

Is Parapsychology's Secret, Best Kept a Secret? Responding to the Millar Challenge

Adrian Parker

Preprint ID: [m3pParker0429](#)

Submitted: February 11th, 2013

Accepted for Open Peer Review: April 29, 2013

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Abstract: A short overview is made of the attempts to relate the experimenter effect to the conventional theory of social interactive influence. There is however much neglected evidence to suggest it is more than this. The author's own involvement in psi-conducive and psi-mediated experimenter effects is presented. Some of the most striking results in this respect came with the Ganzfeld series of experiments. Examples of psychic experiences in the lives of successful experimenters are given.

Parapsychology is rapidly losing both its viability and vitality. This state is apparently the result of a progressive dislodgement of its university footing, the loss of personnel, the demise of major spokesmen and women in the field, and the loss of financing (Hyman, 2010; Parker, 2012). One of the memorable quotations of John Beloff describes precisely why the problem has arisen: "Rhine succeeded in giving parapsychology everything it needed to become an accredited experimental science except the one essential: the *know-how* to produce positive results when and where required." (Beloff, 1973 p. 291).

This statement was written in the 1970s, and over the next few years it motivated the efforts of some researchers, especially Richard Broughton, Brian Millar and myself, to come to grips with the experimenter effect as underlying the problem. The issue that Brian Millar recently (2012) brings up in his article is whether the experimenter's own psi ability can masquerade as the participant's ability and it is this, which according to Millar, is the root of the problem.

As a student about to begin doctoral studies, I wrote to J. B. Rhine about this issue and I received the following reply:

I do not think the experimenter's own psi ability has shown up to be as an essential factor, but I still think we must assume that every experimenter has the ability and only needs to learn how he himself can liberate and register it. However the very conditions that help the experimenter to liberate this might be the kind that would help him to induce his subjects to perform successfully. (Rhine, Sept. 5, 1975)

During the next decade, we introduced terms such as psi-conducive and psi-inhibitory as well as psi-mediated experimenter effects in order to describe how the experimenter was part of the test situation. Rhea White (1976a, 1976b, 1977) published papers which brought together the research on what we already then knew about the experimenter effects. This evidence indicated that, as well as conventional means of influencing participants through motivation and belief, the experimenter's own psi ability could be a factor. Like placebo effects in medicine, the experimental ritual could be seen as merely a means of engaging our belief systems of the designated "subject" in the experiment in order to make things work. In this case the role of the experimenter becomes a disguise in order to avoid being the identified "psychic".

This latter part of the issue received little attention. Instead most academic parapsychologists in the UK (where they are now mainly located) still today look at psi as being normally distributed and to be studied like any other psychological ability. The experimental work done on experimenter effects is still scanty. There are the much-cited Wiseman and Schlitz experiments (1999), which initially found a difference between Schlitz and Wiseman in their ways of handling participants and the results they obtained from them. However this finding did not replicate. Caroline Watt and Peter Ramakers (2003) did replicate and extent a 1975 study of mine on how experimenter belief in psi influences the results. They looked more closely at how this effect was happening. Failing to find any personality and cognitive explanation, they were unable to decide between whether the effect was caused by experimenter psi, by the believer experimenter eliciting psi from participants, or by an interaction between experimenter psi and participant psi. In other words their preliminary findings were much in agreement with the above quotation from J. B. Rhine.

Arguably the only study to look closely at “the psychology of the psi conducive experimenter” is that by Matthew Smith (2003). Smith was able to confirm that this group of experimenters often had their own psychic experiences and perhaps therefore believed more strongly in the success of their experiments. One of his studies (Smith and Saava, 2008) followed this up using 16 student experimenters who had their belief in psi manipulated, but the significant scoring rate they obtained with auto-ganzfeld did not relate to these manipulated expectancies of the experimenters. It would appear that no attempt was made to assess whether or not the manipulations went against their prior beliefs or not and how this might have affected outcome. More to the point, there was no attempt to see if the successful experimenters could be distinguished by their own psi ability. Since this was for Smith and Saava a rarely successful experiment, we might expect a follow up from it. However it would seem the project remained unfinished because of a bizarre turn of events. Matthew Smith, apparently lured by his own performance in the TV series *Most Haunted*, left academic life with an aim, according to his web site, to become his own media psychic. Even more bizarrely, his co-experimenter, Louie Saava, also left the field and opened a website declaring “everything is pointless” and that parapsychology is a fake science.

