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A Compendium of the Evidence for Psi¹

Adrian Parker
Department of Psychology,
Goteborg University

Göran Brusewitz
Swedish Society for Psychological
Research

Abstract: The article has the purpose of making readily available for scrutiny primary sources relating to studies that give evidence of psi-phenomena. Although the list is not offered as providing compelling evidence or “proof” of psi, it is meant to provide a strong case for a recruitment of resources. The effects are not marginal or non-replicable ones although it is clear that in many cases they appear to be dependent on certain experimenters and participants. The presentation includes sources relating to classical studies, meta-analyses of replication studies, the testing of high scoring subjects, and proof-orientated experiments, as well as the critical appraisal of this work. Where possible, web site addresses are given where this material is available and accessible. Theory driven research is needed especially concerning the nature of the experimenter effect.

This compendium occurred as a result of what is perceived as a need to deal with the persistent claims concerning the lack of evidence for psi and thereby lack of justification for funding research and academic positions in parapsychology. It has been often asserted, sometimes with some measurable effect, that there is no evidence for psi, or if there is, it is so marginal and non-replicable and that resources should be instead allocated to the study of belief in the paranormal. The compendium presents evidence that appears to refute this assertion,

The standard works summarizing this field, Wolman's *Handbook of Parapsychology* and Kurtz's *The Skeptic's Handbook of Parapsychology*, are both long since out of date and out of print. It is true that there are several very readable and informative texts (Broughton, 1991; Edge, Morris, Palmer, & Rush, 1986; Irwin, 2000; Stokes, 1997) but these are not concise sources of works listing evidential publications. A controversial and for some even provocative source of material because of its declared advocacy, is Dean Radin's *A Conscious Universe* (1997).

In compiling this current compendium, it should be immediately said that the list is not intended to convince the reader that psi has been proven. Beside the principle impossibility of *proving* phenomena in empirical science, we believe that in even seeking the final compelling evidence of psi, no matter whether it be through any of the lists of proof orientated experiments, replications, or meta-analyses, is a futile enterprise. The Pratt-Woodruff-experiment is often cited as the classic example of how an experiment which at its time of conception was considered well designed but which later was found to be deficient from the fraud-proof aspect and we have no reason to believe that the best experiments designed today would be convincing for

¹ This paper was reviewed by Dr Joakim Westerlund.

our successors. Indeed some critics such as David Marks and Ray Hyman uses this to discredit the whole field by claiming a ten year life span for experiments, implying that parapsychological findings invariably only last this period until a fatal flaw is discovered. In reality some findings such as the Maimonides Dream–ESP work have lasted a much longer period.

The replication studies and the meta-analyses, which are also listed below, are obviously part of a general pattern of evidence that we are dealing with real effects, but their greatest value is as sign posts for finding the most promising routes towards the goal of understanding the phenomena. In our opinion they again can never be proof or compelling evidence.

Since the phenomena are for most people so alien to everyday reality and an insult to our intellect, it is natural that skepticism should dominate and direct us to find alternative explanations for evidence that poses as being final and compelling. This is a standpoint that we, the authors, wish to share with critics but given the fallibility of our intellect and limitations our knowledge, we would not wish to assert that it is a particularly scientific standpoint. Yet the paradox is that humans – including scientists - seldom make decisions or take standpoints, which are purely, based on a dispassionate evaluation of evidence but rather on our need for creating a stable understandable world¹. One of England’s best known spokesmen for science, Lois Wolpert (2004) expressed this neatly when on being confronted with the evidence for the paranormal, said “An open mind is a very bad thing - everything falls out.” (But then a closed mind could be said to be an empty mind since nothing new comes in.) Clearly these phenomena will only become less provoking for the emotional needs of our intellect when some degree of understanding is reached as to the nature of psi.

Of course there is also the intuitive argument that these phenomena are themselves best understood in altered states and indeed it may well be that individuals who have thin boundaries, are “feeling” orientated in a Myers-Briggs sense, score high on openness to inner experiences and transliminality do show a greater belief or even receptivity to psi, simply because they are able to integrate and understand these psi-experiences in such states (Goulding & Parker, 2000). Indeed it can be said that this is part of the reasoning behind Charlie Tart’s concept of state-specific sciences (Tart, 2000). Philosophically speaking this is actually not so new. Romantic scientists as well as poets valued altered states of consciousness for perceiving the true wholeness of the world and considered the world of reductionism as depriving Nature of its essence (Puhle & Parker, 2004).

