

# Susan Blackmore -- The Elusive Open Mind: Ten Years of Negative Research in Parapsychology

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*What does a psychologist who's had an extraordinary experience do?  
Sets up a research program to test for psi. The lessons are surprising.*

EVERYONE THINKS they are open-minded. Scientists in particular like to think they have open minds, but we know from psychology that this is just one of those attributes that people like to apply to themselves. We shouldn't perhaps have to worry about it at all, except that parapsychology forces one to ask, "Do I believe in this, do I disbelieve in this, or do I have an open mind?"

The research I have done during the past ten or twelve years serves as well as any other research to show up some of parapsychology's peculiar problems and even, perhaps, some possible solutions.

I became hooked on the subject when I first went up to Oxford to read physiology and psychology. I began running the Oxford University Society for Psychical Research (OUSPR), finding witches, druids, psychics, clairvoyants, and even a few real live psychical researchers to come to talk to us. We had Ouija board sessions, went exploring in graveyards, and did some experiments on ESP and psychokinesis (PK).

Within a few weeks I had not only learned a lot about the occult and the paranormal, but I had an experience that was to have a lasting effect on me — an out-of-body experience (OBE). It happened while I was wide awake, sitting talking to friends. It lasted about three hours and included everything from a typical "astral projection," complete with silver cord and duplicate body, to free-floating flying, and finally to a mystical experience.

It was clear to me that the doctrine of astral projection, with its astral bodies floating about on astral planes, was intellectually unsatisfactory. But to dismiss the experience as "just imagination" would be impossible without being dishonest about how it had felt at the time. It had felt quite real. Everything looked clear and vivid, and I was able to think and speak quite clearly.

You can imagine the intellectual conflict I experienced (and of course I had no idea it was only a prelude to far worse mental conflicts!). The psychologists and physiologists who were teaching me made quite different assumptions about human nature from those made by the people I met through the OUSPR. The latter, for the most part, assume that there is "another dimension" to man, that we can communicate directly mind to mind, that there are "other worlds" waiting to be explored in altered states of consciousness, and even that consciousness is separable from its physical home and might survive the death of its body. The conflict was a challenge to me and I conceived the objective (I think naively, rather than purely arrogantly) of proving my teachers wrong, or at least showing that psychologists were closed-minded in ignoring the most important of human potentials—the paranormal.

Even at that very early stage I made a crucial mistake — or a series of crucial and related mistakes. First, I assumed that all these odd and inexplicable things — ESP, PK, OBEs, mystical experiences, ghosts, poltergeists, and near-death experiences — were related and that one explanation would do for all. Second, I assumed that there had to be a

paranormal explanation — that we were looking for psi. Third (and I don't know whether this was just cowardice or an attempt at being sensible for a change), rather than launching straight into what really interested me — the OBE—I thought it was more "scientific" to begin with psi. After all, there had been research done on ESP and PK and, though generally rejected, it had some basis in scientific research. It seemed far easier, and safer, to start there. I didn't notice what I was doing. I can only point it out with the benefit of hindsight. I just took psi to be the key to the mysteries and wanted to study parapsychology.

The first thing I did was to develop my own theory of psi. This theory involved the notion that psi and memory are aspects of the same process, that memory is a specific instance of the more general process of ESP. Eventually I got a place at Surrey University to do a Ph.D., and it was then that I set about testing my theory.

While I was at Surrey I was lucky enough to be given the chance to teach a parapsychology class. It attracted more than a hundred students. so I had plenty of subjects for my experiments. I began three kinds of tests. First, I predicted a positive correlation between ESP and memory. That is, if memory and ESP are aspects of the same process, then the same people should be good at both of them. I did many tests of this kind (Blackmore 1980a). Second, I predicted that the best target materials for ESP should not be those that are easy to perceive, but those that are easy to remember. I did a series of experiments with different target materials (Blackmore 1981a). Third I predicted that the errors and confusions made in ESP should more closely resemble those made in memory than those made in perception. I had high hopes for this method since the study of errors has always been so useful in psychology, for example, in the study of visual illusions. I also did many experiments to test this (Blackmore 1981b). However, the only noteworthy thing about all of the results was the number that were not significant.

