Open Peer Commentary
on ‘The Sense of Being Stared At’ Parts 1 & 2

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Staring at the Back of Someone’s Head Is No Signal,
And a Sense Of Being Stared At Is No Sense

I: Introduction

The first of Sheldrake’s twin articles in this edition of the *Journal of Consciousness Studies* presents the case for the existence of a ‘sense of being stared at’, which is purported to be a capacity to discriminate at above chance levels between being stared at and not being stared at from behind, by an observer who is, on standard accounts of vision, unseen. That case is based on a summary and amalgamation of the results of dozens of experiments, most of them so-called direct-looking experiments (where the observer is present in the room with the subject), along with some experiments that use more indirect methods (where the observer and subject are in separate rooms connected by closed circuit television or one-way mirrors); anecdotal accounts of a sense of being stared at are proffered in a supporting role. In the second of Sheldrake’s articles, he presents a case for a radical re-conceptualization of vision, one that, he claims, will allow us to explain how people are apparently sometimes able to tell whether some otherwise unseen person is staring at the back of their heads.1 In this commentary, I argue that Sheldrake presents and analyses the data in the wrong way, and that labelling such a capacity a sense is a misnomer. I also suggest that there really is no such capacity, but that, to the extent that there is anything substantive and meaningful in the data at all, it indicates a capacity that is rather more cognitive than sensory-perceptual, namely a belief or reasoning bias.

II: Mistaking Signal For Noise

Sheldrake is convinced by the data summarised in his article that at least some people are able, at least some of the time, to tell that someone else is looking at them, even though they are unable to see that person (in the usual sense). Notice that there are two issues here. First, there is the question of whether there is, and ever can be, a signal in the first place; that is, whether the fact that someone is staring at your back can be signalled by that very behaviour alone, without any

[1] From what I could fathom from this re-conceptualization of vision, there is also a hint of a putative account not only of the sense of being stared at, but also of why it is only stares that can be discriminated by this ‘eyes-in-the-back-of-the-head’ capacity. Certainly an account of the former ought to explain the latter issue.

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additional behavioural or verbal cues. Second, there is the question of whether people are able to detect such a signal, and if so, how sensitive their ability is to do so. The problem with the first issue is that we have no means by which we can discover the existence of such a signal that is independent of subjective report, of a feeling of being stared at. The prospects of finding such an independent means of measurement are highly remote, despite Sheldrake’s brave — but, I have to admit, rather vague and ultimately misguided — attempt to specify, in Part 2 of his piece, a means by which such a signal might manifest itself and a means by which people might be able to detect such a signal. The idea of a ‘staring at you’ signal, and Sheldrake’s specific proposal, are far outside the realms of current scientific knowledge, pace Sheldrake’s attempt to persuade us otherwise.

The problem I see with respect to the second issue, in addition to what I have already implied, is Sheldrake’s presentation of it. I have two points to make, the second relatively minor compared to the first (though nonetheless important). First, consider what Sheldrake says of the participants’ task in the typical experimental setup used to test for the presence of a sense of being stared at. In addition to being asked to detect the presence of a stare, Sheldrake claims that: ‘The subjects are being asked to detect the absence of a stare, an unnatural request with no parallel in real-life conditions’ (p. 18). No they are not. The subjects are only being asked to detect the presence of a stare. The task is a simple yes–no task (a well-known variant of the two-alternative forced choice); the participants have to answer either ‘yes’, the person is staring at me (stare present), or ‘no’, the person is not staring at me (stare absent). The stare (a signal) is either present or it is not, and in either case there is ‘noise’, which comprises all other stimuli in the environment transduced by the relevant sense organ (though just what this would amount to in Sheldrake’s conception of a sense of being stared at is unclear; see Section III, below), along with the variability in the responses of that sense organ and its associated perceptual mechanisms in the brain. Successful detection of the signal depends on its strength (against the strength of the noise) and the sensitivity of the relevant sensory transducers and relevant neural machinery. Subjects can be either right or wrong about the presence of the signal. They can say the signal is there when it is (a hit or true positive), that it is not there when it is indeed not there (a correct rejection or true negative), that it is there when it is not (a false alarm or false positive), and that it is not there when it is (a miss or false negative). This is the basis of signal detection theory (e.g., Green & Swets, 1966; Stanislaw & Todorov, 1999).

