



## Adrian Parker's Fabrication of Reality

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### **The Delmore Tests** (Saturday 13)

When the Parapsychological Association held its nineteenth annual convention in Utrecht in 1976, hosting professor Martin Johnson decided to treat the assembled scholars and researchers to some educational entertainment. This was in the days before Uri Geller became the most debunked psychic in history and the sort of tricks he performed was still on every parapsychologist's lips. So Martin Johnson invited magician/journalist Ulf Mörling, a.k.a. "El Globo", to demonstrate how alleged paranormal phenomena could be created through magic tricks. The intention was explicit and announced – Mörling was introduced as a magician, not as a psychic.

After successful demonstrations of precognition and metal bending, Mörling performed a telepathy experiment – in essence a variation of the old "draw-something-on-a-paper-and-put-it-in-an-envelope" trick. This also turned out successful but what happened next is truly astonishing (my translation):

*"At least ten of the parapsychologists in the audience, some of them having recently earned fame for field studies, expressed their conviction that Mörling really was a genuine 'psychic' without knowing it, something that was suggested to be 'a parapsychologist's worst nightmare.' One of those who most persistently argued that Mörling is a psychic without knowing it, was the same Ed Cox who had accounted for Uri Geller's wonders with a manipulated watch in *The Journal of Parapsychology!*" (Johnson, 1982, p. 115–117)*

In retrospect, such an absurd gullibility and thirst for wonder seems almost sweet and innocent. Uri Geller was thoroughly debunked by Marks & Kammann in 1980 (Marks, 2000) and Randi in 1982 (Randi, 1982), and a multitude of further embarrassing exposures later, no serious researcher will lend any credibility to the Israeli Jesus-wannabe, or to others performing the same kind of carnival tricks he did (and still does).

Randi followed up his Geller exposé with a direct blow to the parapsychological community when he sent two young

magicians posing as psychics to the McDonell Laboratory for Psychical Research at Washington University in St. Louis. The magicians, Steve Shaw and Michael Edwards, easily convinced the staff that they were the real thing and they were tested for a period of three years, without anyone even suspecting them of cheating. Prior to the tests, Randi had contacted the director, physics professor Dr. Peter Phillips, and offered to help with controls and protection against fraud and trickery. Phillips rejected Randi's offer. Videotapes from the experiments clearly showed that Shaw and Edwards were cheating, if you looked carefully. But no one at the McDonell lab had the inclination to look carefully. (Hines, 2003, p. 132–133)

When Randi finally revealed the ploy, the parapsychological community was taught a lesson that should be a textbook example in every science methodology and social psychology class. Since then, serious researchers have turned their interest to testing "normal" people rather than flamboyant gold-diggers and attention-addicts. Too bad so few have the spine to give Randi credit for this true progress in a controversial field of science. But enough about what serious parapsychologists have done, let's review what the less serious are up to.

Next to professor Etzel Cardéna at Lund University, Briton Adrian Parker is perhaps Sweden's most renowned parapsychologist. Besides holding a position as senior lecturer in Psychology at Gothenburg University, Parker is also a former board member of the Parapsychological Association and one of three researchers currently listed on the board of the Swedish Society for Parapsychological Research (SSPR). Parker's areas of interest are consciousness and PSI, although the latter seems to be his main preference. Together with the Psychology Department at Stockholm University and the Freiburg Institute, his Gothenburg group is developing an improved Ganzfeld technique. If you're not familiar with the term, Ganzfeld experiments are, according to its proponents, the best way to test individuals for extra-sensory perception.

In 2003, Parker, along with the SSPR chairman Göran Brusewitz, published "A Compendium of the Evidence for PSI" in the *European Journal of Parapsychology* (Parker & Brusewitz, 2003). The writers offer a list of studies they claim to provide evidence of paranormal phenomena. But not compelling evidence they point out: "the list is not intended to convince the reader that psi has been proven." In fact, they suggest that it is impossible to prove phenomena in empirical science. Thus it is futile to search for such compelling evidence. Instead, the intent is to collect studies that justify research aimed at understanding paranormal phenomena. Most studies in the compilation are still quoted as providing strong evidence today, according to Parker & Brusewitz, but not compelling. Oh, and there are proof-oriented studies listed, and experimental evidence, just not compelling.

What are Parker & Brusewitz really saying? Where exactly on a "proof" or "evidence" scale are these studies to be placed and does that position mean that paranormal phenomena exist or not? Do paranormal phenomena almost exist? What is the difference between strong evidence (which the writers suggest the listed studies provide) and compelling evidence (which the studies don't provide)? And how does the difference relate to the possible existence of paranormal phenomena? The writers give no answer.