After a long series of experiments I had no replicable findings and only a large collection of negative results. Clearly they could not answer my original questions. nor test my special theory. Some of you may already be protesting: What an idiot. Why didn't she just give up and do something useful instead? But I would have responded: This could be useful! If ESP exists, it could be one of the most important findings for science; and in any case you can never tell in advance what research will be useful in the end. You may also be thinking, as many people said at the time: "Oh but this is just what you'd expect. She has only shown that there is no psi." But of course I hadn't done that, and couldn't do that. No amount of negative results can prove the nonexistence of psi. Psi might always be right around the next corner, and there were plenty of corners to look around.

There were also plenty of parapsychologists eager to suggest corners I had not yet turned and reasons why my experiments had not worked. And I was eager to carry on the search. Some said it might be the subjects; students are notoriously not the best ones. So, instead of testing my class, I tested people who came to me with claims of special powers. I tried to design experiments that would test what they claimed to be able to do and that would allow me to impose sufficient controls. In some ways this upset me more than anything, because I met lots of genuine and well-meaning people who were convinced they could communicate by telepathy, or find underground pipes or hidden water, until they tried to do it under conditions that ruled out normal sensory information. Then they, and I, were always disappointed.

Then I tried using young children as subjects. At that time, Ernesto Spinelli was getting outstandingly good results with preschool children in ESP tests (Spinelli 1983). So I set

about designing experiments to use a method similar to his (though not a direct replication) to test my memory theory. It was much harder work than the previous experiments, but much more fun. The children were three- to five-year-olds in playgroups, and they thoroughly entered into the whole idea, being convinced they could transmit pictures to one another. But the results were quite clear. The proportion that were "nonsignificant" was as high as before. The overall results were nonsignificant and so were the correlations with age (Blackmore 1980b).

Why? Spinelli had many suggestions. It could have been that I used colored pictures, while his were black and white; or that the sweets I used as a reward (based on someone else's previously successful experiments) were too well liked by the children and were disruptive; or that I simply didn't have the right personality and rapport with the children. I could only say that I seemed to get on well with the children, but perhaps this was not well enough.

Another suggestion was that the problem was not the subjects themselves, but the state of mind they were in during the experiments. At that time, the ganzfeld experiments were the "latest thing," and the results from Carl Sargent (1980) at Cambridge, and Chuck Honorton (1977) at Princeton, seemed impressive. So I set about doing a ganzfeld study. My subjects each had half of a ping-pong ball covering each eye, lay on a reclining chair, and heard only white noise fed through headphones. I wrote down everything they said. Then they had to look at four pictures and choose which one they thought the agent had been looking at.

I had for some months led an imagery training group, in which we practiced relaxation, guided imagery, and many imagery tasks adapted from Buddhist training techniques. For my ganzfeld study I chose ten test subjects from this group and ten control subjects.

This study taught me a lot. Being in ganzfeld is in itself an interesting experience. Images come pouring in, and it is tempting to imagine that you are picking them up from somewhere outside of yourself. I also had one very impressive experience in which I was subject and my brother was agent. I "saw" people fishing, lakes, mountains, and swiss chalets, and when I saw the targets I picked the correct one right away. It was an amazingly close hit. It set me to wondering whether I had at last found the key! However, in the course of the experiment I saw many equally amazing correspondences, but to the wrong pictures. My remarkable hit rapidly disappeared among the chance scores.

This should have taught me something important, something I should have known all along; that is, one should not rely on subjective estimations of probability (see Blackmore and Troscianko 1985). One should rely only on the statistics, and they were telling me that there was nothing there. Of course I tried it again with my brother, but the second time it did not work. Overall the results were close to chance expectation.

Why did this study also fail? I had used trained subjects in psi-conducive conditions and a method others had found successful. The ultimate suggestion of most parapsychologists was that it was an experimenter effect — more than that, it was a psi-mediated experimenter effect. That is, either I was using my own negative psi or I had some kind of personality defect, or defect in belief, that suppressed the psi of other people. I was a psi-inhibitory experimenter, so that whatever I did I would always get negative results. I began to get the feeling that I had some creeping sickness. I was a failure, a reject; there was something in me that suppressed the true spiritual nature of other people. I tried not to let it

upset me, but I must admit that there is something terribly unflattering about being labeled "psi-inhibitory"!

Well, what could I do about it? It is not entirely an untestable idea. But Sargent had already tested the personalities of successful and unsuccessful experimenters and found the successful ones to be extroverted, confident, nonneurotic, and so on. In fact I fitted the description quite well — except for my results.

