

# Does Psi Exist and Can We Prove It? Belief and Disbelief in Parapsychological Research

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## Abstract

*Psychokinesis research is encountering difficulties in replicating its findings. While experimental and analysis methods became more and more professional in the last decades, researchers complain about a loss of effect size and evidence. Walter von Lucadou explained this erosion of evidence with his “Model of Pragmatic Information” (MPI). He proposed a new experimental paradigm for future research. In elaborating further theoretic implications and consequences of the MPI, it is shown that the MPI is not suitable to provide evidence. Some effects like successful replications of ganzfeld experiments or the successful replicated sheep-goats effect indicate that the MPI has to be upgraded. Some evidence is given that successful experiments are dependent on experimenter’s belief in paranormal phenomena. The best conditions for growing evidence might be the use of test subjects and experimenters who are open-minded and do not doubt in the existence of psi. The demand of skeptics to ban parapsychology from the realm of science have to be rejected. It is a science with its own special research conditions.*

## Introduction

The scientific status and position of parapsychology in the sphere of science has been a bone of contention from the very beginning (Bauer, 1985; Palmer, 1990; Alcock, 2003; Parker, 2003; Parker & Brusewitz, 2003;

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Irwin, 1989; Hoebens, 1982). Exponents of the skeptics' organisation GWUP (the German CSICOP) challenge the 'scientific nature' of parapsychology and seek to ban it entirely from the area of science if it fails to provide proofs for the existence of psi. In this context, it is often claimed that 'parapsychology has yet to succeed in identical replicating a single anomalous effect under laboratory conditions' (Hüsgen & Kamphuis, 2000)<sup>1</sup>. Beside the problems of replication we find in general a decline of evidence and effect size of psychokinetic phenomena. Is this a consequence of the increasing skepticism in the last centuries?

At the beginning of the 17th century there was no academic parapsychological research. Miracles and paranormal events were generally accepted and widely evident in the society. Skepticism was just beginning to be a part of scientific work. In this time, the Italian monk St. Joseph of Cupertino provoked the displeasure of the Holy Inquisition through the numerous cases of him levitating during the elevation of the host which could not be explained scientifically: "There are many skeptical witnesses of the numerous levitations of Joseph of Cupertino who did not trust these phenomena and had enough scientific knowledge to justify their doubts. Yet it was precisely in the presence of such skeptical witnesses that Joseph of Cupertino levitated to amazing heights, virtually every time that mass was celebrated. The levitation occurred to him so frequently and led to such a disturbance of the service that he had to be tied down with lead boots; yet this was to no avail and he rose together with the lead boots. Sometimes he levitated to the ceiling of the church and it was only with the greatest effort that he could be brought down to earth again from the highest ledge to which he held on to after his awaking from ecstasy. On several occasions, an acolyte tried to hold him down but was himself carried upward together with him." (Benz, 1969, p. 218). Macro-PK phenomena like levitation were evident in the 17th century. There was little doubt about it, and even some skeptical witnesses were convinced by the experience of paranormal phenomena.

On the wake of the 20th century, paranormal effects became slowly an area of research. Reports of poltergeist phenomena and macro-PK events were widely discussed but never got an academic status or scientific recognition. Nevertheless, mediumistic phenomena were fascinating the academic world and attracted respected scientists like the radio pioneer Oliver Lodge. The famous German author and Nobel laureate

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<sup>1</sup>Quotes of German papers and studies were carefully translated into English and indicated by single quotation marks. Original English quotes were indicated by double quotation marks.

Thomas Mann only hoped to see 'once again, with my own eyes, the handkerchief ascending into the red light' (Mann, 1983, p. 255), and in 1922 mediumistic-talented test persons could move macroscopic objects many feet by psychokinetic influence (Bender, 1966, p. 496). In the 1930's, J. B. Rhine introduced scientific methods in parapsychology to evaluate macro-PK effects with psychokinetic talented test persons trying to influence dice tossing. Later Beloff and Evans introduced electronic devices and random event generators as targets for micro-psychokinetic influence.