Since then the issue has been largely been left dormant until Brian Millar, now returning to field, took up the question with which he left it in around 1980 (Millar 1979). The question being asked by him may reveal one of parapsychology’s best kept secrets and possibly the reason for the continual crisis of academic parapsychology: Are the successes of successful parapsychologists due to their own psychic ability? (Millar, 2012). It is this question that inspires my own return here to the same issue.

What can now be considered as new and innovative is that Brian Millar presents various means - what he calls “forensic means” - by which the identity of the “psychic” or psi-source in a given experiment can be revealed. Some of these “means” - such as Minimum Cross Entropy - seem at least to me rather obtuse, but there are however other clear and precise predictions. One of these predictions is that the psychic experimenter will show mirror effects on the control groups, depressing their scores, as well as elevating those of the experimenter, in the common goal of producing significance. Millar also predicts that the records of experimenters’ psi-based performances will look like “scree jumps” since the experimenter wants to produce at least a .05 level and .04 does not count.

The notion that the psi effect can be holistic and “morph” into different expressions is consistent with the observations in a little known paper titled “Some notes for a future Einstein in parapsychology”, by Rhine’s co-worker, Gaither Pratt (1974). Pratt recommended that attention be given to the whole series, or the experiment as a whole, as the unit of analysis. Although he did not take up the issue of experimenter psi, he did emphasize that we must free ourselves from our preconceptions about the phenomena following the set rules of science.

As for a *background theory* to explain the psi based experimenter effect, Brian Millar advocates a minimalistic form of observational theory, which he, along with Joop Houtkooper and E. H. Walker, developed during the 1980s. The minimalistic aspect refers to the idea that the further the point of observation is from the initial experiment, the more the effect that the observation has on the scores

will decrease. For example, the hands-on experimenter's influence is going to be greater than the reader's, especially when the final results are read as part of a meta-analysis.

All this is daring and radical thinking, but *what evidence does it have?* Let's take observational theory itself. Brian Millar seems to conclude that the contemporary concept of "decoherence" now plays down the role of consciousness in quantum resolution (if I understand it correctly each event is apparently part of a larger influence in the transition towards macro events). For Millar then it is the point of feedback, rather than the observation itself, which he proposes to be the crucial influence.

However others such as Hameroff see quantum decoherence as giving the sudden "ping" of reflective awareness and this occurs as quantum processes in the microtubules in the brain form a concert and play in unison. The immunologist Robert Lanza takes this a step further and sees "meaningfulness" as the true determinant in ordering nature. Meaningfulness is both a process and an attribute of consciousness and the constants of nature (the most well known being Planck's constant) are the glue holding the universe together, or as some prefer, the fine tuning making it function. Constants are however concepts we humans have arrived at through our own conscious representation of the world. Thus from this point of view, consciousness present in various forms throughout nature should by rights place biology rather than physics as the primary science. What I will assert is that psi cannot be lifted out and separated from consciousness as an "ability". It may seem to be largely unconscious, but this is so only in our normal waking state. Other states such as Ganzfeld, hypnosis, meditation, and psychedelic awareness have consistently been documented as enabling access to a wider spectrum of consciousness, including a psi-mode of functioning.

There are some further indications which suggest that observation theory cannot alone account for laboratory successes. There is, as far as I know, no evidence that the successful target sequences used in ESP experiments are suddenly non-random. This would be expected since the various forms of observational theory do propose that psi capitalizes on deviations from randomness. This would either occur directly through PK from the participants (in this case experimenter-psi) causing the sequences to comply with the target guesses or through some form of psi-based intuitive scanning to identify the biased sequences in order to give the correct responses when a bias occurs in them.

Now let's look closely at the main question: *Are parapsychologists psychics in disguise?*

Already as part of my doctoral work (Parker, 1976), I gave a covert ESP test, which I christened (in honor of a helpful friend) the "Falchikov test", to a large number of contemporary parapsychologists. Three judges then blindly assessed the parapsychologists as psi-conducive or psi-inhibitory. The scores on the test the parapsychologists *actually* did show the predicted significant difference favoring the psi-conducives. At the time these results just seemed to me to be too good to be true, so I never formally published the study. Paradoxically or perhaps even irrationally, I began thinking that at worst they were a lucky outcome, or in the best case scenario they were due to my own psi since at that time I was succeeding at every experiment I tried.

