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## STEFAN SCHMIDT

### Comments on Sheldrake's 'The Sense of Being Stared At'

In the first part of his contribution Rupert Sheldrake gives an overview on the empirical situation regarding 'the sense of being stared at' that results in an optimistic and positive statement about the phenomenon under consideration. Although I am open-minded (but not convinced) regarding the phenomenon, I shall set out to demonstrate that a lot of the empirical material brought forward by Sheldrake is not that conclusive. I will do so with the intention of eliciting more conclusive research in order to better understand this fascinating phenomenon. But before I do so I will make a few comments on the context in which Sheldrake's activities are taking place.

Rupert Sheldrake has his own style of research which differs from how scientific investigations are usually run. One of the main differences is that its science mostly addresses rather the lay public than other scientists. He has written many popular books about unsolved problems in science, he often gives talks to the interested lay public and he is presenting a lot of his research in the media. But it is not only about presenting; Sheldrake also collects his data in a more public way than it is custom for normal science. His experiments often take place in schools, in a TV setting, or he suggests conducting experiments within science museums. In his books and on his website he encourages people to conduct their own experiments. Sheldrake provides them with the adequate material,

randomization lists, online experiments, instructions, data sheet and the like. He suggested recently (Sheldrake, 2004) distributing one per cent of the money spent in science according to the (democratic) wishes of the general public.

Sheldrake continually tries to bridge the large gap between what is taking place in the laboratories and its public perception. His intentions are not only to explain the complicated to the lay people but also to bring the interests of the public back to science. Sheldrake is advocating research into unexplained phenomena that are widely neglected within mainstream science. His democratic approach targets the problem that scientists usually research what normal people do not understand or are not interested in, but avoid investigating those phenomena which puzzle people in their daily lives. The 'sense of being stared at' is only one example, and it could equally well have been precognitive dreaming, telepathic experiences or synchronistic events. By inviting lay people to perform the experiments which professionals refuse to conduct, he brilliantly exposes a wide spread dogma within science. This dogma defines which topics are rightly viewed as belonging to science and which ones are to be regarded as superstitious beliefs and thus are not worth considering. The important point to be made here is that this dogma is itself an unproven belief. Science is defined not by its topic but by its methodological (i.e. scientific) approach. This is a fact which many scientists still refuse to acknowledge.

So is the solution to the widespread neglect of investigations into the paranormal that these are researched by school children? The answer is yes and no! Yes, because this public pressure does not leave science unimpressed. It advocates a change which is needed urgently. No, because most of the data conducted within these public experiments cannot replace good controlled research in the laboratory. If the aim is to nail down 'the sense of being stared at' as a fact then these data will never be sufficient. Such field studies and public experiments are just one part of several approaches that will be needed. Only a multiplicity of approaches all targeting the same phenomenon will succeed in the long run. Sheldrake constantly errs when he argues that his own material is a conclusive proof for the existence of 'the sense of being stared at'. Of course, as an advocate of a neglected public opinion he has to do so. But this is more a political statement than one based on evidence. In my view Sheldrake no longer has any choice but to discuss the data from a more or less neutral point of view. Therefore he is missing parts of the story.

Experiments taking place on a large scale in public, like the staring experiments published by Sheldrake, lack the important controls which allow the conclusion that any effect found in the data is due to the variable under consideration and is not an artifact of any other influences or circumstances. Most of these problems are already mentioned in his paper: sensory cueing, scoring errors, cheating and the like. I would agree with Sheldrake that none of these artifacts are solely responsible for his average hit rate of 54.7% (Mean chance expectation 50%). But can't it result from a mixture of them? Some kids are cheating because they want to present positive results to the investigator, others get confused with the notation (if you don't stare and the other person says 'not stared' you have to