Today skeptic scientists are often involved in parapsychological experiment design in order to protect it against fraud or misinterpreted natural explanations. At the same time, anomalous PK phenomena become rare and weak in parapsychological research, and have shrunk to minor statistical mean value deviations of micro-PK effects in large databases containing abstract columns of numbers. It is required to run tens of thousands, even hundreds of thousands of PK experiments before any significance becomes apparent. The days of flying monks and PK-moved objects are over. Why did the effects lose their impressive strength? Were they all the result of fraud?

Skeptic scientists argue that with improved methods of analysis and evaluation many errors, artifacts and even fraud were excluded which seem to have been the true source of claims of the paranormal for them. When the highest standard of analysis is reached, no paranormal phenomena would remain in their view. But this is only one interpretation. It is the aim of this paper to introduce another interpretation: increasing skepticism for itself might be one reason for the erosion of evidence. This could depend on the nature of paranormal phenomena itself.

### **The problem of successful replications**

In 1997 the Princeton Engineering Anomalies Research group (PEAR) published its evaluation of a twelve-year series of micro-psychokinesis tests with random number generators (RNGs) which came to a (statistically) impressive conclusion: "The overall scale of the anomalous mean shifts are of the order of  $10^{-4}$  bits per bit processed which, over the full composite database, compounds to a statistical deviation of more than 7 sigma ( $p = 3.5 \times 10^{-13}$ )" (Jahn et al, 1997, p. 363). The effect size of one bit in every 10,000 which could be changed

by the test subject in the intended direction appeared to be reliable and leading to the expectation that psychokinesis really exists as an anomalous, replicable phenomenon. A similar conclusion was drawn by Dean Radin: "After sixty years of experiments using tossed dice and their modern progeny, electronic RNGs, researchers have produced persuasive, consistent, replicated evidence that mental interaction is associated with the behaviour of these physical systems" (Radin, 1997, p. 144). This leads to the expectation that PK effects could be easily reproduced with a large number of tries and test subjects (Radin & Nelson, 1989). In 1996, the collaborative programme of anomalous Mind-Machine Interactions (MMI) under the leadership of the PEAR group was established. The laboratories of the Freiburg Anomalous Mind-Machine Interaction (FAMMI) and the Giessen Anomalies Research Program (GARP) took part in it. Their common goal was to replicate the successful PK results of the PEAR PK experiments. What also could be more disappointing than to discover in the years that followed, that the large-scale replication test performed by the MMI consortium was neither able to confirm the effect size that had previously been established nor to attain the level of significance which was to be expected on the basis of the tests run previously (Jahn et al., 2000).

But this disappointing result is not limited to the PEAR laboratory and the MMI consortium alone. While Radin and Nelson still claimed a reliable effect (Radin & Nelson, 1989; Radin & Nelson, 2000), a new meta-analysis based on effect-size computations rather than on Stouffer-Z revealed that the observed effects were not as reliable as they seemed to be: "A sensitivity analysis showed that only 67 studies, each with an average of 2366 bits, would be required to bring the database down to non-significance. Thus just a few studies could potentially change the conclusions from this meta-analysis." The PK meta-analysis of Steinkamp, Boller and Bösch collected the data of 357 published experimental studies and 142 control studies, and "both yielded the same effect size of  $\pi = .50003$ , although the effect size from the control studies went down to  $\pi = .49999$  once one large control run reporting a significant effect had been removed." (Steinkamp et al, 2002).

At first glance the situation looks much better for the field of ganzfeld experiments (Utts, 1991). Bem and Honorton (1994) reported replicable evidence for an anomalous process of information transfer in 1994. Milton and Wiseman criticised the Bem and Honorton study, reanalysed the data and found a non-significant result in total. Bem,

Palmer and Broughton (2001) criticised the Milton and Wiseman study because they included studies which were not conform with the standard ganzfeld procedure, and they confirmed the significant overall result of the Bem and Honorton meta-analysis. But we don't know how robust these results remain if somebody works out a meta-analysis using those rigorous criteria which were applied in the Steinkamp, Boller and Bösch PK meta-analysis (Steinkamp et al, 2002).