I have tried to find references to this semantic orgy in methodology and science philosophy literature. Nothing, but that might only reflect the poor state of my library. Then I consulted the Cobuild English Dictionary for Advanced Learners, 2001, third edition, pp. 528, 1229:

1. Evidence is anything that you see, experience, read, or are told that causes you to believe that something is true or has really happened.

1. Proof is a fact, argument, or piece of evidence that shows that something definitely true or definitely exists.

Again, there might be scientific definitions that differ from these – a privilege of science is freedom of definition, i.e. freedom to define anything as you please, as long as you motivate and explain your definition, and use it in the same sense within the frame of your research. Parker & Brusewitz apparently don't think it's necessary to explain their definitions, and they toss them around in different variations without demarcation. Perhaps they hope that no one will question their terminology as long as they use it with ease. Or they might be caught up in a relativistic or postmodern frame of mind: what they write is true for them, in their context. Unfortunately, they have made a scientific claim and seek scientific acceptance.

In the dictionary definition, evidence pertains to belief and proof to knowledge. In that respect, I willingly submit to the notion that the Parker & Brusewitz compilation provides evidence, i.e. the studies may cause someone to believe that paranormal phenomena exists. But we don't need science for that. Many people just need to see something fuzzy in the corner of their eye to believe in ghosts. Others believe in paranormal phenomena because their neighbor said he saw a flying saucer. Science is applied when we want to verify that those beliefs are founded in real phenomena, not "seen", "experienced", "told", or "read about" phenomena. Science is applied when we want to know, when we want proof. That is what separates science from nonsense and knowledge from belief.

I suggest that a word-game such as the one displayed by Parker & Brusewitz, or anyone else, has one single purpose: to make nonsense appear as knowledge. That is to say, to give invalid claims the same status as valid ones. Note that they state that proof is impossible in empiric science. That

means that nothing we do know for certain about this world can be regarded as proven. So the search for proof is futile, not only in parapsychology, but in any field of science. By denying empirical findings a higher status (or a "better" term) than mere suggestions, hunches or agreements, Parker & Brusewitz hope to narrow the gap between phenomena we know exist and phenomena they want to exist. Thus, they seek to promote a science that is unable to produce verifiable findings by degrading sciences that are able.

Let's see this *modus operandi*, and some even more disgusting behavior, at work by looking into some of the studies in the compendium. In the following, I will address only Parker since I am assuming that Brusewitz is only decoration – people who don't know that the SSPR is a rather sad group of gullible Gellerites and UFO fetishists might be impressed by the name of its chairman. I'm not. And when I commented on this paper on a Swedish internet forum, only Parker came forward to defend it (although he did not actually defend it – instead, he suggested that we should discuss other things, he complained about not getting the Lund University parapsychology chair and he dropped a lot of names in the field of parapsychology, allegedly his friends.)

*"A possible exception [from studies providing strong evidence] might be the Delmore experiments but as we note below these have been defended by no less critic than Richard Wiseman. /.../ Parapsychologist and illusionist George Hansen was of the opinion that Delmore's success could be explained in this way [card skills] while parapsychologist and illusionist Richard Wiseman concluded after practical experimentation that the proposed method could not have been used."* (Parker & Brusewitz, 2003)

Please note what Parker is doing here. He is presenting a study that may not qualify as providing strong evidence but claims that Richard Wiseman has defended it. So his reason for having the Delmore tests on the list is that they were defended by Wiseman, a much respected skeptic. Parker even elaborates on this and claims that the critique from one parapsychologist and illusionist has been refuted by another parapsychologist and illusionist through experimentation – it is obvious that Parker is seeking rhetorical points by this repetition of titles. 1-1=0.