However perhaps a generally more acceptable view is the argument put forward here, that the evidential studies would take on a completely new status when a theory of psi is developed which relates the phenomena to other areas of research and makes the phenomena in some measure more understandable. In the crude sense, psi will then be considered “proven”. Consequently the purpose of this compendium is not to convince the skeptic but merely to collate research reports that justify a major

¹ A good example of an emotive view is David Marks new edition (2000) of *The Psychology of the Psychic*. It is difficult to discern the validity of his critique because of emotionally toned personal attacks and vendettas pervade the whole book.

scientific effort towards gaining an understanding of what the nature of the apparent phenomena. For this reason replication and meta-analysis studies are listed in this case, not because of their evidential value per se, but because of their importance in suggesting that progress is being made towards getting a grip on the phenomena. Indeed a positive sign is the ceasefire in the war over psi that the editors of the recent *Journal of Consciousness Studies* chose to depict the July 2003 volume on parapsychology with. The editors declared that the issue is no longer between opponents but about getting a grip on the nature of the phenomena. The argument that a hundred years of research has not achieved this and therefore the phenomena have to be false, is built on a misconception: the research resources of parapsychology are equivalent to a mere two months of research in American psychology and these resources have been nearly exclusively devoted to research aimed at the accumulation of evidence rather than process-orientated research (Schouten, 1998).

The collection compiled below is primarily concerned with laboratory studies and experimental evidence. However this is not meant to deny or downgrade the importance of case studies and some mention of these should be accorded. There would seem to be general agreement that the two most important classical works in the area of psychical research are *Human Personality* by Fredrick Myers (1901/2003) and *Phantasms of the Living* by Myers, Gurney, & Podmore (1918/62). Arguably some of Ian Stevenson's works such as *Telepathic Impressions* (1970) could be added to this list. Scholarly reviews of the field of psychical research have also been written by Donald West, Alan Gauld (1968) and Archie Roy (1996).

Psychologists and skeptics in general readily dismiss the validity of spontaneous cases believing that modern cognitive psychology can deal with these experiences on the basis of selective memory, hallucinations, and cognitive biases. While not denying these theories their validity, in reality there is no better reason for dismissing all the well documented cases of psychical research than there is for dismissing out of hand all human testimony, and indeed some good ones for not doing so. (For further discussion of the limitations of cognitive theory see Parker, 2000, Parker, 2002). Granted this, the greatest value of spontaneous cases is then in showing that psi phenomena are not mere empty statistical anomalies, at best belonging to quantum physics, but that they do have an important psychological content and meaning.

Before leaving classical psychical research, it is worth then mentioning that publications – a hundred years of documented case studies (as well as more cent experimental studies) - are now available on-line for down-loading through the *Society for Psychical Research* at: <http://moebius.psy.ed.ac.uk/~spr/>

One of the first attempts to make a selection of evidential experiments was a privately circulated list by R. A. McConnell in 1975 of 21 research reports. This list covered the work with at the time contemporary high scoring subjects (Stepanek, Muratti, Delmore, and Bessent) the Schmidt work with random number generators, the sheep-goat work (the effect of belief/disbelief in ESP on scoring), and psi-conducive states, all of which appear in the list below. John Beloff published a list of seven evidential experiments in the *Zetetic Scholar* in 1980. This included the classical Brugman experiment, the Stepanek work, the Schmidt work and the Delmore work. My esteemed colleague Nils Wiklund was inspired by this to attempt

to set up crucial evidence in order to try to achieve some consensus amongst former PA presidents as to the best *seven* experiments offering evidence of psi. By reducing the issue to the absence or presence of flaws in these experiments, Wiklund proposed that further scrutiny would then resolve the issue as to whether there was genuine ESP or merely error some place. Wiklund refers in his report to an apparently unpublished list obtained by Diane Robinson in 1981 through contacting all the members of the Parapsychological Association. Her list confirmed Wiklund's but added the Maimonides Dream-ESP studies, the remote viewing experiments, and the sheep-goat studies. The seven studies composing the consensus list that Nils Wiklund arrived at, were as follows: Schmidt's experiments, the Delmore experiments, Stepanek experiments, the Ganzfeld studies, the Brugmans experiment, Pratt-Pierce experiments, and Pratt-Woodruff experiments. (Wiklund, 1984, on-line at: <http://www.psy.gu.se/EJP/EJP.htm>).

What is of current interest from this is that most of these studies would still be quoted as providing strong evidence today and are consequently included in the compendium below but as part of a larger unitary series reflecting more accurately the function of the design. A possible exception might be the Delmore experiments but as we note below these have been defended by no less a critic than Richard Wiseman.

Since the time of Wiklund's survey, the use of meta-analysis has for better or for worse shifted the focus from final proof orientated experiments to the question of whether or not a replication effect has been obtained. The consensus would seem to be that replication has been achieved, but there is still disagreement as to the quality of the replication studies in excluding all sources of error. For this reason, the classical studies, the proof-orientated studies and the testing of high scoring subjects, all are part of the placard of evidential evidence.

Unavoidably the selection below is to some extent a personal one but by building on what has gone before and what is being currently discussed, we believe it has some consensus.