According to signal detection theory, the performance of subjects in yes–no tasks can be fully described by the hit rate (the proportion of hits) and the false alarm rate (the proportion of false alarms). The hit and false alarm rates reflect two factors: the response bias or decision criterion of the participants (how likely they are to respond yes or no to signals of given intensities) and their sensitivity to the signal. Percentage correct scores, which Sheldrake reports and upon which he bases his claims about the existence of a sense of being stared at, reflect sensitivity reliably only in the absence of any response bias. In yes–no tasks, response bias is more likely than in standard two-alternative forced-choice tasks, so
reliance on percentage correct scores as measures of sensitivity is problematic. The most widely accepted measure of the sensitivity of subjects is the value of d', which, if certain assumptions are met, is independent of response bias. The formula for calculating d' need not concern us here; it suffices to note that d' expresses a relationship between the hit and false alarm rate, and that a d' value of 0 indicates an inability to distinguish signal from noise, whereas perfect performance is indicated by a d' value of +∞.

Thus the crucial comparisons for the being-stared-at-from-behind experiments discussed by Sheldrake are not the proportion of right versus wrong answers in each of the looking and not looking trials, as he makes out, but rather, the proportion of hits versus false alarms. If we take the values in Sheldrake’s Figure 1A (p. 11 above; exact values obtained from Sheldrake, 1999, Table 5), we see that the overall hit rate across these many experiments is 0.59 (i.e., 59%) and the overall false alarm rate is 0.493 (i.e., 49.3%). Plugging these values into the Applet for calculating d' from the hit and false alarm rates provided at http://wise.cgu.edu/sdt/sdt.html yields a d' value of 0.25. On a scale of 0 to +∞, 0.25 is a very small d' value indeed. So how does that compare to typical d' values in other kinds of yes–no perceptual tasks? Azzopardi and Cowey (1997) reported that the sensitivity of a blindsight patient, GY, in detecting the presence of static, vertical, black-and-white, square-wave contrast gratings presented in his blind field ranged from 0.833 for the lowest contrast to 1.354 for the highest contrast. Blake et al. (2003) reported that a group of children with autism were impaired relative to a group of typically developing children in being able to discriminate biological motion in point-light displays from phase-scrambled versions of the same displays (i.e., to say whether a person was present in the display or not). The mean d' value for the autistic children was around 1.2, whereas it was around 2.5 for the typically developing children. Grimshaw et al. (2004) had participants detect the presence of a target emotion (happy, sad, or angry, depending on the experimental block) amongst a series of facial expressions (happy, sad, angry, or fearful) presented for only 50ms, by requiring them to press a button to indicate ‘yes’ (target present) or ‘no’ (target absent). The mean d' values were 2.5 for sad, 2.6 for angry, and 3.45 for happy targets. In sum, if there really is a ‘staring at you’ signal, then a d’ value of around 0.25 indicates that we are not very sensitive to that signal at all.