So what is it Wiseman has tested? In the paper referred to by Parker, Wiseman (1995) reports on how he tested his own notion on the possibility of Delmore having used a "foot shiner". Writes Wiseman:

*"While discussing the Delmore case, it occurred to me that the test conditions might not have prevented Delmore from using a 'shiner' attached to his foot."* (Wiseman, 1995)

From this, we can state that Wiseman did not test any idea proposed by Hansen. Further more, according to Parker, Hansen suggested "card skills" as an explanation for Delmore's test results. A "shiner" is a small mirror attached to the foot, it has nothing to do with "card skills". So not even in the context of Parker's own fabrication does it make sense. But has Wiseman defended the Delmore tests in any way? I asked Wiseman in an e-mail and got this reply:

*"You are correct. I only experimented with that one idea and Hansen's other (and many) criticisms of the tests are valid."* (Wiseman, 2006)

So Wiseman hasn't defended the Delmore tests at all, and in fact agrees with Hansen's critique. And when I corresponded with Hansen (2006) on the matter of Parker's paper, he commented on the fact that Parker do indeed make it sound as if Hansen has proposed the "shiner" while this is not the case. So Parker is making claims not only contradicted by

himself in the previous text, but also by his own reference – Wiseman's paper on the "shiner" test, and by Wiseman himself, corroborated by Hansen. A benevolent conclusion would be that Parker has pulled an "Ed Cox" – contrary to facts, he persists in promoting his own fantasy. A more probable conclusion is that Parker is engaging in deliberate deceit.

Hansen's (1992) critique of the Delmore tests goes far beyond a suggestion of "card skills" (link to Hansen article). Parker knows this, but tries to trivialize it. He also minimizes Delmore's capacity as a conjurer to him having "some, albeit apparently elementary, card skills" – thus displaying a fundamental ignorance of the time and practice necessary to execute the sort of sleight-of-hand techniques that Delmore bragged about and performed publicly.

The Delmore tests have no place on a list of studies providing evidence for PSI – whatever criteria for evidence you chose. But what is worse is that Parker, an alleged scientist, deliberately distorts verifiable sources, in this case using the authority of one of his more renowned and respected peers in a deceitful way. As I will show in the next blog, this is not an isolated incident, but a systematic way of fabricating reality that underlines the entire paper.

### **Targ's & Puthoff's remote viewing experiments** (Saturday 13)

I have shown that parapsychologist Adrian Parker engages in deliberate deception concerning the Delmore tests when he distorts the writings of his peers in order to turn seriously flawed research into "evidence" of paranormal phenomena. Let's continue with another post in Parker's (2003) compendium: Targ's & Puthoff's research on people claiming to be able to close their eyes and "see" distant places.

Remote viewing was launched in the 1970's mainly by physicists Russell Targ and Harold Puthoff at the Stanford Research Institute (not in any way associated with Stanford University). Targ and Puthoff claimed that remote viewing could be performed by anyone and that the very positive results of their research were replicable. In a remote viewing test procedure, someone (x) goes away to a location ("target") not possible to reach by ordinary sensory perception. Another person remains in the laboratory with the test subject. At a chosen time, the impression the test subject gets of the target is recorded. Usually the subject also produces sketches of the impressions he or she gets. A third person, a judge, then brings the subject's recorded impressions and/or sketch to the target and validates how well it corresponds with the location. Ordinarily, several targets are tested in one trial so that recordings and/or sketches can not be matched by other means than the impressions. If you omit the "secret intelligence" terminology used, remote viewing seems to be some sort of telepathy – the impressions x get of the location is somehow transferred to the subject (Nickell, 1992). In more imaginative anecdotes, remote viewers claim to be able to "see" every where, at any time and without anyone being at the target location.

In Parker's listing, the Targ & Puthoff remote viewing research is presented like this:

*"The first series of remote viewing experiments by Russel 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 levels but Marks and Scott insisted there were still some cues." (Parker & Brusewitz, 2003)*

From this, you get the impression that the critique raised against Targ's & Puthoff's research was refuted when Tart allegedly showed that the suggested flaws were superficial, but that the critics out of stubbornness maintained that there still was flaws. Was that really the case?

The Targ and Puthoff experiments were part of the government funded research at Stanford Research Institute (SRI) from the beginning of the 1970's until 1992, when the project was transferred to the Science Applications International Corporation (Wiseman, 1998). The tests Parker refers to were conducted during the first decade with alleged high scoring subjects like Pat Price and Hella Hamid. Some of them had been recruited from the Scientology Church, due to the fact that Puthoff at the time were a member of the sect (Alcock, 1998). Targ & Puthoff claimed that they had done hundreds of experiments and most of them had been successful. Some of the subjects performed amazingly well and one of them could even perform precognition by describing the targets, not only before they were visited, but before they were even chosen (Hines, 2003).

The SRI tests followed the standard design; when the subject reported his or her impressions, the recordings were handed to independent judges who then visited the target locations and validated the accuracy. Extrasensory perception was indicated when the judge were able to clearly link an assertion to a target location (Hines, 2003).