But this should not be taken to mean that I entirely agree with the extreme way the challenging question is formulated by Dr. Millar. There are obviously psychic participants other than the experimenters themselves, who seem to be responsible for significant results. In some cases, perhaps in most cases, it may be a joint effort. Indeed Brian Millar may recall that when I was successful at finding two high scorers amongst a group of 25 students, I immediately brought in John Beloff as a witness to the high scores of one of them, who continued to score in his presence but not when he took over as experimenter. The other high scoring participant did however produce high scores in a joint experiment I carried out with Millar. Ironically on this occasion the participant's scores were no longer significant with me as the experimenter, but were significant with Millar as the experimenter. Since it is my understanding that this is the one and only occasion when Brian Millar obtained significant results in psi testing, this once again seems to demonstrate the elusiveness of psi by morphing into another mode of expression. If my own psi played any role, then it may be as Rhine seems to imply - that it enabled potentially high scoring subjects to express their own "ability". Moreover, the personal

history of the two high scorers suggested that they had spontaneous psychic experiences prior to and congruent with their laboratory performance (Parker, 1975).

Another part of my doctoral work critically reviewed the previous work on experimenter effects. I was struck by how the first two volumes of the Journal of Parapsychology contain four reports directly concerned with experimenter effects. At that time, as often now, it was supposed that the experimenter effects were explicable in terms of the social relationships. A major study, which is seldom if ever quoted in the literature reviews because of its enigmatic finding, is that carried out by J. L. Woodruff and Laura Dale (1950) to specifically test the social relationship theory of the experimenter effect. Both participants and the two experimenters testing them made ratings of how warm and friendly they were to each other. Laura Dale's results were non-significant but Woodruff obtained the exact opposite of what had been predicted: Significantly low scores were obtained with high ratings and high scores with low ratings of warmth and friendliness. Somehow it would seem that the results were a weird joint and negative collaboration between Woodruff's psi and the participants' psi. It may seem difficult to understand how this inversion could happen without knowing more about how the experiment was conducted. Since the hypothesis of "friendliness gives psi" was upfront and being tested, it may have lead to unnatural and exaggerated ways of relating. Given his previous track record, it seems likely though that Woodruff's own psi ability had played a role. Perhaps it is best likened to the collaborative work of contemporary artists releasing a striking expression of a form of *negative* creativity found today in modern art and architecture.

I would like now to reveal some of the previously unreported experimental evidence which supports at least a qualified version of Brian Millar's thesis - that experimenter psi is the major interactive factor which explains many of the inconsistencies and elusiveness of the phenomena called psi.

The work referred to earlier (Parker et al., 2000) concerning the qualitative analysis of the "Gothenburg best Ganzfeld hits", contains something that should have been commented on in the reports. About half of these 20 or so best hits presented there came from those trials involving either myself or the two other experimenters who assumed the roles of receivers in the Ganzfeld. Each contributed one session *without having any prior knowledge of the target pool of film-clips*. One of these was Kathy Dalton, who had run the successful Edinburgh Ganzfeld series. Another experimenter-participant was myself and I contributed four trials which were included in one longer Ganzfeld session. The aim here was to identify the randomized four targets and place them in the correct temporal order ($P=.04$). I succeeded at this task, but equally interesting was that all four transcripts showed real-time correspondences with the target clips. The third experimenter involved in producing impressive real-time hits in this report was a former highly successful receiver.

This is not to claim that the Gothenburg Ganzfeld results were merely due to our own psi. Remember, I wrote "about half", so the remainder were due to selected subjects, several of whom, I should add, returned to the lab for second, or even in one case a third session to repeat their successes.

The results are of course relevant to the next question: are we dealing with purely quantum level statistical effects, or the type of observer feedback that form a basis to the Millar theory?

The qualitative analysis of the Ganzfeld work described above, using the recording of hits in real-time, suggests at first glance that we are not merely dealing with statistical anomalies or simple observational effects since participants were describing the external events displayed on the computer screen, sometimes sequence by sequence, just as they happened. One of the best indications of this was when I secretly, in a middle of an on-going session, substituted the sender for a close friend of the receiver. The receiver at that very point in real time remarked "Where have you been?"