The Early Classical Experimental Studies

The Brugmans Experiments

Originally published in French, the Brugmans experiment on the whole survives still the ten-year rule. Carried out at the University of Gröningen, the selected subject, Van Dam, sat in a cubicle and make his selection of the target symbol, which the sender one floor above had randomly chosen, by reaching out and tapping at the appropriate symbol on a checker board.

Schouten, S. A. & Kelly, E. F. (1978) On the experiments of Brugmans, Heymans, and Weinberg. *European Journal of Parapsychology*, 2, 247-290

Pope, D.H. (1952) The Brugmans experiment. *Journal of Parapsychology*, 16, 1-3

Gardner Murphy provides also a translation and a critical review pointing out some, in his opinion, minor shortcomings in the report:

Murphy, G. (1961) *Challenge of Psychical Research*, New York, N.Y.: Harper & Row. Pp. 56-62

The Experiments of the Duke Parapsychology Laboratory:

The Rhine work done at the Parapsychology Laboratory of Duke University requires special comment since there are so many misconceptions surrounding it. When Rhine reached retirement age, he moved the laboratory to new premises; a move which allowed him to continue as its director and devote the work of the institute along with its recent private donations exclusively to full time research without teaching commitments of Duke. This may have been a major historical mistake: privatizing the laboratory meant not only loss of status; it was to halt the slow process of integrating parapsychology into mainstream psychology. It could also be argued that the publication of research papers more or less exclusively in the *Journal of Parapsychology* rather than in mainstream psychology journals may have further contributed to isolating researchers from their peers. These factors may even have repercussions today and explain why parapsychology has become more established in Europe where it has maintained its bases within universities.

These difficulties, successes, and setbacks have been recorded by two historians (Mauskopf and McVaugh, 1980) at Duke. They report (page 306) that some fifty universities and colleagues had experimented with the ESP-card testing technique developed by Rhine and his colleagues and while some had success with individual subjects, the results were never like the one in five subjects that Rhine had let them to expect. Many mainstream psychologists believed they had provided the right conditions but positive results were seldom forthcoming, and in the wake of this, they simply turned to other things. Open-mindedness may however not have lasted long and Louise Rhine described how many of the graduates and doctoral students belonging to the period when the Parapsychology Laboratory was located at Duke, subsequently left the field because of the prejudice against it meant that a continued association with the parapsychology would have a detrimental effect on their careers (L. Rhine, 1983).

The classical work detailing the experimental achievements of the Duke laboratory until 1940, is:

Rhine, J.B., Pratt, J. G., Smith, B. M., Stuart, C. E., Greenwood, J. A. (1940/1966) *ESP After Sixty Years* Boston: Bruce Humphries

And the two most cited experimental studies are:

Rhine, J. B. & Pratt, J. G. (1954) A review of the Pearce-Pratt distance series of ESP tests. *Journal of Parapsychology*, 18, 165-177.

Pratt, J, G. & Woodruff, J. L. (1939) Size of stimulus symbols in extrasensory perception. *Journal of Parapsychology*, 3, 121-158.

The Rhine work was severely criticized by Mark Hansel (1980) in his book *ESP - A Scientific Evaluation* which speculated on various scenarios by which subjects and experimenters could have cheated (reviewed in Parker, 1991). However to explain all the results, would require four of Rhine's students and two of his main experimenters to have independently cheated. Moreover the scenario for cheating in the Pierce-Pratt appears to have been based on a disproportionate diagram of the layout of the testing room while the pattern of responses consistent with Hansel's cheating scenario for the Pratt-Woodruff experiment may well have to have a more innocent explanation:

Pratt, J. G. (1976) New evidence supporting the ESP interpretation of the Pratt-Woodruff experiment. *Journal of Parapsychology*, 40, 217-227

Other Classic Experiments:

Russell Targ has edited new editions of some of the classic books detailing the results of the classical ESP-drawing experiments. Most, if not all of these, of these would be deficient by modern standards, which require additional controls especially concerning the random selection of target. However, since they usually allow the reader to make an assessment of the complete series from which targets have been represented, it must be said some of the results are extremely impressive and the conditions for success described there may be instructive for process research.

Sinclair, U. (2001) *Mental Radio* Preface by Albert Einstein. Charlottesville, VA: Hampton Roads)

Warcollier, R. (2001) *Mind to Mind* Prefaced by Ingo Swann, Charlottesville, VA: Hampton Roads

Of the drawing experiments, those of Whatley Carington can be considered the most controlled and some of these do appear to fulfill the modern safeguards and requirements. See for example:

Carington, W. (1941) Experiments on the paranormal cognition of drawings. *Proceedings of the Society for Psychical Research*, 46. 34-151, 277-344
On-line at <http://moebius.psy.ed.ac.uk/~spr/>

Controlled Experimentation with High Scoring Subjects

Although the frequency of high scoring subjects was clearly much less than during the first years of the Rhine laboratory, it is a myth that they entirely disappeared from the scene as a result of the tightening of controls, as the following list of names of those tested and the associated publications testifies to. The list is not meant to be exhaustive but covers the most well known selected subjects:

Michael Bessent:

Honorton, C. (1971) Automated forced-choice precognition tests with a "sensitive". *Journal of the American Society for Psychological Research*, 65, 476-481.