The other key to my failures seemed to be belief. I was told that I didn't get results because I didn't believe strongly enough in psi, because I didn't have an open mind! But what could I do about that? I couldn't just change my beliefs overnight or test ten subjects while believing and another ten while not! I argued that in the beginning I had believed in psi and still had got no results, but I couldn't prove this against the counter-argument that I had never really believed at all.

However, I did have an idea. There were still things in which I did believe. I could test the Tarot. In my preoccupation with everything occult, I had been reading Tarot cards for about eight or nine years. They really did seem to work. People told me that I could accurately describe them using the cards, and this was, naturally, gratifying. I even thought it might have a paranormal basis. So I set about testing the cards, doing readings for ten people, keeping the procedure as close as possible to a normal Tarot reading, but isolating myself, as the reader, from the subjects. They then had to rank all ten readings to see whether they picked their own more often than chance would predict (Blackmore 1983).

It worked! The results were actually significant. You can imagine my excitement — perhaps I had at last found something. Perhaps there was no psi to be found in the standard laboratory experiments, but something paranormal could appear when the conditions were closer to real life. But then I talked to Carl Sargent. He pointed out that all my subjects knew one another, and if they knew one another their ratings and rankings could not be independent. So I had violated an assumption of the statistical test I was using.

This seemed so trivial. Their knowing one another could not help them pick the right reading, could it? No it couldn't; but this meant that the estimate of probability was inaccurate — and, after all, the results were only marginally significant. So I repeated the experiment twice more with subjects who did not know one another. I expect you can predict the results I obtained — entirely nonsignificant.

You may choose to interpret these results in different ways. Some parapsychologists have claimed that the first experiment found genuine psi and that the later ones didn't summon the same attitude, the same novelty, the same enthusiasm, that made psi possible—or even that psi itself doesn't like being replicated. But I think I had finally reached a stage where I no longer felt it was worth pursuing such arguments. I chose this point to say: "I think that, however many more experiments I do on psi, I am probably not going to find it."

Now we finally come to the question: "What do these negative results tell us?" Of course the one thing they do not tell us is that psi does not exist. However long I went on looking for psi and not finding it they could not tell us that. But I found myself simply not believing in psi anymore. I really had become a disbeliever. Like one of those doors with a heavy spring that keeps it closed, my mind seemed to have changed from closed belief to closed disbelief.

But either way I suffered. There was mental conflict whether I believed or disbelieved. I had many questions. One was this: How far could I generalize these negative results? The situation was the converse of the normal situation in science when one gets positive results and has to ask how far they can be generalized. Here I had to ask whether my negative results applied only to those experiments carried out by me, at those particular times, or whether they applied to the whole of parapsychology. There is no obvious answer to that question. If one had replicability one could answer the question as one does in other areas of science. But without replicability it is impossible.

The next question was: How could I weigh my own results against the results of other people, bearing in mind that mine tended to be negative ones while everyone else's seemed to be positive ones? I had to find some kind of balance here. At one extreme I could not just believe my own results and ignore everyone else's. That would make science impossible. Science cannot predict the results operate unless people generally believe other people's results. Science is, and has to be, a collective enterprise.

At the other extreme I could not believe everyone else's results and ignore my own. That would be even more pointless. There would have been no point in all those years of experiments if I didn't take my own results seriously. Indeed, it is a fundamental principle in science that one has to take notice of the results one finds.

So there is no right answer to how to weigh them up. And these problems are only aspects of the basic dilemma of parapsychology. which is whether to believe or disbelieve in the existence of psi. Either way, I suggest, one meets conflict.

In the believer's position one is saying: "I believe there is something negatively defined, defined as communication without the use of the recognized senses, or action without the use of the muscles of the body. I have faith that future experiments will find this thing, even though so far they have failed to produce a replicable effect." If one takes this position, then one not only has to accept the open-ended nature of the search but also has to face up to the mounting negative results.

But what about the disbeliever's position? The disbeliever is only saying: "I do not believe there is this negatively defined thing. I do not believe the search will be successful. I have faith that all experiments with positive results could be successfully debunked." So the disbeliever is in a kind of mirror image of the believer's position. But of course one can never debunk all the experiments, and there will always be more in the future. So the search is equally open-ended. And the disbeliever has to take notice of those positive results. I am thinking particularly of the results of Carl Sargent, Charles Honorton, Helmut Schmidt, and Robert Jahn. I suggest that if we think these can easily be dismissed then we are only deluding ourselves. One cannot offer simplistic counterexplanations and throw all these results away. I am not saying that these results may not, in the future, succumb to some normal explanation; they may well do so. But at the moment we do not have such an explanation.