Impressed by Targ's & Puthoff's results, David Marks and Richard Kammann tried to replicate the tests with five subjects but failed to find scores beyond chance. Marks and Kammann had found it necessary to edit out information that could have provided the judges with cues to which targets had been visited, while Targ & Puthoff had reported that the subject records had been handed to the judges unedited. This means that if the judges in the Targ & Puthoff trials received transcripts with cues regarding the order in which the recordings had been made and, in addition, a non-randomized list of target locations, they could easily have matched the impressions with the targets, even if they were not consciously aware of the cues' significance.

Targ & Puthoff had reported that all transcripts were handed to the judges in random order, but when Marks visited SRI, one of the judges, Arthur Hastings, told him that the transcripts had been delivered in the order the targets had been visited during the tests. When Marks was able to read the transcripts from the trials with Price, he discovered a multitude of cues clearly indicating the order of the transcripts – for instance, in the third target transcript, reference was made to "yesterday's two targets". When Marks and Kammann conducted additional tests with the method used by Targ & Puthoff, five transcripts were perfectly matched to five targets (Alcock, 1998).

So what did Tart do? According to Parker, he conducted re-tests but omitted the cues and was still able to replicate Targ's & Puthoff's results. The problem is that no one was actually able to verify this – Targ & Puthoff refused to submit data until July 1985 and Tart had in part used material already public and even published (Hines, 2003).

But the question of cues in the transcripts is only one of several charges brought against the Targ & Puthoff remote viewing research. Alcock (1998) suggests four other serious flaws.

First, the tests were not conducted independently of each other. For instance, the subjects were taken to the target locations and received immediate feedback after each impression had been recorded. Thus, subsequent statements were not independent of prior targets. Hastings had also told that different subjects tended to focus on different factors. One was focused on architectural and topographical factors, while

another focused on x's behavior. In addition, the subjects' names were noted in the header of the transcripts, which might have helped the judges.

Second, when analyzing the Hammid tests, Marks and Kammann found that sketches were missing for three out of six tests. They also found references to additional tests with Hammid that had not been accounted for by Targ & Puthoff. In the so called Technology tests, they found that anything from one to five tests with five subjects was reported. Why had Targ & Puthoff reported only on one of five tests with three of the subjects, four out of five with a fourth and all five with Hammid? Sketches were also missing from the records of these tests.

Third, there was no control or control groups, and thus no reference or relation to lack of remote viewing occurrence. A subject might for example have been asked to make two statements, one for a real target and one for a fictitious – without revealing to the subject that one of the targets did not exist. The judges would then have had to evaluate the "fake" statements too, resulting in a much more reliable notion of whether something paranormal really had occurred. There were also indications that the tests and the data analysis was subject to considerable sloppiness.

Last, but not least, the evaluations were completely subjective and Marks and Kammann noticed, during their own tests, that both x and the judge could feel very strongly for a correlation between subject and target, a correlation that de facto did not exist.

The best summary of the Targ & Puthoff remote viewing tests is perhaps Alcock's own words:

*"Given these various criticisms, there should remain little doubt that the Targ-Puthoff studies are fatally flawed, and that rather than trying to save something from them by arguing whether or not a given flaw pertains to a given subset of trials, remote viewing proponents should instead design and run a proper, well-controlled experiment with an appropriate control group."* (Alcock, 1988)

What is evident in the Parker compendium, is the fact that he again belittles the rather massive criticism raised against a study he lists as giving evidence for psi. And again he claims that the fragment of criticism he do mention, has been refuted. What is compelling regarding the Targ & Puthoff research is that it so obviously constitutes "crank science". They refuse to submit data when requested – as they did in their Uri Geller "tests" too. All their research, not just the remote viewing experiments, shows fundamental methodological flaws. They have rightfully been called the Laurel & Hardy of parapsychology (Randi, 1982). But what do you call a scientist that refers to those clowns' activities as "evidence for psi"? I don't know if Parker's merits stretches beyond psychology but any serious scientist engaging in experimental research should be able to recognize crap science when confronted with it. Targ's & Puthoff's "research" is without doubt utter crap but Parker doesn't want to see it. What does that make Parker?

#### **The Schmidt Experiments (Sunday 14)**

So far, I have pointed to the fact that Swedish parapsychologist Adrian Parker (Parker & Brusewitz, 2003) engages in deception and belittling of the criticism raised against the studies he lists as evidence for paranormal phenomena in "A Compendium of the Evidence for psi". In doing this, I have also made it clear that the Bill Delmore tests and the Targ & Puthoff experiments on remote viewing does not constitute evidence of any kind, to any degree – they are only evidence of crap science. It is now time to add yet another feature of Parker's: lack of methodological insight.