Furthermore, consider that a d' value as low as 0.25 indicates that subjects with a liberal response criterion (i.e., with a response bias towards saying ‘yes’) produced a high proportion of hits but also an almost equally high proportion of false alarms, whereas subjects with a conservative response criterion (i.e., a response bias towards saying ‘no’) produced a low proportion of hits and a low

[2] This is not the place to discuss what those assumptions are. For the sake of argument, I am assuming that these assumptions are upheld in the present case. There are other, non-parametric measures of sensitivity, which can be used if these assumptions relating to the use of d', and thus to the independence of this measure of sensitivity from response bias, do not hold. See Stanislaw & Todorov (1999) for discussion.
(again, nearing equal) proportion of false alarms. With rather higher d' values, as the response criterion decreases (i.e., gets more liberal), the false alarm rate does not increase as rapidly as the hit rate. (A nice graphical illustration of this is provided at http://wise.cgu.edu/sdt/sdt.html.) Presenting the data as Sheldrake does (i.e., as mean percentages correct and incorrect in the looking and not looking trials) obscures possibly important individual differences between subjects in their response criteria. Indeed, if, as I will suggest in Section IV, the findings in the being-stared-at-from-behind experiments reflect a belief or reasoning bias operating in some (but not all) subjects, rather than a sensory-perceptual capacity to detect whether someone is staring at the back of one’s head or not, then high proportions of both hits and false alarms is exactly what we would expect to find.

The second point I wish to raise regarding Sheldrake’s presentation of the apparent ability of people to detect when they are being stared at from behind is this. Sheldrake emphasises the ‘astronomically significant’ (p. 14) statistical results obtained when the results of many direct looking experiments are combined, resulting in analyses over ‘tens of thousands of trials’ (p. 14). If we set aside worries about conducting statistical tests on the combined results of studies that likely differed, even subtly, in various ways (more on which below), such significance values are indeed very impressive. Lest we forget, however, significance values tell us only about the reliability of any effect (the probability that the results are due to chance, rather than to the effect or variable in question), not about the strength or size of such an effect. Now Sheldrake does not conflate these two issues. Nonetheless, it is important that the distinction is emphasised, for a statistically naïve reader might take Sheldrake’s point to imply that consistent findings of around 55% correct guesses in direct looking experiments (5% above chance) is a large effect, when it is in fact very small. Just how small the reported effects are, we do not know. For measures of effect size are reasonably straightforward to calculate, and are included as output options in most statistics computer packages, yet Sheldrake does not report the effect sizes of any of the experiments he reports (admittedly, nor do the majority of published studies in psychology).

In the previous paragraph I mentioned that there are legitimate concerns about conducting statistical tests on the combined results of different studies, even if those studies have essentially identical experimental setups. One concern is that the subjects’ expectations or motivation may vary across studies, depending on varying demand characteristics, as the result of even small differences in experimental procedures, and experimenter expectancy effects. This is a particular problem in the case of forced-choice yes–no tasks, for as signal detection theory teaches us, a subject’s response criterion, and thus the proportions of hits and false alarms, can vary even when the signal intensity and the subject’s sensitivity to that signal remain constant. Clear demonstrations of such changes in subjects’ response criteria are provided by studies in which the payoffs for the four possible responses are manipulated (e.g., varying the size of rewards for hits and correct rejections, and the size of penalties for false alarms and misses).
Nevertheless, subjects’ response criteria can also vary as a function of more subtle factors, such as differences in demand characteristics and experimenter expectancy effects.

III: In What Sense Is the Sense of Being Stared At a Sense?

If there is a sense of being stared at, then it is rather different from any of our other senses. Each of our six main senses (vision, hearing, smell, touch, taste, proprioception) conveys a multitude of messages; indeed, in some cases, the number and variety of the informational contents are essentially boundless. The more complex the sensory organs and the neural systems devoted to processing information transduced by those organs, the more and richer the messages that are conveyed. A sense of being stared at, in contrast, is held to convey only one possible message: ‘someone is staring at me’. That would be a very primitive sense indeed. Granted, there are organisms out there that have primitive sense organs of sorts, sense organs that convey only one message. Certain single-cell organisms, some jellyfish, starfish and leeches, for example, can sense the presence of light somewhere nearby (thanks to their light-sensitive body surfaces, rather than to their possession of eyes, as such), but cannot distinguish amongst different sources of light or even what direction it is coming from, and certainly cannot use that light to form an image of any sort (example borrowed from Dawkins, 1996). Is Sheldrake proposing that we have some equally primitive sense organ and thus sensory capacity? I doubt it. As becomes clear in the second of Sheldrake’s articles, he wants to include the sense of being stared at not as a separate sense, in the way vision or hearing are senses (or even in the way that a primitive capacity to detect the presence of light might be regarded as a sense), but as an aspect of vision — albeit only after a radical re-conceptualisation of that sense.