These disappointments certainly fed the (skeptical) suspicion that anomalous paranormal effects do not exist (Alcock, 2003). However, according to Walter von Lucadou there would have been in fact no real reason to be disappointed in case of PK experiments, if the MMI consortium had applied his model of pragmatic information to the replication and the formulation of the effect size expectation. His model predicts that decline effects must arise in future replications (Lucadou, 2001). Is the model of pragmatic information convincing enough to reject any skeptic objection?

### **The model of pragmatic information**

The model of pragmatic information (MPI) is a theoretical approach predicting such declining effects in psychokinesis experiments. It is not yet a complete and finalised theory, merely a model which seeks to describe the conditions in which an anomalous effect might be expected.

In MPI anomalous or psi effects are not supernatural but meaningful correlations between the test person (psi agent) and the target system (RNG). While interacting, the psi agent (or test subject) and the RNG become a closed system with self-referential dependencies, an "organisational closure" (Varela, 1985). This is irrelevant to any temporal or spatial distances, it is a non-local analogy to non-local effects in quantum mechanics (Lucadou, 1992). Its boundaries are defined by the ratio of internal and external pragmatic information in the interaction of its constituent parts. (Lucadou, 2001) The correlations of MPI are — in worst case — only a weak violation of the laws of nature as known today because the underlying mechanism of the correlation is unknown. However, the situation becomes more critical when such correlations are supposed to be used for long-distance transfer of information or signals. The possibility of intervention paradoxes prohibits such an information transfer: it would be a serious violation of natural laws (If I know what will happen in the future I can act in the present in such a

way that I can prevent unpleasant future events occurring). Therefore Lucadou recommends: "Do not treat psi as a signal!" (Lucadou, 2001, p. 10).

Pragmatic information is "a measure for the meaning of the information". It manifests itself in "its effect on the system", but it has no informative content (unlike a newspaper or a newscast on the radio). In order to show what pragmatic information is, Lucadou reported the following party scene: 'One of the guests is called to the telephone, returns after a short time and leaves the party without a word to the other party guests. No one else could have heard what was said during the telephone call, but we all know that something important must have occurred' (Lucadou, 1997, p. 144). We are unaware of the content of the call itself, but only of its effect.

Pragmatic information (I) which a system produces, is in itself the product of further factors which exclude the possibility of using pragmatic information for signal transfer: An event with the character of novelty happens unexpectedly and suddenly, it cannot be the basis of signal transfer because I don't know before whether this event might happen or not. An event which acts with autonomy cannot be used for signal transfer because I don't know how or where it will appear. These factors of pragmatic information exist in opposites: "Novelty" vs. "Confirmation" (Weizsäcker, 1974). Lucadou added later "Autonomy" vs. "Reliability" (Lucadou, 1997). The portion of pragmatic information grows in line with an increase in the portion of autonomy and/or novelty. The system itself contains something that resembles a "memory" in which the system states of the past are "stored". While the factors "Confirmation" and "Reliability" rise, the product of the produced pragmatic information falls. These factors are responsible for the decline effects observed in the replication experiments, because the novelty declines when repeated tests are run to reproduce such effects. At the same time the autonomy is limited, since one possible test result is already available as a result of the pilot experiment. In order to enable a repetition of a high degree of novelty, the effect must emerge either elsewhere in a replication where it is expected, or it must change its effect size or direction. MPI provides the possibility of conceptual replications with high degrees of novelty and autonomy. Identical replications have to fail: if they are successful they could be used for signal transfer which would violate the exclusion of an intervention paradox.

After all, with such a model the results of random experiments can

be described. Yet, how do we know whether a single psi effect claimed is a (still) unexplainable anomaly and not simply a variety of coincidence? Can meaningful research activities be conducted at all under such conditions?