Honorton, C. Precognition and real time ESP performance in a computer task with an exceptional subject. *Journal of Parapsychology*, 51, 291-320.

J. B. Muratti:

Musso, J. R. & Granero, M. (1973) An ESP drawing experiment with a high-scoring subject. *Journal of Parapsychology*, 37, 13-36.

Sean Harribance:

Palmer, J. (1998) ESP and RNG PK with Sean Harribance: Three New Studies", *Journal of Parapsychology*, 62, 112-113.

Alexander, C. (2000) Neurophysiological Research on an Individual Experiencing Anomalous Mental Phenomena: a Case Study. *International Journal of Psychophysiology* 35, (1), Feb 2000, pp 42-43

Joe McMoneagle:

Targ, R. (1994) Remote viewing replication evaluated by concept analysis. *Journal of Parapsychology*, 58, 271-284.

Pavel Stepanek:

Pratt, G. (1973) A decade of research with a selected ESP subject: An overview of research and reappraisal of the work with Pavel Stepanek. *Proceedings of the American Society for Psychological Research*, 30, 1-78.

Martin Gardner (1989) wrote a monograph *How Not to Test a Psychic* where he speculated on the possible means by which Stepanek could have cheated. Pratt was now deceased but one of his closest co-experimenters, Jurgen Keil, wrote a rebuttal.

Gardner, M (1989) *How Not to Test a Psychic*, Buffalo, N.Y.: Prometheus

Keil, J. (1990) How a skeptic misrepresents the research with Stepanek, *Journal of Parapsychology*, 54, 151-168.

Bill Delmore:

Delmore belongs to the more controversial of high scoring subjects since he possessed some, albeit apparently elementary, card skills. Parapsychologist and illusionist George Hansen was of the opinion that Delmore's success could be explained in this way while parapsychologist and illusionist Richard Wiseman concluded after practical experimentation that the proposed method could not have been used:

Kelly, E. F., Kanthamani, B. K. H., Child, I. L., & Young, F. W. (1975) On the relation between visual and ESP conditions in an exceptional ESP subject. *Journal of the American Society for Psychological Research*, 69, 185-197.

Hansen, G. (1992) Criticisms of the research with Bill Delmore. *Journal of Parapsychology*, 56, 307-333.

Wiseman, R. (1995) Testing the notion that a foot shiner could have been used during the Delmore experiments. *Journal of Parapsychology*, 59, 63-65.

Meta-analysis and Replication Reviews

It would be a misconception to present the results of meta-analysis without some form of *quality analysis*, as any kind of hard evidence for the existence of psi but if quality analysis is included then the meta-analysis can certainly become more persuasive as an indication of the degree to which the effect is replicable. The strength of the evidence for a real psi effect and for its replication will then depend on what the results of the **quality analysis** specifically say about a) the controls for the sources of artifact, and b) the use of a homogenous design. Obviously effect size of the psi measure should show either be non-significant or positive relationship to quality of design - it should *not* show a reversed or negative relationship.

Excluded from this selection are some areas (the defense mechanism test, presentiment, sidereal time, and possible environmental and geomagnetic effects), which have been, at least for a period, a focus for research efforts. The reason for this is that there is no consensus as yet as to what the findings mean and as to whether or not they are dependent on the experimenter).

ESP: Forced choice Experiments

A meta-analytic study of forced choice precognition experiment gave an astronomically significant result and there was a large effect size for the studies using specially selected participants. The findings were however found to show a dependence on the experimenter.

Honorton, C. & Ferrari, D. (1989) Future Telling: A meta-analysis of forced-choice precognition experiments. 1935-1987. *Journal of Parapsychology*, 53, 281-302.

Another database was used in order to compare the effect sizes of clairvoyance and precognition testing conditions. This consisted of 31 experiments that were selected for an equivalence of design. No evidence was found for the superiority of either contingency, neither was an influence of quality on effect size found.

Steinkamp, F., Milton, J. & Morris, R.L. (1998). A meta-analysis of forced-choice experiments comparing clairvoyance and precognition. *Journal of Parapsychology*, 62, 193-218.

PK: Forced choice experiments

PK experiments with dice casting collected by Radin and Ferrari, produced initially 148 experimental studies and 31 control studies but only 68 studies had controlled for die bias. The group of 59 statistically homogeneous studies gave a very small effect size of $\pi = .5016$ significant at only $p = .02$.