IV: The Sense of Being Stared At Is Likely to be a Belief or Reasoning Bias

A telling allusion to what I think is really going on in the being-stared-at-from-behind experiments is made by Sheldrake on p. 25, where he mentions Jonathan Jones’s unpublished finding of higher levels of arousal in people with high scores on the Schizotypal Personality Questionnaire when they were being stared at from behind, compared to when they were not being stared at, whereas low scorers did not differ in their arousal levels. This is indeed an interesting finding, and, I have to concede, might be considered to count against the sceptical position I am advancing. That said, to my knowledge this finding has not been replicated in a peer-reviewed published study, and so must be taken with a pinch of salt. However, the main point I wish to draw from this paragraph of Sheldrake’s article is not related to this particular finding, but rather, to the suggestion that people who tend towards a schizotypal personality type differ from people who are less schizotypic in respect of their responses in the being-stared-at-from-behind experiments. Rather than being more sensitive to
having the back of their heads stared at, I suggest that people with schizotypal tendencies manifest an amplified response or reasoning bias.

Reasoning biases are common in schizophrenia, especially in people with persecutory delusions. Schizophrenics with persecutory delusions are more likely to jump to conclusions on the basis of insufficient information than non-deluded controls (e.g. Huq et al., 1988; Dudley et al., 1997a,b), and have a greater tendency to attribute negative events to external personal causes (e.g. Kaney & Bentall, 1989; Lyon et al., 1994). People with persecutory delusions also tend to have biased attention and memory for material associated with personal threat, and appear to experience a heightened state of vigilance for threat in inappropriate or ambiguous contexts, believing there to be threat without supporting evidence (e.g. Bentall et al., 1995; Kaney et al., 1992; 1997). (For reviews, see Blackwood et al., 2001; Garety & Freeman, 1999.) All this is consistent with the possibility that schizophrenics with persecutory delusions will tend to show a bias towards answering ‘yes’ in the being-stared-at-from-behind experiments discussed by Sheldrake. To the extent that persecutory delusions and other schizotypy tendencies in the normal population are like attenuated forms of those symptoms in clinically psychotic individuals, we might also expect that a response bias for answering ‘yes’ in being-stared-at-from-behind experiments will positively correlate with the degree of delusional ideation and schizotypy personality style in non-clinical groups. Interestingly, Brébion et al. (2000) found that schizophrenics with higher hallucination scores had a tendency to respond with a high rate of false alarms in a recognition memory test for items presented either verbally or visually, and that both hallucinators and delusional patients were more likely to mistake imagined (verbally presented) items as pictures they had actually perceived, suggesting that such people may show similar response biases in the being-stared-at-from-behind experiments. I recommend that these predictions be tested in future research on the (misnamed) sense of being stared at.

References


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Nomenclature and Methodology

In his first paper Sheldrake attempts to uncover whether or not the ‘sense of being stared at’ is real or illusory, concluding that ‘the great majority of the evidence supports the reality of this sense’ (p. 29). I would like to discuss some of the issues that he raises and how they pertain to his central argument and the body of evidence for and against remote staring detection. I will be focussing most of my comments on part one of his paper.