In 2000 the author made a conceptual replication of the full moon effect as claimed by Radin and Rebman (1998) for casino payout rates with Retro-PK experimental data from Fourmilab (Walker, 1996), expecting that the Fourmilab Retro-PK data would demonstrate the same full moon effect. The time serial analysis of the experimental data with respect to lunar phase was published in the year 2000 (Etzold, 2002). A significant  $z$ -value of 3.24 for the first 53,082 Fourmilab Retro-PK experiment data seems to confirm Radin and Rebman's claims of a peak effect in the full moon period. Was it an anomaly or just a coincidence? After the publication in 2000 I made a replication of my first analysis with the next 47,192 experimental data which were accumulated in the Fourmilab Retro-PK data base until August 2001. This time I was doubtful about the outcome of the analysis. I could not believe that the observed lunar effect was persistent enough for replication (MPI for example forecasts a decline effect for the new evaluation). Now I got a (negative)  $z$ -value of -2.49 for the specified full moon time period, and I reported that this replication failed (Etzold, 2002).

Referring to my results (Etzold, 2002), von Lucadou wrote (2002, p. 83): 'The MPI...does not state that, if the experiment were to be repeated, the effect that had been established earlier would simply disappear, since it was merely a random fluctuation. Under MPI, it either disappears *slowly*, something which one would not normally expect to occur with a random fluctuation, or it *overturns* (as was the case in the Etzold study), or it appears in *other channels*, as occurred during the large-scale MMI replication experiment (Jahn et al, 2000)'. In a somewhat schematised form, three possibilities therefore emerge under MPI for an anomalous effect during replication:

- (a). Slow reduction (Decline)
- (b). Overturn, change of signs
- (c). Emergence in 'other channels' (Displacement)

Do these truly represent all of the possibilities, or are there more? As far as the three possibilities are concerned, (a) and (b) would appear

to be reasonable to the extent that they are found in the observation direction or at the other end of the scale. Yet, possibility (c) appears to be highly problematic. How do I know in which 'channel' the effect will re-appear? What happens if I am unable to find the channel because I do not possess the methods and measuring techniques for this channel?

These three possibilities therefore are not a real help if I am unable to say immediately after completing the replication experiment and prior to evaluating the data whether or not, under the circumstances, I can expect the outcome to fall into category (a), (b) or (c). Without further definition, the three possibilities put forward by Lucadou can be applied to the expected effect of any given RNG experiment in replications. A lack of evidence always remains. Lucadou himself admits in general (2001, p. 7): "To my conviction, parapsychology has... not yet succeeded in establishing indisputable scientific evidence that psi exists."

In a discussion with Volker Guiard (Lucadou, 2003), Lucadou points to his two fundamental theorems of parapsychology which I would like to reiterate at this juncture (Lucadou, 1997, p. 162):

1. Psi phenomena are non-local correlations in psycho-physical systems that are induced through pragmatic information which is generated by the (organisationally closed) system. The physical part of the system might be a random event generator, the psychological part is represented by a test person.
2. Each attempt at using non-local correlations for the purpose of signal transmission causes these to disappear, or converts them in an unpredictable manner.

These theorems are not widely accepted. In connection with the second fundamental theorem and its implied avoidance of intervention paradoxes, Lucadou also writes 'that psi must be conditioned in such a manner that no reliable signal transmission can result. This would suggest that, during a psi experiment, each statistical deviation that is measured and which can be interpreted as psi or an anomaly may not exceed a certain parameter' (Lucadou, 2003, p. 139). A *signal transmission* would mean: a clear and identifiable signal which is more than pragmatic information without any uncertainty.

### **According to MPI the existence of psi cannot be proven**

In empirical science, inductive evidence is taken to confirm hypotheses which are derived from experience, observations and experiments. In this context, the term 'inductive' merely stands for a probable causal link between a hypothesis and the findings of an experiment or observation. The amount of truth which results from an experiment, based on a hypothesis, becomes all the more probable, the more frequently it can be repeated. Evidence relies on information which can be obtained from the interpretation of the experimental data.