Radin, D., & Ferrari, D. C. (1991) Effects of consciousness on the fall of dice. A meta-analysis. *Journal of Scientific Exploration*, 5, 61-84

The search for experiments with random number generators during the period 1969-1984 gave 332 a combined p-value of 10^{-43} . Significant differences were found between the experimenters involved:

Radin, D. I., May, E. C. & Thomson, M. J. (1986) Psi experiments with random number generators: Meta-analysis Part 1 In D. H. Weiner & D. I. Radin (Edit.s) *Research in Parapsychology*, 1985, (pp. 14-17). Metuchen, NJ. Scarecrow Press.

The combined results of 597 experimental studies using random number generators, taken from period 1959-1987, when analyzed for binary hits, gave a p-value of 10^{-12} . The control studies (235 studies) confirmed chance expectancy. Despite this confirmatory result, it should be noted that the overall effect was a weak one at 51% instead of 50%. There was no apparent effect of quality of design on outcome:

Nelson, R. & Radin, D. (1989), Statistically robust anomalous effects: Replication in random event generator experiments in *Research in Parapsychology*, edit. L. Henckle & R. E. Berger, 23-26 Metuchen, NJ: Scarecrow Press.

A further data search by Fiona Steinkamp and co-workers was made for experiments using a *concurrent output* of the RNG for the control series; this gave 357 experimental studies and 142 control studies. The Stouffer Z for the experimental studies was 13.09 but when weighted for *study size* became 2.70, $p = .004$, with a very small effect size of $\pi = .50003$. A significant negative curvilinear relationship was found between study size and effect size indicating the effect came from smaller studies. Moreover it was the selected participants who performed significantly better:

Steinkamp, F., Boller, E., & Bösch, H. (2002) Experiments examining the possibility of human intention interacting with random number generators: A preliminary meta-analysis. *Proceedings of the 45th Convention of the Parapsychological Association*, Paris. Pp. 256- 272.

In view of the small effect size, it is worth mentioning that there are some well documented case studies in area of psychokinesis where the large effects have been observed and in which normal explanations appear to be fully controlled for and

found inapplicable (Bender, 1974; Gutierrez, G. (2002); Resch, 1968; Resch, 1969; Gregory, 1985; Roll and Persinger, 2002).

ESP: Free response experiments

The meta-analysis by Julie Milton of 78 free response studies from 1964-1993 gave a Stouffer $z = 5.72$, $p < 5.4 \times 10^{-9}$, one-tailed, and a small (Cohen) effect size of $d = .16$. This was calculated to be lower than that of the mean effect size of the **ganzfeld data base**, $d = .26$, although not significantly so. *Quality analysis* found only one flaw (absence of blind transcription or editing) with a relationship to the effect. Eliminating studies with this flaw, gave $z = 3.83$, $p < 7 \times 10^{-5}$, one tailed. Milton advises some caution in drawing conclusions because the exact outcome measure was rarely specified in advance.

Milton, J. (1997) Meta-analysis of free-response ESP studies without altered states of consciousness. *Journal of Parapsychology*, 61, 279-319.

Distance Mental Influence and Remote Staring

A current meta-analysis of the research design using Distance Influence on Living Systems or for short DMILS - found 36 studies which showed a small effect but significant Cohen effect size $d = 0.11$, $p = .001$. After eliminating some of the earlier studies for being faulted on inadequate randomization and weighing for the influence of study size, the relationship between effect size and *study quality* became non-significant. Nevertheless, the best seven studies did fail to give a significant effect size.

Schmidt, S., Schneider, R., Utts, J., & Walach, H. (2003) Distant intentionality and the feeling of being stared at - Two meta-analyses. *British Journal of Psychology in press*.

The same publication reported a current meta-analysis for the Remote Staring design found 15 studies with a mean Cohen effect size $d = 0.13$, $p = .01$. The collection was homogenous, and there was a non-significant positive relationship between study quality and effect size. Remote staring experiments were however generally rated as having a lower quality than those of DMILS:

Schmidt, S., Schneider, R., Utts, J., & Walach, H. (2003) Distant intentionality and the feeling of being stared at - Two meta-analyses. *British Journal of Psychology in press*.

Rupert Sheldrake provides a strong rebuttal of some of the alleged flaws in current experiments on remote staring:

Sheldrake, R. (2004) The need for open-minded skepticism; A reply to David Marks. *The Skeptic*, 16, 8-13.

Psi and Process Variables

Research efforts into process variables have been primarily at identifying a psi-conducive state and personality variables that might identify high scorers. The first area has given rise to dream or REM periods, the Ganzfeld, and Remote viewing appears to involve a focused, meditative state of consciousness. The latter area has not given much in the way of replicable findings with the exception of belief in psi and extraversion as predictor variables.