I have divided my comments into three main areas. First, I will discuss the nomenclature that Sheldrake uses in the paper and which is used in the field as a whole to describe this phenomenon and why a consensus is required. Secondly, I will illustrate how there are issues between the different methodologies used in this area concerning two subtly different senses of the concept of ecological validity, namely: realism and generalisability. Finally, I will show how the CCTV-based method offers a superior methodology for the investigation of remote staring detection, and that it represents a considerably different methodology to the other methods listed. This difference, both in terms of the greater robustness and validity when compared to the other approaches, means that the results from the CCTV method have to be considered separately to the other results. This has consequences for the main thrust of the argument that Sheldrake puts forward in this paper.

I would like to thank Paul Stevens and Claudia Coelho for their helpful comments on an earlier version of this commentary.
I: Definition

As Sheldrake notes, the ‘sense of being stared at’ has been investigated for over 100 years, with the majority of the research being conducted from the early 1990s onwards. I would initially like to raise an important point about the terminology in use in this area. Sheldrake uses the term ‘the sense of being stared at’ to define what Braud et al. (1993a,b) describe anecdotally as ‘[having the] feeling that someone was staring at you from behind and, upon turning around, [finding out that] you were correct’ (1993a, p. 373) and operationally as ‘the purported ability to detect when one is being watched or stared at by someone situated beyond the range of the conventional senses’ (1993b: p. 391).

However, the term that Sheldrake uses is potentially misleading, as it does not define whether or not the stare is conventional in origin, or beyond the range of the conventional senses. An individual could be uncomfortable due to the sensation provided from someone staring at them directly from the front. I advocate the term ‘remote staring detection’, not only because remote staring has been used more often in the research than any other term, but also because it defines the three core elements of this phenomenon; first, due to the way that the experiments are designed they are testing if the individual is detecting the stare of another. Secondly, the term ‘remote’ makes the clear distinction that we are talking about a stare beyond the range of the conventional senses. Finally, the individual is generally ‘staring’, a term that is sometimes removed in other nomenclature defining this phenomenon (e.g., ‘unseen gaze’, ‘covert observation’, etc.), although it is of particular importance when defining the nature of the phenomenon. There has been a debate in the social psychology literature on interpersonal interaction about the use of particular terms to describe certain visual interactions, with research demonstrating that the term ‘staring’ is consistently placed at the most extreme end of a scale in terms of the length of time of an eye-fixation (Kirkland and Lewis, 1976; Baker, 2001). Ellsworth et al. (1972), made an important definition of staring for their study examining the social impact of staring when they defined it as ‘a gaze or look that persists regardless of the behaviour of the other person’ (p. 303). Most eye-based dyadic interactions employ a complex relationship of ‘mutual gaze’ (Argyle and Cook, 1976; Argyle, 1988) between the two individuals, which provides several different types of communication (Kleinke, 1986). However, staring represents an anomaly to this because it remains fixed regardless of the other person’s behaviour. Therefore ‘staring’ is an important term with which to frame remote staring detection, as the stare from the remote individual continues regardless of the behaviour of the individual being stared at. Although there might be some form of interaction between the two individuals, from a purely descriptive position, the term ‘remote staring detection’ appears to be more appropriate than any other term used to describe this particular phenomenon.
II: Issues of Ecological Validity

One of the main issues surrounding the research into remote staring detection is ecological validity. This issue is complex, and weaves its way throughout many of the different areas of the literature. For example, is the use of direct looking methods more ecologically valid than the separation of the individuals involved by a CCTV link? Is the use of conscious guessing more ecologically valid than the use of unconscious physiological measures? These questions have implications for the controls used in these experiments and how they might restrict the investigation of the real life phenomenon.

Part of the problem is that one can identify two subtly different meanings of the concept of ecological validity that need to be teased apart in any discussion of the validity of different methods used to investigate remote staring detection. First, there is the issue of generalisability—can the data obtained from a particular method be generalised to the real life phenomenon that the experiment is attempting to measure? In relation to this issue the discussion will focus specifically on the extent to which the laboratory-based measurements of electrodermal activity can be assumed to be present in all instances of remote staring detection. Secondly, there is the issue of realism, which refers to how closely a particular method recreates the phenomenon as one assumes it happens in ‘real life’. In relation to this issue, the discussion will focus on whether or not the direct looking experiments are a closer representation of the real-life phenomenon of remote staring detection than the CCTV laboratory-based experiments. I will now examine both of these issues in detail.