For parapsychology this process of obtaining evidence, according to von Lucadou, depends fundamentally on MPI: "Because the MPI is a general system-theoretical description of interacting systems which acts self-referential, it can also be applied to the system that creates scientific evidence." (Lucadou, 2001, p. 10)

The information contained in the claims of evidence can, for example, be summarised in one sentence: 'psi-phenomena exist'. This is more than just external pragmatic information. It is a concrete piece of information content. This means that the correlation must be so convincing that it unmistakably 'carries' such information and consequently assumes the character of a signal. This approach, however, violates Lucadou's second fundamental theorem of parapsychology since, after all, the intention of this 'horizontal signal transmission' is to convey the information that 'the anomalous psi-phenomena exist'. The consequence of this is that the anomalous phenomena disappear or are modified in an unpredictable manner. In concrete terms, this means, that as soon as the experiment is repeated for the purpose of proving the anomaly, the results of the experiment will vary in the frames of the null hypothesis.

For skeptics the condition *sine qua non* for claiming evidence of an effect is a successful replication of the effect. Hergovich (2001, p. 122) summarises the skeptical position: 'To date, no convincing experiment has been found that proves the existence of psi-phenomena. It is not because the methods required by psychology could not be adequately applied or because the effect sizes were perhaps too weak..., but because the effects are not reliable enough.' Under MPI in the present shape, however, the effects cannot be 'reliable enough'.

The situation gets even more complicated because in such an experiment which should provide evidence, the whole dubiety of our con-

ventional worldview is present. With such a burden of information, the replications of an experiment for proving psi possibly has to fail according to Lucadou's second fundamental parapsychological theorem.

If the presumptions of the MPI were correct, psi comes therefore in fact across as a troll, a ghost that only manifests itself when there is no scientific conclusiveness. 'The more confident one is of having 'bagged' the psi effect, the lower the chances are that it can be replicated in a future experiment' (Lucadou, 1997, p. 187). However, on this basis, it is not possible to prove psi by further replications with the help of scientific laboratory research, and any attempt will lead to further disappointment. What ways out are there? At first we have to check whether the MPI is conform with the observed phenomena in parapsychology.

### **Is the MPI correct with its presumptions?**

Anomalies in the sense of psi effects are evidently phenomena with the quality that they cannot be proven using conventional scientific methods. If the MPI is correct, we have to look for evidence but not for convincing proofs because proof-testing methods will destroy any possibility of finding evidence. Lucadou (2001, p. 13) has therefore proposed a new experimental paradigm that has been derived from MPI and which modifies the exterior test procedures and their evaluations with a view to attaining better findings. These include among others: no accumulation of evidence; short test runs; triple blindness; conceptual, i.e., no identical replications. However, his fundamental requirement alone, that of not-treating psi as a signal, raises doubts as to whether this new paradigm can produce better results. As long as this new paradigm is also accompanied by a level of interest in producing scientific proofs, any potential anomalous effect may be bound to collapse, no matter how much autonomy and novelty the experiment is subjected to.

Another critical point too is the postulation of short test runs. It is easier to make a study with short test runs, but it is easier too to suppress non-significant results of short test run studies. Additional, the interest of a journal's editor to publish short test run studies with non-significant results might be lower than to publish a long test run study with non-significant results.

Many scientists do not accept the MPI and its presumptions. One reason is that some empirical findings are not conform with the MPI. There is a lot of anecdotic material which suggests an anomalous in-

formation transfer which might be excluded by the implications of the MPI. Even in laboratory research, ESP could be used already for anomalous information transfer: Ryzl (1966) was successful in identifying “five three-digit numbers. . . without a single mistake.” Ryzl claimed indeed, that “to do this, it was necessary to make 19,350 single color-calls. . . The average speed on the whole was about 400 calls per hour so that the mere accumulation of the data took some 50 hours (with two persons participating).” But this amount of time seems to be negligible with respect to the importance of this result today. In the context of the MPI, such a result might not be possible.

The concept of pragmatic information is controversial. Every scientific experiment is a kind of information transfer. We want to get new information and we want add knowledge to our world view. We have seen here that a scientific progress in psi research is only possible if we treat psi as a signal. Without this signal we don't have anything which we could interpret.