1. Altered States: Dream-ESP

The classic collection of the studies carried out at the Maimonides Laboratory in New York from 1962 to 1979, has recently become re-available:

Ullman, M., Krippner, S., & Vaughan, A. (1974/2003) *Dream Telepathy* Baltimore, Maryland: Penguin Books Republished Charlottesville, VA: Hampton Roads. (See Sjödin 2003, in this issue.)

Irwin Child (1985) published an article in which he detailed the false and fictitious nature of many of criticisms leveled against the Maimonides work:

Child, I. L. (1985), Psychology and anomalous observations. The Question of ESP in dreams. *American Psychologist*, 40, 1219-1230.

The Maimonides work is often dismissed for replication difficulties but Simon Sherwood and Chris recently showed the replication attempts have been largely confirmatory. The effect size of the replications however did vary and they did differ from the original work. The Maimonides studies gave on average a medium effect size at $d = 0.33$. The post Maimonides studies had a small-medium effect size at $d = 0.14$. Sherwood and Roe concluded that this difference might be due to the difference in techniques and that the Maimonides studies obtained their highest successes with specially selected individuals. The post Maimonides studies obtained their best results with selected experimenters.

Sherwood, S. & Roe, C. (2003) A review of the Dream ESP studies since the Maimonides Dream ESP studies. *Journal of Consciousness studies*, 10, 85-110

2. Altered States – Hypnosis and Psi

Although significant psi effects are apparent, they are somewhat inconclusive as to whether or not the effect depends on hypnosis.

Honorton, C. and Krippner, S. (1969), Hypnosis and ESP performance: A review of the experimental literature, *Journal of the American Society for Psychological Research*, 63, 214-252.

The above review found 22 studies where there was a waking control group and of these 9 showed significant differences between the two states but in some cases there was a significant negative or *psi missing effect*.

Stanford, R. G. & Stein, A. G. (1994) A meta-analysis of ESP studies contrasting hypnosis and a comparison condition. *Journal of Parapsychology*, 58, 235-269.

The studies with control series included in this review totaled 29, but it was noted that about a third of these had fewer than four participants and the superiority of hypnosis seemed to depend on the particular experimenters.

3. Altered States - Ganzfeld

The Ganzfeld achieved popularity as a design because of its relative convenience (compared to the equipment required for dream studies), its face validity in importing real-life contingencies, and foremost because it was hoped that it would provide a psi-conducive technique relatively free from the experimenter effect.

The current debate, which has threatened to become an “everlasting story”, concerns the question of whether or not a replicable effect has been obtained. The issue is whether it is sources of error or of psi that have been replicated (The debate is reviewed by Palmer, 2003, and by Parker, 2003). This leads onto the key question, highlighted by the findings of Bem, Palmer and Broughton (2001), which is whether or not some of the recent replications failed because they showed lack of quality and homogeneity in following the original design.

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

Milton, J. and Wiseman, R. (1999), Does psi exist? Lack of replication of an anomalous process of information transfer, *Psychological Bulletin*, 125. 387-391.

Storm L., and Ertel S. (2001), Does psi exist? Milton and Wiseman (1999) meta-analysis of ganzfeld research, *Psychological Bulletin*, 127, 424-433.

Milton, J. and Wiseman, R. (2001), Does psi exist? Reply to Storm and Ertel, *Psychological Bulletin*, 127, 434-438

Bem, D., Palmer, J., & Broughton, R. (2001) Updating the ganzfeld database: A victim of its own success? *Journal of Parapsychology*, 65, 207-218.

Some of the above articles can be loaded down, courtesy of Daryl Bem:

http://homepage.mac.com/dbem/online_pubs.html#psi

4. Remote viewing

The first series of remote viewing experiments by Russell Targ and Hal Puthoff produced a controversy in *Nature* as to whether references relating to the previous targets, occasionally present in protocols from sessions, could give cues to the judges

and thereby explain the successes. Removal of these references by their colleague Charles Tart apparently made little or no difference to scoring level but Marks and Scott (1986) insisted there were still be some cues.

Tart, C.T., Puthoff, H. E., & Targ, R. (1980) Information transfer under conditions of sensory shielding. *Nature*, 284, 191.

Marks, D.F, & Scott, C. (1986) Remote viewing exposed. *Nature*, 319, 444.

An excellent BBC recording of the controversy and some live sessions have been made available by Russell Targ and this material will shortly be made available on-line through the Swedish SPR website. <http://parapsykologi.se/>

Although there have been many successful experiments on remote viewing, few could be said to be high quality replications with a methodological stringency. However Russell Targ recently reported a study which apparently fulfilled the criteria of methodological stringency and which obtained a large effect size of $d = .63$, which is consistent with previous replications:

Targ, R. (1994) Remote viewing replication evaluated by concept analysis. *Journal of Parapsychology*, 58, 271-284.

It should however be noted that whatever its nature, the success of the remote viewing technique relies heavily on the availability of specifically selected participants and probably also specifically selected experimenters.