Realism

Throughout his paper, Sheldrake refers to different types of research as evidence for or against remote staring detection, highlighting two particular methodologies: the ‘direct looking’ and the ‘CCTV-based’ experiments. These different types of methodology can be broadly placed along the continuum that I have outlined in Figure 1. I have also included two additional approaches that he does not clearly place into this classification; namely his own ‘Window’ experiments, where the starer and staree are separated by a window (Sheldrake, 2000), and the ‘One-way Mirror’ experiments, where the starer and staree are separated by a one-way mirror.

Figure 1 demonstrates the methodological development and increasing sophistication of the remote staring detection studies. This development has been gradual over the past 100 years, although some researchers, Sheldrake included, have advocated a return to simpler measures. For example, Sheldrake has argued that

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[4] The ‘staree’ is the individual who is being stared at. The ‘starer’ is the individual who is doing the staring.

[5] The terms ‘one-way’ and ‘two-way’ mirror are interchangeable and refer to a sheet of metal-coated glass which reflects approximately half of the light and allows the rest to pass through it. When placed in a wall between rooms where one room is dark and the other is well lit, it is possible to see clearly through the mirror into the lighter room from the darker one, but it appears to be a normal mirror from the lighter room.
‘direct-looking tests are far easier to perform than CCTV trials’ (p. 14) and that, ‘a great advantage of simple experiments in which subjects make conscious guesses is that they enable many more people to take part in this research than the CCTV method. They are also closer to the real life phenomenon’ (Sheldrake, 2001, p. 122). He has argued this because typically, with many experiments, as the degree of control over extraneous variables decreases, the degree of the realism element of ecological validity increases (as noted in Figure 1). However, this might not necessarily be the case in the remote staring detection literature.

There has been a dramatic rise in CCTV-systems for everyday surveillance by businesses, and local and national government, particularly in the UK, over the past 10 years. This means that people are observed via CCTV on a daily basis, (as Sheldrake himself notes, p. 22). In fact, it has been estimated, based on surveys on the proliferation of CCTV systems in London, that as of 2003 there were as many as 4.2 million CCTV cameras in the UK, which translates as one camera for every 14 people (McCahill and Norris, 2003, as cited by Norris, McCahill and Wood, 2004). The experiments that used CCTV methodologies recreate this, and therefore are equally ecologically valid to the other methodologies. They are recreating an everyday experience from real life, although it might be different from the type of experience recreated by the direct looking experiments. As Sheldrake notes in his paper (p. 22), and in his previous work (Sheldrake, 2003), his interviews of personnel in the surveillance industry suggest that people do detect being watched via CCTV in real life.

**Generalisability**

An aspect of methodology where Sheldrake does not draw enough of a distinction in his paper is the difference between conscious and unconscious remote staring detection. Within the literature, conscious measures commonly involve the staree verbally indicating or writing down whether or not they think they are being stared at during a particular epoch. In contrast, the unconscious measures...
involve the measurement of the staree’s electrodermal activity (EDA) during staring and no-staring epochs.