In the compendium, Parker writes:

*"The RNG experiments by Helmut Schmidt have retained their status and were replicated by him many times."* (Parker & Brusewitz, 2003)

Before I disclose the nature of the status the Schmidt experiments have retained, let's consider what methodological status Parker displays.

Now, he has obviously learned that replicability is something that is important in experimental research. But replicability means that the outcome of a study must occur again if the study is replicated by someone else. And here is Parker, senior lecturer at Gothenburg University, rendering credibility to a study that has been replicated by the same researcher over and over again! Parker doesn't have the methodological insight to realize that Schmidt can replicate his own studies for all eternity – they achieve validity only when they are replicated by someone other than Schmidt. How much credibility are we to render a researcher that lacks such fundamental knowledge in methodology?

So, what has Schmidt done and what is the status of his doings? "RNG" means Random Number Generator and is subsequently an instrument that generates random numbers (Journal of Parapsychology, 2003). Schmidt used RNGs to turn on one of several lights. In the precognition tests, the subject pressed a button to predict which light would turn on and in the clairvoyance tests, the light that would be turned on is decided before the subject responds (Hines, 2003).

The criticism of Schmidt's experiments is extensive. Most of Schmidt's studies lack control or control group, immediate feedback is a matter of routine, thorough analysis of data is missing, he works almost isolated from other researchers, except for his 1986 study data is not available to other researchers, the "Modulus 4" generator he used produces an excessive number of 4 compared to 1, 2 and 3 – in several cases it is number 4 that represent the significant result, he totally ignores suggestions on improvement of his methods, he sometimes acts as both experimenter and subject, in order to create a "auspicious environment" he lets subjects have free access to experiment equipment and in some cases subjects have conducted tests on their own without any experimenter present. Alcock concludes:

*"My review of this database leads me to conclude that there is no evidence in any of these REG studies of any effect which needs explanation by reference to PSI forces. None of the studies as they stand would be accepted for publication in a good psychology research journal, in my view, quite apart from their subject matter. They are all flawed, some terribly so."* (Alcock, 1988)

Parker notes that Palmer (1996) has rejected one bias hypothesis, but neglects to mention that Palmer (1997) himself suggests another bias hypothesis a year later.

It is evident that Parker's strategy is to belittle the criticism raised against the studies he lists, in the Schmidt example labeled "Some Well Controlled Proof Oriented Experiments." There is no sufficient control in Schmidt's studies at all! That is the real status the Schmidt experiments have retained.

Further more, in this case it is equally evident that Parker lacks fundamental methodological insight – the Schmidt experiments "were replicated by himself many times"!

#### **Adrian Parker replies (Tuesday 16)**

Tonight, I received a comment on my criticism from Adrian Parker. It was sent to my Hotmail, with the stated option to publish it if I so choose. I do. So here is Adrian Parker's reply:

*"Thanks for your skeptical notes. I am also a Swedish sceptic so I am thankful for your criticisms, some of which I think are at least in some measure, valid. I do however note a slight tone of animosity which makes you respond in danger of losing all its effect. For my part, I have to apologize if my joint paper with Goran seemed just too positive for your taste. However, we repeatedly have said that psi is not proven: I find some of the experiments persuasive to the degree of making me want to do further research. If I am fooling myself I want to know and research seems a better way than armchair criticism or concerning myself with what comes over at times as rather fanatical criticism. Therein lies the true difference between us. Otherwise, let me admit again: you are right that Wiseman rather Hansen thought of the shoe shiner, but once again I ask: Why is this really so very important to you? The shoe shiner was their most promising counter hypothesis and the rejection of that hypothesis was defended by Wiseman. To that extent I was correct about 'Wiseman's defence' but you are correct my statement should have been more precise and less misleading. Of course we can all come up with other cheating scenarios but none of these easily explain Delmore's very high scores on the RNG. Nevertheless I regarded these experiments as a possible exception to valid evidence.*

*I note that you never mention that Hansen has himself even as a skeptic believes that the border between what he regards as a genuine psi and magical skills (with in some cause even the use fraud) is a fleeting one. This I hope we can agree is, at least in this context, a cop out, but at least you see the diversity of opinion even amongst magicians such as Hansen. But why make so much of this when I said myself these experiments were controversial and a possible exception? The same is true of the early Targ work which does not figure in my proper list and is mentioned included for historical reasons and then the references to both sides of the controversy were given. I do however thank you for pointing out the insufficiently of our statement that the Schmidt RNG experiments were replicated by himself many times. Despite the apparent safe guards, I agree, it is crucially important that they replicated by others and carried out under the critical eye of skeptics and of the three references that were given, one concerned just such conditions of critical observers.*