Another point is the forecasts of the MPI. As long as the MPI does not provide an exact formula which allow us to forecast the appearing and the magnitude of an expected anomalous effect, the MPI is suspected of being a self-immunisation strategy against critics. But this formula is necessary to test the MPI for its correctness (Schestag, 2002).

### **Belief and Disbelief in Parapsychological Research**

Meanwhile believing in the existence of something has become part even of such solid sciences like physics. The hypotheses and theories about parallel universes or a infinite number of open multiverses seem to be mathematically correct (Susskind, 2006). But it is impossible for us to enter these multiverses. Space and time are insuperable frontiers for our explorative attempts of today. We have to believe that they exist, based on mathematical predictions.

In parapsychology the situation is somehow reversed. We don't have a theory of anomalous phenomena, but we have experiences which indicate the existence of anomalous phenomena. And we have indications that dealing with psi anomalies scientifically requires that I already believe in the existence of these anomalies if I want to obtain positive and significant results. This credo is not to be interpreted as any form of intellectual shortcoming like Hergovich (2001, p. 171) claims, but rather as an opportunity. By doing this, the experimenter is tak-

ing off the pressure from his research activities to have to prove something that cannot be proven. This could facilitate the scientific progress in parapsychology. The traditional Cartesian doubts that are prevalent in natural science are also merely a subjective fundamental principle which is just as capable of producing its 'cognitive blind spots' as 'belief' does. Yet, in the case of parapsychology, the Cartesian doubts are counterproductive, as it has been shown at last by the failed replication tests performed by the MMI consortium (Jahn et al, 2000). Only by this way it can be checked if the claimed human-machine interaction actually exists, if the thoughts in the mind of the experimenter can generate a corresponding effect in the physical world. Those researchers who believe in the existence of anomalous phenomena or believe in their own ESP or PK ability will get more positive results in their studies with other test persons (Smith, 2003b). Those who doubt this will get also the appropriate 'psychokinetic result' which seems to negate the existence of paranormal phenomena. The growing lack of positive PK results, the "erosion of evidence" (Lucadou, 2001, p. 7) might be a result of growing disbelief in the possibilities of PK which for itself could be a PK-generated result.

The first true indication of this effect emerged in the studies performed by Gertrude Schmeidler (1943) on the effect of belief or disbelief persuasions in ESP experiments. She observed that subjects who believed in an anomalous effect (the 'Sheep') performed better than those who viewed anomalous effects with skepticism (the 'Goats'). Schmeidler's notion of separating the 'sheep' from the 'goats' was: "Do you believe it is possible that ESP can be shown under the condition of this experiment?" A meta-analysis of the 'sheep-goat ESP studies' for the years between 1947 and 1993 performed by Lawrence (1993) produced an astronomically high  $z$ -value of 8.17 ( $p = 1.33 \times 10^{-16}$ ) which provides high evidence for the existence of a sheep-goat effect. Edgar Wunder complements in reaction to my own reflections (Etzold, 2004): "The meta-analysis of Lawrence already was even a successful replication, namely of the above comparable study of Palmer (1971). Palmer (1971) found in the studies published till there a sheep-goat effect of a medium effect size which Lawrence found in the studies published afterwards again in the same order of magnitude". It seems that contrary to the predictions of the MPI successful replications are possible.

Believing in psi seems to improve the results of PK experiments. Smith (2003b) evaluated the psychology of the 50 named 'psi-conductive

and psi-inhibitory experimenters' and found by multiple regression of self-report questionnaires that higher psi-conduciveness scores were associated with belief in one's own ESP or PK ability.

Smith, while discussing different kind of experimenter effects based on explanations of social-interaction (Smith, 2003a), has collected some successful studies of parapsychological experiments which might confirm this statement and supposed: "If psi is real, then it is plausible, indeed likely, that the experimental participants are not the only source of psi in a successful parapsychology experiment. The experimenter may also exert a psi influence over the data. Given that apparently 'psi-conductive' experimenters typically tend to believe that psi exists, and are highly motivated to obtain findings in support of psi (often more so than their research participants) then one might argue that the experimenters are potentially a more significant source of psi than the participants." (Smith, 2003a, p. 79) Others before him have suggested the same experimenter-influence and noticed some anecdotic material: "For example, when Blackmore, a devoted parapsychologist for many years, found herself increasingly skeptical about *Psi* as a consequence of her inability to produce experimental evidence for it, she noted that 'many parapsychologists suggested that the reason I didn't get results was quite simple — *me*. Perhaps I didn't sufficiently believe in the possibility of *Psi*' (Alcock, 1987, p. 561).