It is debateable whether or not conscious measures are more ecologically valid than the unconscious measures, as the physiological stimulation provided by a remote stare would most likely act as a precursor to cognitive awareness. Braud et al. (1993a) decided to conduct the first study using physiological measures because of this, stating that,

[remote] staring detection frequently takes the form of spontaneous behavioural and bodily changes. Often, such changes are reported to be rich in physiological content (for example, tingling of the skin, prickling of the neck hairs) and automatic movements (for example, spontaneous head turning, unplanned glances). Higher cognitive functions seem to play minor roles in these staring detection contexts. (Braud et al., 1993a, p. 376–7)

In fact, they also suggest, when discussing previous research using direct looking methodologies employing conscious guessing, that ‘such a procedure would be expected to maximise possible cognitive interferences and distortions of subtle internal staring-related cues’ (Braud et al., 1993a, p. 376). Therefore, measuring physiological arousal could be more ecologically valid than the behavioural measures, as the information from processing the unconscious, physiological stimulus of the remote stare is not reaching conscious awareness. By measuring the ‘pure’ unconscious physiological reaction we are avoiding the ‘contaminated’ cognitive measure. This type of processing of stimuli without conscious awareness has been noted in other areas, such as: change blindness (see O’Regan, 2003, for review) and perception without awareness (see Pessoa, 2005, for review).

III: Can the EDA-CCTV and Direct Looking Methods Be Directly Compared?

The use of EDA measures, with the controls that are implicit in their use, are normally combined with the CCTV method, mainly because of the controls both methods provide, and because they require considerable resources that combine well in the laboratory. These experiments involve separating the starer and staree into different rooms and measuring the EDA of the staree during randomly-scheduled epochs when the starer stares or does not stare at them via the CCTV system. This combination of EDA and CCTV (or EDA-CCTV) provides an even more robust methodology, and all of the 15 experiments (from nine studies) that Schmidt et al. (2004) included in their meta-analysis of EDA-based remote staring detection studies combined the CCTV method with the physiological measure. Schmidt et al. (2004) scrutinised the studies for a variety of issues concerning the veracity of the method, such as: safeguards, the quality of the specific methodology for electrodermal measurement, and overall methodological quality. They found a significant, but small, effect (Cohen’s $d = .13, p = .01$) across all of the studies.
The size of this effect is also an important factor when comparing the CCTV method with the other methods, particularly the ‘direct looking’ experiments. Sheldrake claims in his paper that the direct looking experiments have an overall significance value of \( p < 1 \times 10^{-20} \) (p. 15). However, as was pointed out above, Schmidt et al.’s (2004) meta-analysis found that the EDA-CCTV remote staring detection studies had a far smaller overall significance value of \( p = .01 \). These significance levels clearly do not match, and the difference between them is readily apparent: the larger the significance value, the less robust the controls. Even if a less conservative estimate is used, by examining the meta-analysis of the EDA-based remote staring detection studies by Schlitz and Braud (1997), there is still an enormous discrepancy between the significance of the effect that Sheldrake notes and the significance size that Schlitz and Braud (1997) notes (\( r = .25, p = .00005 \)). There is obviously a significant effect in the remote staring detection studies, as the well-controlled EDA-CCTV studies demonstrate, but there is a strong suggestion that at least part of the disproportionately high significance level of the direct looking experiments could be due to a lack of adequate controls.

The CCTV method has become increasingly divorced from the others in the continuum. Researchers employing the other methodologies in the continuum have gone to great efforts in their attempts to reduce extraneous variables and sources of sensory leakage, but CCTV is the only method that can categorically claim to have achieved this. It does not fall foul of the possible artefacts that Sheldrake describes (p. 21), and he in fact relies upon the results from studies that employed the CCTV method in order to bolster his argument. As soon as these methods are separated and the CCTV method is no longer used to provide support, Sheldrake’s arguments against possible artefacts explaining the remote staring effect are forced to rely solely upon less secure methods and unverifiable anecdotal reports. For example, when he argues against ‘subtle sensory cues’, he states that, ‘in addition, positive results in experiments using one-way mirrors and CCTV seem to eliminate the possibility of sensory cues.’ When arguing against cheating, he again appeals to methods ‘separating lookers and subjects by … one-way mirrors or by closed circuit television’ (p. 21) providing positive results to show that this remains an impossible criticism for all of the studies, without any regard for the methodology used. Finally, when arguing against hand scoring errors, he again relies on the CCTV method to bolster his argument as he states ‘also, there was no hand scoring in the CCTV trials’ (p. 21).