5. Extraversion

A clear difference was obtained between extraversion as related to performance in free response studies compared with forced choice studies, suggesting the effect of extraversion is limited to the free response series. The free response studies had a medium effect size at 0.20. The effect was remarkably homogeneous for the experimenters involved.

Honorton, C., Ferrari, D., & Bem, D. (1998) Extraversion and ESP performance: A meta-analysis and a new confirmation. *Journal of Parapsychology*, 62, 255-276.

A further analysis challenged the conclusion in the above article that the extraversion finding for the forced choice studies was an artifact of filling in questionnaires after the feedback of their ESP scores. When group testing was eliminated, the extraversion effect was found to be of comparable magnitude to the free response studies:

Palmer, J., & Carpenter, J. (1998) Comments on the Extraversion-ESP meta-analysis by Honorton, Ferrari, and Bem. *Journal of Parapsychology*, 62, 277-283.

6. The "Sheep-Goat" Effect

The hypothesis that the dichotomy of belief versus disbelief in psi as measured by the sheep-goat scale predicts the direction of scoring on ESP tests (but obviously can never explain it if all the controls are in place), is supported by the following reviews:

Palmer, J. (1971). Scoring in ESP tests as a function of belief in ESP. Part I. The sheep-goat effect. *Journal of the American Society for Psychical Research*, 65, 373-408.

Palmer, J. (1972). Scoring in ESP tests as a function of belief in ESP. Part II. Beyond the sheep-goat effect. *Journal of the American Society for Psychical Research*, 66, 1-26.

Palmer, J. (1977). Attitudes and personality traits in experimental ESP research. In B. B. Wolman (Ed.), *Handbook of Parapsychology* (pp. 175-201). New York: Van Nostrand Reinhold.

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

7. The Experimenter Effect

Perhaps the most reliable and replicable finding in parapsychology is that of the experimenter effect (Parker, 1978). It should be emphasized for the reader new to the subject that the effect cannot explain the psi-effect if all the controls are in place. The reviews listed below collate that evidence and the limited efforts that have been made to determine its nature:

White, R. A. (1976b) The limits of experimenter influence on psi test results: Can any be set? *Journal of the American Society for Psychical Research*, 70, 335-369.

The effect also appears to occur with other participants involved in the outcome of the experiment:

White, R. A. (1976a) The influence of persons other than the experimenter on the subject's scores in psi experiments. *Journal of the American Society for Psychical Research*, 69, 133-166.

These papers are available on-line at <http://www.aspr.com/jaspr.htm>

White, R. A. (1977), The influence of the experimenter motivation, attitudes and methods of handling subjects in psi test results. In *Handbook of Parapsychology*, 273-301, edit. B. Wolman, New York: Van Nostrand Reinhold.

Smith, M. (2003) The psychology of the psi-conductive experimenter: personality, attitudes towards psi, and personal experience. *Journal of Parapsychology*, 67, 117-128.

Some Well Controlled Proof Oriented Experiments

The RNG experiments by Helmut Schmidt have retained their status and were replicated by him many times.

Schmidt, H. (1969) Quantum processes predicted? *New Scientist*, 16 October, 114-115.

Schmidt, H. (1969) Precognition of a quantum process. *Journal of Parapsychology*, 33, 99-108.

Schmidt, H. (1993) observation of a psychokinetic effect under highly controlled conditions. *Journal of Parapsychology*, 57, 351-372.

Mark Hansel and later James Alcock in a more specific form proposed that Schmidt's results might have been due to his participants capitalizing on local biases in the target sequences. A study by John Palmer analyzed these sequences and rejected this hypothesis:

Palmer, J. (1996) Evaluation of a conventional interpretation of Helmut Schmidt's automated precognitive experiments. *Journal of Parapsychology*, 60, 149-170.

The other main criticism that Hansel (1980) made concerning that Schmidt worked alone, was answered by Schmidt (1993, below) in which his highly successful results were independently observed and replicated.

Schmidt, H. (1993) Observation of a psychokinetic effect under highly controlled conditions. *Journal of Parapsychology*, 57, 357-372.

The carefully controlled success of the experiment together with Morris and Rudolph was initially impressive but it should be mentioned that a later attempt by the authors to replicate this study was unsuccessful.

Schmidt, H., Morris, R. L., Rudolph, L. (1986), Channeling evidence for PK effects to independent observers, *Journal of Parapsychology*, 50, 1-16.

Recently Daryl Bem has presented the results of precognition experiment using the "Mere Exposure" technique under highly controlled conditions. The task was to identify precognitively the preferences that would be later shown for subliminal targets. The results gave the predicted effect and were independently replicated by a sceptical observer.