This is possible. Smith commented in view of experimenter effects: "From a methodological perspective, whatever the purported mechanism(s) of this effect of the experimenter upon the data, it does raise potential problems for skeptical researchers who wish to attempt to replicate psi experiments. This is because it suggests that such researchers, especially if they act as the experimenter who comes into contact with research participants, are less likely to obtain positive findings even if the psi effect is real." (Smith, 2003a, p. 82) This material gives some evidence for the claims that a causal link exists between the erosion of evidence with increasing scientific criticism and skepticism. If this is true one skeptic experimenter or even other persons like checkers or observers (White, 1976a) could dominate the effect size of the whole experiment.

Alcock (2003) told an example for this case in which his friend Jeffers was involved, but without noticing that he himself could be the reason for obtaining negative results. "Jeffers stands in lonely company as one of the very few *neutral scientists* who have empirically investi-

gated the existence of psi phenomena.” (Alcock, 2003, p. 36) Jeffers tried a conceptual replication of the PEAR RNG-PK experiments, not using RNGs but interference of light as target for anomalous influence. Alcock himself, whose position is radical skeptic, was involved in this experiment: “Jeffers came to me at least a tad defiantly, requesting that I review his experimental design and offer any suggestions and criticisms before he began his research. He stressed that I should not after the fact, were he to obtain data supporting the parapsychological interpretation, then argue that the experiment was not to be taken seriously because it had fallen methodologically short in some fashion. Thus began our relationship, which was to grow into the very positive one that it is today.” (Alcock, 2003, p. 36-37).

Alcock himself became part of the organisational closure, in this case as a doubtful experimenter who wished to find the confirmation for his disbelief in Jeffers’ experimental result: “As Jeffers reports in his paper, his research findings give no support to the Psi hypothesis.” (Alcock, 2003, p. 37) The possibility that Alcock himself produced via the experimenter’s psi faculties the negative result of Jeffers’ research was not discussed in his paper, but cannot be ruled out if we apply the MPI for the whole system which consists of Jeffers, his experimental target and also Alcock as critical designer and reviewer of the experiment. Alcock, who believes in the null hypothesis and asks to give the null hypothesis a chance will find nothing else than evidence for the null hypothesis. If psi exists, and I believe it, psi will also acting in the skeptics attempt to obtain evidence for the non-existence of psi.

### **Consequences**

In science we have “two schools of research on belief in the paranormal” (Lawrence, 1993, p. 83), represented by scientists and investigators who differ fundamentally in their approach. ‘Parapsychologists’ as well as the ‘skeptics’, organised in different communities. Every school has their own lists of studies which provide evidence for the correctness of their own belief or disbelief. These “schools” have been around since the inception of scientific parapsychology, and they are testimony to the fact that the scientific status of parapsychology was controversial in the beginning.

The conclusion drawn by the parapsychologists that predicated anomalies (or psi effects) cannot be proven in sense of a skeptical proof,

might alleviate the tension in the relationship. For the 'skeptics', this would mean making a concession of not demanding from the parapsychologists, what they (and other scientific disciplines) can not produce. For the parapsychologists, it would mean relief, in that they would no longer need to 'prove' anything to 'the others'. Instead of having to invalidate their own findings in a proof-orientated world of research, they have now found space to ask for other questions, parameters of psi performance for example. If I evaluate under which conditions an anomalous effect appears or not, I do not only know that there is an effect but I know how it works too. Belief and disbelief are such parameters.