Indeed, it appeared that at least for a period, Ganzfeld gave us the much sought-after portal through which we could observe some of the psychological processes showing how real-life psi functions. As I

have documented elsewhere this suggested that so-called *top processes* (the same processes involved in interpreting normal perceptual information) were involved in psi.¹

But the success did not last. Tensions grew within the team, which came to a crisis over the issue of whether or not the primary analysis should be based on subjects' own evaluations of the Ganzfeld mentation or those of a single trained judge. This study run by Anneli Goulding, who had favoured judge evaluation, became one of the last Gothenburg Ganzfeld studies. The outcome was disappointingly not as predicted but nevertheless meaningful as seen in the above context. A statistically significant negative score (so-called psi-missing using the appropriate two-tailed testing) occurred for the receiver evaluations of how their Ganzfeld mentation matched the targets, while the external judge's scores were clearly non-significant. Assuming psi exists, then these results suggest the so-called trickster (Kennedy, 2012) is within us and then both the significant negative finding and the non significant finding are understandable from the conflict loaded issues they related to.

This is surely a pointer as to how the whole situation can influence performance and create what we might call "the morphing effect". Indeed, the expression "morphing" might be a better term for what is usually called "elusiveness" or even "the trickster effect" of psi. Again, assuming psi exists, it may well be the case that it is *neither robust nor an ability* that can manifest in the same form irrespective of the conditions, but rather an expression of connectedness of both conscious and unconscious minds.

If we assume experimenter psi has validity as a factor in the results of experiments then, like participant psi-performance, it may be equally dependent on ambience and enthusiasm. When our public sourced funding was terminated and Lund University did not fulfill its promise to use its enormous funds of the Thorsen donation to support the field, enthusiasm was gone. The last Ganzfeld study was carried out during this period and despite multiple data analyses, did not generate a single statistically significant result. A similar situation occurred in the Ganzfeld study with Brian Millar and John Beloff, which I ran after a major break-up with a girlfriend. All this is naturally post-hoc and anecdotal but it seems worth putting on record.

Is then Brian Millar then merely stating an inconvenient truth when he says that the major successes are due to relatively few experimenters and that the current crisis occurs in the field because they are becoming more and more scarce? Certainly there are examples of major researchers who did seem to possess a psychic ability and these individuals appeared to be previously more common. Brian Millar's favorite example detailed in his paper is the Scandinavian Haakon Forwald, but mine would be another Scandinavian, Martin Johnson, who held for 13 years the Western world's only-state funded professorship in parapsychology. Johnson was raised in Northern Sweden amongst the Sami culture and he recorded many examples of his own premonitory dreams of which at least one was well-documented: the downing of the DC-3 spy plane (see Parker & Mörck, 2011). Martin Johnson was successful in a wide range of psi-testing situations, but his main achievement was the application of the Defense Mechanism Test (DMT) in order to predict psi scores. This was so consistent that DMT was once held up as a strong candidate for the repeatable experiment in parapsychology. With this expectation in mind, it was disillusioning when I arranged a supervision session with Martin Johnson in order to learn the DMT and he jokingly introduced it with the words "And now we shall have a séance". The analogy with a séance may have been appropriate since the technique seemed even more subjective than other projective tests (including the Rorschach) which I had been trained to use as a clinical psychologist. More objective was that a later re-analysis by Elendur Haraldsson, Joop

¹ I am well aware that the findings relating real time hits to top down processes may prove to be another elusive or even an artifactual finding. The finding was however not a prior hypothesis but empirically generated. Nevertheless there is an analysis by Westerlund et al., 2006 suggesting the real-time hits were due to subjective validation. As stated there in the paper, the analysis was based on the data from a study showing psi-missing amongst the participant judgments which makes Westerlund's position very difficult to interpret. Striking real time hits were not present in studies that were non-significant (e.g. Parker and Sjöden, 2010).

Houtkooper and Martin Bäckström indicated the hits with the DMT occurred when Martin unaccountably deviated from his set pattern.

But were then the spectacular successes of the Duke Laboratory due to the “psi abilities” of Rhine or Pratt? There is nothing to my knowledge in their biographies (Keil, 1979; Rao, 1982; Berger, 1988) to suggest that either Rhine or Pratt had their own psychic experiences. Further historical research might of course come to suggest otherwise.

In the case of Honorton, little is written about his biography, but in his presidential address (Honorton, 1976) he presented evidence of how in three of his own studies he had unconsciously influenced the outcome of random number generators. He ends the address by urging that psi-mediated experimenter effects should be seriously studied. Following his early death, Rex Stanford in a tribute to Honorton, noted how the successes of Honorton’s Ganzfeld work were vulnerable to experimenter psi influence and urged the implication of a control means of randomization, which would use computerized sampling of random number tables. Apparently this was intended but prevented by Honorton’s premature death.

