the "toad-in-the-hole." A commented overview of a discourse of an anomaly in Germany) pp. 124-146.

Ulrich Magin

The phenomenon of the "toad-in-the-hole" was scientifically discussed in Germany. Generally, a block of stone is broken in a quarry and a living toad jumps out of a hole in it, seemingly alive after hundreds of years in its natural prison cell. The topic appears far off today, but was of relevance well into the 19th century. These cases enabled informed speculation of the age of the earth, the occurrence of the Deluge, etc. This paper gives an overview of the material published in the 18th and 19th centuries in Germany on this topic.

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