*You are welcome to publish this in its completeness but I hope you understand I have no further time to spend on such debates. This means that you can of course continue with slander (before doing so you might like to ponder why you have 0 comments to your blog) but I hope you have the good nature not to do so and instead see our areas of common concern. I take note of your criticism and should you choose to use your real name, I suggest that we send a joint note to the EJP acknowledging the above points."*  
(Adrian Parker, January 16th, 2007)

### **Response to Adrian Parker (Tuesday 16)**

First of all, I must thank Adrian Parker for taking the time to comment on the criticism I have put forward regarding his paper "A Compendium of the Evidence for psi" (Parker & Brusewitz, 2003). In a previous discussion, Parker tended to discuss anything but the paper, so I am also thankful for the fact that he restrains himself to the issues questioned. However, as his reply is ridden by the same rhetorical markers as his paper, I am compelled to consider it, not a clarification, but a smoke screen. As Parker has announced that he does not have the time to involve himself in further debate, I will respond to his reply in the form of statements rather than questions.

*"I am also a Swedish sceptic so I am thankful for your criticisms, some of which I think are at least in some measure, valid. I do however note a slight tone of animosity which makes you respond in danger of losing all its effect."*

In a world where words have no meaning, you can call yourself what you want. And I can call myself a senior lecturer at Gothenburg University. It's all fine and dandy, but unfortunately, it's make-believe. In your paper, you have shown that you praise research that even the more gullible of your peers consider worthless. Regarding the studies I have discussed so far, you systematically neglect the multitude of critique raised against them and claim that the fragment of doubt that you do convey have been refuted. If social psychology were to look for a materialization of "confirmation bias", you would be the first in line (you can look up "confirmation bias" in any introduction to social psychology, Mr. Parker). When confronted with a paranormal claim, your impulse is to salute it and pay homage to it without reservation. Mine is to investigate the claim, pretty much like I'm checking your compendium now. By calling yourself a "skeptic", you hope to avoid controversy, but to be a "skeptic" you have to be able to employ rational and critical thinking. So "skeptic" you are not, whatever you choose to call yourself.

*"For my part, I have to apologize if my joint paper with Goran seemed just too positive for your taste. However, we repeatedly have said that psi is not proven: I find some of the experiments persuasive to the degree of making me want to do further research."*

I'm aware that you state that psi has not been proven. But you also state that it is impossible to prove phenomena in empirical science – in any empirical science – so by extinguishing proof as a possibility, you render your statement about proving psi worthless. What you do consider possible is for research to provide evidence, in the case of your listed studies strong but not compelling. Whatever wordplay you choose, the quality of findings in parapsychology will be compared to the quality of findings in other fields.

It is of course hilarious that you have been persuaded by the experiments you have listed, given that you label yourself a "skeptic". If crap science and almost total lack of methodological stringency has that effect on you, no wonder you produce papers like the one at hand and get offended by criticism.

*"If I am fooling myself I want to know and research seems a better way than armchair criticism or concerning myself with what comes over at times as rather fanatical criticism. Therein lies the true difference between us."*

Of course you are fooling yourself. But my problem is that you are trying to fool others, by deceit and cover-up. And you are doing it by posing as a scientist.

I appreciate your effort to belittle my argument by calling it "armchair criticism" – it is completely in line with the strategy employed in your paper. But since your paper in itself is a result of "armchair science", it is only appropriate that it is confronted with "armchair criticism". I take it you are not conceited enough to label "A Compendium of the Evidence for psi" experimental research. Besides, it seems that the data of your "armchair research" is outperformed by the data of my "armchair criticism".

*"Otherwise, let me admit again: you are right that Wiseman rather Hansen thought of the shoe shiner, but once again I ask: Why is this really so very important to you? The shoe shiner was their most promising counter hypothesis and the rejection of that hypothesis was defended by Wiseman. To that extent I was correct about 'Wiseman's defense' but you are correct my statement should have been more precise and less misleading. Of course we can all come up with other cheating scenarios but none of these easily explain Delmore's very high scores on the RNG. Nevertheless I regarded these experiments as a possible exception to valid evidence."*

The matter of the "shoe shiner" is important because you claim that Wiseman has refuted something that Hansen has suggested. That is simply not true. Further more, you claim that the Wiseman paper constitutes a defense of the Delmore tests. It does not, in any respect. It is simply a test of Wiseman's own notion and he emphasizes that Hansen's critique – all of it – is valid. The "shoe shiner" was not their most promising counter hypothesis, not even a joint one – that is something you make up as you go along. I repeat: the "shoe shiner" was Wiseman's own idea, tested by himself.