Given this, were our own Ganzfeld successes explicable this way? The early work used dice to gain random access to printed tables and the later studies were dependent on the computer’s generation of random numbers. The best results occurred when I was the main experimenter with the receiver rather than being responsible for the randomization. Since an argument can even here be made for psi-based experimenter effects, I do not mind putting on record that my mother had a reputation for being psychic, but if I have inherited such ability, then it is dependent on a multitude of factors.

It may then well be that Rhine’s (quoted in full earlier) statement: “the very conditions that help the experimenter to liberate this [his own psi] might be the kind that would help him to induce his subjects to perform successfully” is insightful and accurate.

The Zeitgeist and the zeal of the researchers as part of this Zeitgeist is certainly one of these major conditions. The Zeitgeist in combination with their personal zeal appears to be at least in part the explanation for the astounding early successes of the Rhine and Pratt.

Although we cannot say much more about any psi based experimenter effect of Rhine and Pratt in this respect, there are what I regard as likely living candidates of experimenter psi occurring in this context. The consistent successes of Roger Nelson and James Spottiswoode in confirming their own, in my personal opinion, rather implausible theories, could arguably be due to experimenter psi. (There is no space here to go into the debate on the topic of “Global Consciousness” but I refer the reader to the contributions by May, Nelson, Bancel, and Spottiswoode in the *Journal of Scientific Exploration*, 2011, 25, 639-698.)

However the irony here is that Ed May might himself be an example of the same practice. When parapsychologists were taking the “Sidereal Time” seriously, Ed May, who is a renowned successful experimenter, asked to have access to and to analyze our Ganzfeld results from this perspective, which we enabled. Dr. May found that he was able to confirm this (for me even then as now, rather bizarre) hypothesis on our data. I know of further striking examples amongst my colleagues, who when looking through parapsychological data to test what appear to me to be contrived hypotheses, have found astronomical significant effects, but effects which later fail to replicate. Like Brian Millar, I am wary however that if too much attention is given to the investigation of current experimenter-psi by normal forensic means, the “thieves”, as Millar put it, may begin to wear gloves.

There is however a far greater danger and it is one that Millar reveals only in a parenthesis. He writes: “Another possibility is that psychological research is as contaminated by experimenter psi as parapsychology itself” (p.19).

I see this not as a possibility but as a probability and a dire one. Nowadays academic psychology is often run according to the profit motive, hyper-aware of its public image and with cut-throat

competition for the large funds required to sustain research. Several times my applications for funding have received methodological approval but have been turned down explicitly because I do not have a conventional explanation for psi that would enable psychology to keep its status quo. Just how inimical parapsychology has become was made clear to me when applying for Perrot-Warrick Senior Research Fellowship, and sought to fulfill the requirement of a host university in the UK. Naively, I was curious to know if Cambridge University, which after all did host the actual fund, would also host the researcher. I made contact with a former fellow doctoral student, who now against all odds had become a lecturer in clinical psychology at Cambridge. My friend, who had in the meantime transformed from an open-minded and colorful character into a grey man behind a desk, informed me with absolute certainty that parapsychology was banned there in all shape and size. The reason he gave for this was that if psi occurred, then many of the secure findings of psychology would be forthwith undermined.

The situation has progressively worsened, as the overreactions to the Bem publication and the Sheldrake TED talk have shown and as I personally experienced when I was recommended for the appointment as the Thorsen Professor at Lund. After much dispute, the Lund chair was given to Etzel Cardena with the assumption that he would use this large donation for conventional psychology. When this did not entirely happen, nine Lund professors publicly denounced him and wanted to "psyche him out" from the chair for "spreading pseudoscience" and "scientific aberrations" (Newsvoice 2012, Svenska Dagbladet 2012). Leaving whatever other thoughts one might have about this aside, these episodes are dramatic examples of science having its own "papal inquisitors".

In the wake of this, it is clear that in order for parapsychology to take on this now fierce and formidable opposition, it would need considerable backing from a higher authority. I know of only one such emerging authority and this is quantum science.

This means it is now a waiting game. Until there is a fuller emergence of a quantum physics and quantum biology (Lanza, 2009) to provide the secure theoretical basis for consciousness studies and parapsychology to operate from, it is probably politically correct to continue keeping the secret and allow psychology to follow the rules of conventional science, treating parapsychology as "anomalous psychology", or expressed satirically, letting it (to quote Paul Simon's song) "slip-slide" after psychology's bandwagon (Parker, 2012). On the other hand the most politically correct choice is not always the most honest or wisest one.

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