Whether you are a believer or a disbeliever you will suffer mental conflict and anguish. So what is the solution? Easy, isn't it? Have an open mind. But human beings are not built to have open minds. If they try to have open minds they experience cognitive dissonance. Leon Festinger (1957) first used this term. He argued that people strive to make their beliefs and actions consistent and when there is inconsistency they experience this unpleasant state of "cognitive dissonance," and they then use lots of ploys to reduce it. I have to admit I have become rather familiar with some of them.

First there is premature closure. You can just pick one theory and stick to it against all odds. But I could not do that after all those years. What I could do was only slightly more subtle; that is, I could prefer one theory and ignore the evidence that goes against it. In this way the believer can dismiss negative results by using all the old arguments: The time, the place, the emotional state, or the "vibes" weren't right. Or the disbeliever can refuse to look at the positive results. You may think I wouldn't refuse, but I have to admit that when the Journal of Parapsychology arrives with reports of Helmut Schmidt's positive findings I begin to feel uncomfortable and am quite apt to put it away "to read tomorrow."

Alternatively one can jump on a simple counterexplanation, such as "It's all fraud and delusion." Well, maybe it is, but that too creates dissonance of its own. To go around thinking that everyone is cheating, or deluding themselves, can turn one into a permanently suspicious and miserable sort of person, and it can damage one's self-esteem. Suspecting that some effect is fraudulent and tracking that down systematically is one thing, but approaching everything one hears about as though it must be fraud is destructive.

Then there are other cheap ploys. You can decrease the perceived attractiveness of the opposition. The believer can find it easy to put down one famous critic as a dried-up old professor with no real contact with the field anymore, or a more recent one as having shifty eyes and too bushy a beard! Or the disbeliever can dismiss research on the grounds that all parapsychologists are Scientologists, or are too committed to religious beliefs, or are too dreamy-eyed and vague to be doing "real science." But none of this will really wash. And most of us know it won't. Nevertheless, we go on doing it because it is so very hard to have an open mind.

I have said rather a lot about what negative results do not tell us, but is there anything they do tell us? I think we are now in a position to see that there is. I suggest that, wherever you start in parapsychology, if you base your research on the psi hypothesis then you will be forced to do ever more and more restricted research, to back up into ever less and less testable positions, and to produce ever more feeble and flimsy buttresses to hold your theory together. In the end, whatever the questions you started with, you are forced to ask more and more boring questions until there is only one question left: Does psi exist? That question, I submit, is unanswerable.

This process is not restricted to those who get negative results. Helmut Schmidt is among the best researchers in parapsychology, and he has been forced to ask the question "Does psi exist?" Charles Honorton is another example. He is working on fraud-proof, fully automated procedures, even though he might prefer, as do most people in parapsychology, to do process oriented research, as I did when I started with my question "Is ESP like memory?"

I think that is the problem with parapsychology, and it is a problem that starts from the very hypothesis of psi. The structure and definitions of parapsychology are to blame. The negative definition of psi, the hundred years of bolstering failing theories, and the powerful will to find something are at fault. They not only force us to ask, "Does psi exist?" but force us to answer in terms of belief. Where there is no rational and convincing answer, belief takes over, and that is why there are two sides, and such misunderstanding.

Here, it seems to me, lies the crux. All those negative results teach us only one thing, that we have been asking the wrong question. And the whole history of parapsychology looks

like a string of wrong questions. Parapsychology is, if it is based on the psi hypothesis, a magnificent failure; not because psi doesn't exist, but because it asks unanswerable questions.

An entirely different aspect of my research was prompted by my personal out-of-body experience. I never entirely forgot it. I went on wanting to understand it and eventually tried to tackle it directly.

The first question I asked was the obvious one: "Does anything leave the body in an OBE?" This question may seem close to the unanswerable "Does psi exist?" but I think it is different enough, or perhaps I was just more ruthless in trying to answer it. From experiments of my own, and from reading the literature, I concluded that we do have an answer. And it is "No." You may have heard about an isolated incident of an OBE when someone correctly read a five-digit number (Tart 1968), or when a cat responded to its owner's out-of-body presence (Morris et al. 1978), but I prefer to look at the whole body of evidence (see Blackmore 1982). I concluded that these were unreplicable and that in general we have enough evidence to answer that there is no real evidence for psi in OBEs, there is no evidence of anything leaving the body, and there is no evidence of effects caused by out-of-body persons.