More than in any other scientific discipline the researcher and the experimenter themselves are part of the experiment they observe and analyse. Their expectations, hopes, fears, beliefs and disbeliefs are self-referential, they act as self-fulfilling prophecies (Watzlawick, 1985). There are two possibilities how the experimenter's belief or disbelief could affect the results: via affecting the participant's belief or disbelief in paranormal phenomena (Wiseman & Watt, 2002), or somehow more direct: by unconscious influence on the outcome of the experiments in the same manner as the attempts of the test persons to get a convincing result. The experimenter, regardless of his beliefs, has probably the highest interest of all in the outcome of the experiment. Therefore, he might be the most powerful psi acting agent — possibly against his own will.

As we have seen, the existence of anomalies or psi effects cannot be proven in a radical skeptical sense. A grain of doubt will always remain. But this is common with all other scientific research. Everybody is likely to find evidence for his own belief. It is equally possible to gather strong evidence for the existence of psi like it is possible to gather strong evidence for the null hypothesis. The one is true, and the opposite is true, too. Evidence in this case means only that belief or disbelief create their own corresponding results in the real world (Etzold, 1992). The answer for the question "does psi exist?" (Parker, 2003) is undecided and has to be undecided as long as we have found no convincing evidence which might even satisfy skeptic doubts. Eberhard Bauer (1991, p. 138) states that in spite of all the skeptic doubts, parapsychology still belongs in the realm of science. For scientific acceptance now it is more important to say under which conditions the existence or inexistence of psi is falsifiable. In general, the thesis "psi does exist" is falsifiable if every human experience can be explained in conventional scientific terms. The thesis

“psi does not exist” is falsifiable if anomalous human experiences will be found which cannot be explained in conventional scientific terms. Bauer qualifies this by writing that parapsychology ‘does not seek to prove psi but instead wants to find explanations for a certain type of human experiences for which temporary was used the neutral theoretical term psi’. (Bauer 1991, p. 142). Parapsychology has to be considered a scientific discipline as long as human beings have experiences which can’t be explained with the help of conventional scientific knowledge. However, this discipline has research approaches different from any other scientific branch. Against skeptic claims that no paranormal effects were ever replicated, we have to state that replications are possible. Parker and Brusewitz (2003) have given a list of successful research reports. The summarised results of parapsychological experimentation are indicative of an anomalous process of information transfer. Evaluating the state of belief/disbelief of the experimenters in connection with the experimental results might be another way for finding growing evidence. However, it is highly questionable if this will convince skeptics.

Lawrence claimed: “What is needed is a good, reliable, accurately validated measure of general belief in the paranormal... Questions should most certainly include the Schmeidler question seen to be joint most successful measure of belief in terms of getting results.” (Lawrence, 1993, p. 83) And White adds: “It is obvious that the role of the experimenter (conceiving this term in its broadest sense) must be taken into account in designing the results of parapsychological experiments” (White, 1976b). And Parker added: “High-scoring subjects and successful experimenters are to be found and a technology is available.” (Parker, 2003, p. 132) Test subjects like experimenters should be tested before the beginning of an experiment, using a variant of Schmeidler’s question: “Do you believe it is possible that PK can be shown under the condition of this experiment?” For doing successful parapsychological laboratory work it seems necessary and consequent to document the belief or disbelief of the experimenter for further evaluations. But the most promising way to respond to the question of missing evidence might be the search for parameters and modulator variables of psi performance.

We know that the daily weather is very elusive. While the sun may shine at one moment, clouds could appear in the next hour and the day could finish with heavy rain. In the past people had only weather proverbs for forecasting the weather of the next few days, and we know how reliable those weather proverbs had been. Most of them were noth-

ing more than superstitious beliefs. Today, the most powerful computers and immense data pools of different environmental data are necessary to forecast the weather alone of the next few days. In relation to that, the prediction of the appearing and disappearing of an anomalous effect is still in its infancy. Kennedy argued, "that many factors combine together to make psi elusive" (Kennedy, 2001) and Wunder phrased the only meaningful question: 'In which respect psi anomalies are replicable and in which respect they are not (yet)?' (Wunder, 2004) To find the answers for these questions might be the tasks for the next time.

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*Zeitschrift für Anomalistik, 4, 40-44.*