Bem, D. (2003) Precognitive habituation. Paper presented at *46th Convention of Parapsychological Association, Vancouver.*

Evidence of Psi as Having Communicative Content

Relevant to the debate over whether or not psi is some form of error some place, an anomaly or an unknown form of communication, are the cases where psi appears to be functioning at a level or close to complete information transfer. Rhine reported 3 such cases using ESP cards (where chance expectancy was 5): Linzmayer scored 21 of 25, Pearce made 25 hits in a row correctly, (Rhine, 1950, p. 62, 75); a child subject Lillian obtained a perfect score (Rhine, 1954, p.126), and in the Reiss series the subject averaged 18 hits during 74 runs of ESP cards (Rhine, 1954, p.116). There have been also later cases in the literature (e. g. Parker, 1974) and some collation of these might be informative.

Although most of these were under informal conditions (the scores occurring during spontaneously challenging or playful conditions), whatever the explanation for such scores, it cannot be any minor error, artifact, or aberration in random sequences.

Lower levels of success have also been utilized to show that there is information transfer. For instance by applying the majority vote technique in order to arrive at a consensus target (taking into accounts psi hitting and psi missing), James Carpenter apparently succeeded in transmitting the target word "PEACE."

Carpenter, J. (1975) Toward the effective utilization of enhanced weak signal ESP effects. Paper presented at the meeting of the *American Association for the Advancement of Science*, New York, N.Y: January 1975.

Conclusion

It appears quite clear from the above review that irrespective what interpretation is given to specific research reports, the overall results of parapsychological experimentation are indicative of an anomalous process of information transfer, and they are not marginal and neither are they impossible to replicate. In the face of this, the critic who merely goes on asserting there is no evidence for psi is using a tactic reminiscent of Mohammed Saeed al-Sahaf, Iraq's former information Minister, in blindly asserting there are no American troops in Baghdad.

While the conditions for precise replication and for producing the phenomena to hand, still elude researchers, the psi-effect is replicable to the extent that it permits meaningful and productive research. This fits well with the conclusion reached by Jessica Utts (1996) in her report on government-sponsored research into remote viewing (the so-called stargate project) concerning the existence of a small to medium effect. Both Utts and the other expert involved, Ray Hyman (1996), while disagreeing as to how far research had come, agreed that that future research should be continued and supported by the universities, which has not happened. An argument could be made for psi-research being no more difficult than many other difficult but important fields of human research such as hypnosis, creativity, sexual behaviour, and psychotherapy.

To return to the issue of compelling evidence, the friendly critic James Alcock (2003, p. 48) has vividly argued that if compelling evidence were ever forthcoming “parapsychologists would be knocked over in the stampede by experimental psychologists to explore this new and exciting area of research”. But is this so? During the days of the early Rhine work there was clearly ripple of interest amongst psychologists but hardly a stampede. The Bem and Honorton paper (even before the Milton and Wiseman critique) never produced more than a faint stir amongst experimental psychologists, with not a single replication attempt forthcoming. Compelling evidence would clearly have to be of such a nature that it leads to control over the phenomena in order to produce them on demand - and such control would normally require some theoretical understanding of the phenomena in question.

What may complicate the situation in parapsychology is the dominance of experimenter effects. This is sometimes dismissed as merely a post-hoc explanation for failures but even in successful experiments it is rampant as evident in the above reviews). Then there is also the often muted possibility of that some of the effects may be due to experimenter psi (see White, 1976). Since this is a relatively unresearched area, it does not form part of this review. The upside of all this is that sometimes with the right experimenter, the right participant, and the right technique, the effects are large and demonstrable, and it is under these conditions we should be able to learn something about the process.

In recent correspondence between James Alcock and Adrian Parker, it became apparent that different views of the experimenter effect underlie their different evaluations of the psi-data base: Alcock regards the experimenter effects as an indication of failure to reach objectivity and therefore of an error some place while Parker regards the experimenter effects as occurring as a result of the influence of the experimenter as a catalyst or facilitator. Alcock writes “Parapsychology seems doomed to looking in the rear-view mirror - arguing about the empirical goodness of one study or another - rather than being able to declare: ‘this is what we know - see for yourself’.” Parker also would endorse this need but points out there are windows and maps for driving forward (e, g, Honorton, Ramsey, & Cabibbo, 1975; Parker, Frederiksen, & Johansson, 1997).

In Sweden, communication of research findings to the public is considered to be the universities’ third task. Despite the subtleties involved in presenting the above, dealing with the press media often demands clear and concise replies to such questions as: “Is psi proven?” A reply that could be justified on the basis of the above, would be: “Evidently not - otherwise the question would not be asked - but science has succeeded in lifting the phenomena that the public commonly reports, into the laboratory and imposing checks and controls, and yet the phenomena do appear to persist. Science needs then to move on and answer Yes to the question: Should we now be looking at new and better explanations.”

Inquiries to:

Dr Adrian Parker
Department of Psychology,
University of Gothenburg, Box 500,
SE 405 30 Gothenburg Sweden
Adrian.Parker@psy.gu.se

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