denote a hit), and others again are helped by some subtle cues nobody (including themselves) is aware of. All these things happen every now and then and everybody who has ever conducted such experiments will be aware of the fact that these easy set-ups can get very tricky after a thorough look at all possibilities for artifacts. The introduction of precautions such as blindfold, earplugs and the elimination of direct feedback (in combination with sensory cues a possibility for learning effects) has reduced the effect from 56.8% and 54.9% to 53% in the following study. So parts of the effect might have been due to these artifacts. However, in direct comparisons in a later study (Sheldrake, 2001) no direct differences could be found for trials with and without blindfolds and with and without feedback. But to complete the picture studies with still tighter controls in a laboratory have to be performed. Public studies have the advantage of a high ecological validity; laboratory studies have the advantage of high controls (internal validity). Both approaches will contrast and complement each other in order to give a complete picture. In a laboratory sensory shielding can be handled in a far better way than in class rooms. Videotaping of the experiments allow for scoring by independent (maybe even blinded) raters to avoid cheating and scoring errors. So why not invite schoolgirls and boys to the laboratory? I even suggest inviting only those kids who had positive scores in order to find out whether they can repeat their success.

At this point, where Sheldrake can already report more than 30,000 trials conducted in the field, we need more input at the better controlled end of the scale, i.e. rigorously conducted high quality laboratory studies.

But unfortunately Sheldrake is following the opposite direction. He promotes a completely uncontrolled online test where everybody who is looking for fun can enter via the internet some invented data without even performing the experiment. I gave this test a try with a colleague in my office. Twice I got confused with the 'correct', 'not correct', 'stare' and 'do not stare' commands and I entered a false positive result. The software (which does not run with every browser) allowed me to go back and to enter a second result for the trials. After twenty trials I had twenty-two results! Then after pressing the send data button I could see that only my wrong results were transmitted. Thus the data reported in this journal already contain my erroneous (and thus positive!) data although I did not set out with the intention to cheat. Such an online test has no scientific value at all! The opposite is true: everybody who has seen this low quality test will take a more sceptical view of other data produced by the author. There is definitely no need for more uncontrolled data but for more controlled trials! Only these are able to rule out the artifact hypotheses.

Finally I would like to comment on the research patterns in these staring trials. Sheldrake often presents his results separated for looking trials and non looking trials. In most of the cases hit rates in the looking trials are higher than in non-looking trials. At several places (e.g. Sheldrake, 1999; 2003) he concludes from this pattern that staring is better detected than non-staring and that this might be due to an evolutionary reason which in turn is a proof of the effect. But this cannot be concluded from this data as splitting the data between looking and

non-looking trials without taking the response pattern of the participants into account leads to wrong conclusions. There are many simple statistical ways to take this response pattern into account (see e.g. Burdick & Kelly, 1977; Radin, 2004). Figures like nos 1–4 in Sheldrake's paper give the impression that people perform differently in looking and not looking trials. But probably they don't. Such figures should always be contrasted with figures where the hit rates for the two possible answers 'Yes, I was stared at' and 'No, I was not stared at' are presented. Only this additional pattern will complete the picture.

In all of these experiments people report more often that they were stared at than that they were not stared at. This might be due to some internal bias which has nothing to do with a staring experience. Maybe people just tend to say 'yes' rather than 'no'. This response bias would then distribute evenly on the stare and non-stare trials. But it might also be that people report more positive answer because they sense the gaze from behind. It is not true that these two hypotheses cannot be differentiated statistically (as claimed in Sheldrake's paper). The relevant test is fairly easy. If the participants really sense the gaze better than the non-gazing then they should get a higher hit rate for the trials where they report 'Yes I was stared at' vs. trials where they report 'not stared at'. If the response bias is evenly distributed the two hit rates should be the same. Table 5 in Sheldrake (1999) gives a grand total of 13,903 trials and allows for such a calculation. If people report 'stared at' they are right in 55.1% of the cases, if they report 'not stared at' this is correct in 54.8 % of the trials. These data favour the hypotheses that the response bias is not related to the any staring experience. Radin (2004) found the same result with a different method, and Sheldrake has long been aware of this fact, but he does not report this important additional information. I would like to suggest a simple empirical test that could resolve this matter. Usually participants complete twenty trials in a row with a more or less even number of stare or non-stare trials. If the participants don't get feedback another two sets of twenty trials each can be performed where the distribution of looking and non-looking trials is highly skewed. E.g. 18 looking trials out of twenty in the next session and then only 2 in the third one. The sequence of these three sessions has to be random with different participants. If the response pattern (number of yes/no answers) is dependent on the distant gaze then it should shift according to the frequency of the gazes. But if the bias is not related to distant staring it will be same in all three sessions.

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