I have not suggested that your statement should be more precise and less misleading. I am claiming that you are deliberately lying and exploiting a well-respected peer's name to promote crap science.

"Delmore's very high scores" doesn't add up to anything since the Delmore tests were seriously flawed in many ways – something you intentionally neglect to mention in your paper and still don't understand. You have not regarded the Delmore tests as exception to valid evidence – you use the authority of a methodologically superior peer, and the fake position you put him in, to include them. That is just plain nasty.

*"I note that you never mention that Hansen has himself even as a skeptic believes that the border between what he regards as a genuine psi and magical skills (with in some cause even the use fraud) is a fleeting one. This I hope we can agree is, at least in this context, a cop out, but at least you see the diversity of opinion even amongst magicians such as Hansen."*

I do not offer my agreement to anything you write without proper references.

*"But why make so much of this when I said myself these experiments were controversial and a possible exception?"*

The principle behind your question is precisely why I make so much of it. You have omitted the final, and vital, part of what you said. I quote: "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." You are in essence stating that since Wiseman has defended the Delmore tests, you include them in your listing. Again: nasty, plain nasty.

*"The same is true of the early Targ work which does not figure in my proper list and is mentioned included for historical reasons and then the references to both sides of the controversy were given."*

Uhm, now you introduce a "proper list"! But that wasn't published in EJP and nowhere else for that matter. So I'm sticking to what you actually have published. And in that paper, you list the "early" Targ & Puthoff remote viewing tests as evidence for PSI. I trust that your listing of studies providing evidence doesn't list studies that do not provide evidence.

*"I do however thank you for pointing out the insufficiency of our statement that the Schmidt RNG experiments were replicated by himself many times. Despite the apparent safe guards, I agree, it is crucially important that they replicated by others and carried out under the critical eye of skeptics and of the three references that were given, one concerned just such conditions of critical observers."*

The Schmidt experiments, as well as the Delmore tests, and the Targ & Puthoff research on remote viewing, does not constitute evidence of any kind, to any degree. Those studies are seriously flawed and any serious researcher with integrity should distance him- or herself from them. You don't.

*"You are welcome to publish this in its completeness but I hope you understand I have no further time to spend on such debates. This means that you can of course continue with slander (before doing so you might like to ponder why you have 0 comments to your blog) but I hope you have the good nature not to do so and instead see our areas of common concern. I take note of your criticism and should you choose to use your real name, I suggest that we send a joint note to the EJP acknowledging the above points."*

As I suspect that you label any criticism of your "work" slander, I will disregard your remark.

As far as the number of comments on my blog goes, it has only been active since Christmas and I already have the second most renowned parapsychologist in Sweden commenting it. And I have returned the favor by being the only one paying any attention to your paper. But I take it you consider that silence as a token of compliance.

I have no interest in doing anything jointly with you. I have no respect whatsoever for you as a scholar or researcher. Such a venture would at best make you able to forward my real identity to your woo-woo followers, at worst let you feed of my efforts that apparently exceeds your own in stringency by far. So I humbly decline your invitation. To acknowledge the above points in a note to EJP is, again, to belittle what should be done. Anything less than an unretracted retraction is futile.

### **Some Final Notes (Monday 22)**

In "A Compendium of the Evidence for PSI", Swedish para-psychologist Adrian Parker (Parker & Brusewitz, 2003) claims that most of the studies listed "would still [today] be quoted as providing strong evidence" of paranormal phenomena. As I have shown, that is simply not true. Both the Targ & Puthoff research, and the Schmidt studies are so flawed that referring to them as evidence of any kind must be considered, at least, naive beyond comprehension. But Parker is not naive; there is something very explicit and intentional in the way he perverts what Wiseman has written on the Delmore tests. And the systematic belittling of the criticism raised against the studies listed is far from accidental. Parker is out on a mission and the end justifies the means, even if they include deception.

Consider the Maimonides dream experiments. Taylor (1981) points to the fact that significant results don't matter if they are derived from subjective judging, as was the case in the Maimonides studies. Others have noted violation against experimental protocol as well as lack of replication (Hines, 2003). But Parker claims that no fatal flaw has been discovered regarding these studies. It's that easy – just stick your head in the sand.