The next question I asked was "Why does the OBE seem so real?" To someone who has not experienced an OBE this might seem a silly starting point, but those of you who have will probably understand why I asked it. That then set me to ask, "Why does anything seem real?" Here I provided myself an answer that seemed to account for the OBE (Blackmore 1984).

Very briefly, I argued that the cognitive system cannot make its decision about what is "real" or "out there" at the low level of chunks of input. Rather, it makes its decisions at the higher level of global models of the world. That is, it constructs models of the world, and chooses one. and only one, as representing "the world out there."

I next had to ask, "Can this decision go wrong?" And the answer is obviously "Yes." When there is inadequate input — damage to the system, drugs, trauma, or any of the many things that can precipitate OBEs — then it might. But what would happen if it goes wrong, the system loses contact with reality? I would say that a sensible strategy would be to try to replace the lost input model with the next best approximation — one built from memory. And we know a lot about memory models. For example, as Ronald Siegel (1977) has pointed out, they are often built on a bird's-eye view. We know they are schematized, simplified, and often plain wrong. Indeed, they are just like the OBE world.

I proposed that the OBE comes about very simply when the system loses input control and replaces its normal "model of reality" with one constructed from memory. It seems real because it is the best model the system has at the time, and it is therefore chosen to represent "out there."

This answered a lot of questions about the OBE; especially about the phenomenology of the experience. It also led to some predictions I have successfully tested. For example, if the OBE occurs when the normal model of reality is replaced by a bird's-eye view constructed from memory, then the people who have OBEs should be better able to use such views in memory and in imagery. In several experiments I found that OBEers were better at switching viewpoints, were especially good at imagining scenes from a position

above their heads, and were more likely to recall dreams in a bird's-eye perspective. I actually had some positive results at last (Blackmore 1986a)!

This theory also led to a new approach to altered states of consciousness in general. To that persistent question "What is altered in an altered state of consciousness?" I could now answer that a person's "model of reality" is altered. I could look at changes induced by meditation, drugs, hypnosis, or a mystical experience, in terms of the changing models of reality (Blackmore 1986b). The OBE could then be seen as only one of a variety of experiences that become possible when the input-driven model of reality is lost.

Interestingly, this theory treats the OBE as a kind of error of reality modeling. And so once again the error can be used to throw light on the normal process at work. But I was only able to come back to this insight once I had abandoned looking for psi. It wasn't that I had rejected the possibility of psi, I had simply ignored it.

I mention my OBE research only to contrast it with my previous work based on psi. In my early work, starting from the psi hypothesis, I was forced to ask, "Does psi exist?" In this research I never had to ask it. The other difference is that I no longer had to worry about having an open mind. That makes me wonder what it is like in other sciences. Of course it is always important to have a potentially open mind. If one's results show that one's hypothesis is wrong, then one has to be prepared to change it; but that need not happen very often — at least if one's hypotheses are any good it shouldn't. One doesn't have to have a permanent open mind. And so it was with the OBE research — and what a relief!

I can conclude that all my negative results did teach me something. Or am I perhaps only trying to get my 50-cents worth? A few years ago I read an article in the British Psychological Society Bulletin about the "Royal Nonesuch of Parapsychology." The author, H. B. Gibson (1979), described Mark Twain's wonderful story of cognitive dissonance, about the show that never was. Many people were lured into paying 50 cents to see a nonexistent show, but instead of decrying the fraud they went out and persuaded others to see it and pay their 50 cents too. Gibson was reminded of this tale, he said, by a conference paper given by a woman who had spent two years in fruitless research on parapsychology. He suggested that parapsychology is only kept going by the "very human tendency to try to get one's 50-cents worth after one has been misled . . . by an unkind fate which has led one into an immense expense of effort in a blind alley."

I fought back in print (Blackmore 1979), arguing that I was not just trying to get my 50-cents worth, that I was after the truth and an understanding of the Nature of Life, the Universe, and Everything. But the problem is that it is very hard to understand the nature of life, the universe, and everything, if you start with the psi hypothesis.

In the end I think my negative results told me that the psi hypothesis leads only to unrepeatability (Blackmore 1985). It forces us to ask ever more boring questions, culminating in the question "Does psi exist?" and to that question there is no obviously right answer. Where there is no right answer, we are in ignorance; and, where we are in ignorance, we should do only one thing — have an open mind. But that is too difficult. After all these years of research, I can only conclude that I don't know which is more elusive — psi or an open mind.