The same goes for the Brugman experiments during the early 1920's. Parker conveniently leaves out that the subject, van Dam, was a performing magician specializing in finding hidden objects using unconscious cues from others. There were also indications that the targets were selected non-randomly (Björkhem & Johnson, 1986). But Parker claims that no flaws have been discovered in the Brugman studies.

In the case of the research at Duke University, Parker claims that it "requires special comment since there are so many misconceptions surrounding it" (Parker & Brusewitz, 2003). But Parker does not account for any such misconceptions or the possible relevance they have for his compendium. It may be that he feels obligated to assign a certain amount of text to Rhine's research, due to the lab's historical significance. Or maybe it's just a way to create an illusion of credibility regarding the Rhine research. In any case, no study conducted at Duke University would be considered

providing evidence of psi by serious researchers. The time before 1940, which Parker claims was a time of "experimental achievement," was in fact a period of immense sloppiness. For instance, the first editions of Zener cards used had such bad printing that the figures could be seen on the back due to an embossing effect or through the cards due to poor paper quality (Hines, 2003).

Poor experiment control, lack of replication, self-deception and wishful thinking marked the entire lab, before and after 1940. The most evident flaw, however, is perhaps best noted by Rawcliffe:

"Yet it is on the question of safeguards against sensory cues that all ESP experimenters are shown to be at fault. None of them appear to have studied this problem seriously and their claims to have 'obviated' all sensory cues are often pathetic in its naivety and evident sincerity. Pathetic too is their much advertised confidence that only parapsychologists can fully appreciate the problems raised by the exclusion of sensory cues in the ESP experimental situation. It is perhaps significant that nearly all the competent work on this important question has been carried out by individuals who were not parapsychologists at all." (Rawcliffe, 1959)

As usual, Parker tries to make it appear as if Hansel is the only one who has put forward severe criticism. In reality, the Rhine research has been scrutinized and criticized by so many researchers that even Rhine himself probably would have admitted most of the flaws. But not Parker.

It is evident that "A Compendium of the Evidence for psi" is not worth the paper it's written on. But, you may argue, it has been published in an alleged scientific journal – the *European Journal of Parapsychology*! There must be something to it if an editor has decided to publish it! So, who was the editor who published Adrian Parker's paper? According to the journal website, the editor that year was... uh, blimey! It was Adrian Parker who published Adrian Parker!

Adrian Parker is an illustrative example of what I think is fundamentally wrong with parapsychology as a field of science. First of all, too many parapsychologists are reluctant to distance themselves from the obvious con-men and frauds – "high scoring subjects", in the past and in the present. There is no scientific benefit in promoting scam-artists, or in treating them with some kind of "scientific respect". They are conjurers and belong behind bars, not in research labs.

Secondly, too many parapsychologists are reluctant to distance themselves from their crackpot colleagues. For instance, Adrian Parker goes around thinking that the reason his compendium has not been refuted is because it is supported by his peers. Having corresponded with some of them, it seems that very few, if any, has even read the paper. Thus, a crap paper is unchallenged and the blame is on the competent researchers who ignores it, not the incompetent who wrote it – he can't help himself. In the end, parapsychology as a field of science suffers and the methodological researcher has to share the title of 'parapsychologist' with the crank.

If parapsychology is to have a future as a scientific discipline, this has to change. The Adrian Parkers of the field has to be recognized and challenged.

Openly and often.

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## Comments:

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*Måns said...*

"before doing so you might like to ponder why you have 0 comments to your blog".

This is by far my favourite part of Parker's reply. As if the validity of your criticism of his paper had anything to do with how many people commented on it...

Looking forward to reading the rest of your review of the paper!

January 16, 2007 12:53 PM

*John Stalberg said...*

"This means that you can of course continue with slander (before doing so you might like to ponder why you have 0 comments to your blog) but I hope you have the good nature not to do so and instead see our areas of common concern."

I, a reader, had no intention to write any comments here.

This comment made me change my mind. The reason I didn't have any intention to comment is that there is not much more to say really, from my point of view!

Parker is either a fraudulent researcher or a person suffering from denial. Either a scammer or else someone that is in a state of denial which render him unable to discuss the

research itself in a manner which is meaningful in the common sense? Or this might be a little bit of both?

There is nothing more to add, as I see it, since it will then reach the point of beating a dead horse! Thank you for going through this crap!

*March 4, 2010 7:35 PM*