The use of the combined method of CCTV and electrodermal activity measurement demonstrates such a high degree of methodological and conceptual difference when compared to other studies in the continuum, that they might represent remote staring detection under the best controlled circumstances, or they might represent a subtly different phenomenon altogether. The effect sizes noted under these conditions are similar to the effect sizes noted under other

[7] It would have been very useful if Sheldrake had provided an overall effect size in addition to this significance level, and to have had a detailed rationale and description of the process of calculation that led him to conclude this overall level of significance for the direct looking experiments.
DMILS (Direct Mental Interaction between Living Systems) studies that have employed electrodermal activity as a dependent measure (i.e. $d = .11$, $p = .001$, as reported by Schmidt et al., 2004). It is possible that this represents a similarity between EDA-CCTV and the wider DMILS effects, which might be a related process, but not necessarily the same as the potential remote staring detection effect observed in the direct looking experiments. In the second part of Sheldrake’s paper, he discusses remote staring detection and he speculates how it might be related to extramission theory and ‘perceptual fields’. However, Sheldrake cannot easily incorporate the findings from the CCTV method, stating, ‘the way in which they can help explain the effects of staring through CCTV is obscure’ (p. 44), demonstrating that, from his theoretical standpoint, the CCTV method is incomparable with the other methods. We do not yet fully understand the significant effect obtained using the CCTV method, and how these findings are related to the data from the other remote staring detection experiments. We need to clarify the issue with further experimentation examining more detailed physiological reactions to a remote starer separated via a CCTV link, and analysis comparing the validity of the different methods.

IV: Conclusions

There are two main inconsistencies in Sheldrake’s argument. First, he relies upon the EDA-CCTV studies to strengthen his argument that the evidence obtained from the direct-looking experiments demonstrate that remote staring detection is a real phenomenon. However, there are considerable methodological differences between these two methods that make a direct comparison difficult. The EDA-CCTV studies are well-controlled laboratory experiments that carefully separate the starer and staree and rely upon unconscious measures. In contrast, the direct-looking experiments cannot incorporate as robust controls due to their very design and their reliance upon conscious guessing. Moreover, the EDA-CCTV studies have demonstrated a significant, if small, effect of remote staring detection on their own, and it is unnecessary to incorporate them with the other, less-controlled studies. It is, however, necessary for the two approaches to stand and be evaluated on their own robustness and validity. The combined EDA-CCTV approach has largely stood up to independent, rigorous statistical and methodological scrutiny, thanks mainly to Schmidt et al.’s (2004) meta-analysis; the remote staring detection studies that employed conscious guessing and direct looking have yet to do so.

Secondly, Sheldrake admits in the second part of his paper that the results from the EDA-CCTV studies do not easily fit into the perceptual fields theory that his is advocating to explain remote staring detection. Essentially, in his paper Sheldrake is attempting to have the best of both worlds; he is happy to use the more robust empirical evidence from the EDA-CCTV studies to back up his claims from the direct-looking experiments, but then sidelines the EDA-CCTV

[8] Extramission theory is described as the concept that ‘vision involve[s] emissions from the eye’ (Cottrell et al., 1996: p. 50).
studies from his perceptual fields theory because there is difficulty in incorporat-
ing them conceptually.

In conclusion, I would agree with Sheldrake that there is promising evidence
that remote staring detection is a real phenomenon, although there is much
research required to reveal its nature. However, the evidence comes almost
entirely from the well-controlled EDA-CCTV lab-based studies that need to be
considered separately from the other approaches.

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SUSAN BLACKMORE

Confusion Worse Confounded

Sheldrake’s two papers are so deeply confused that they should never have been published in JCS. Given that they are published I shall discuss just two worrying confusions; one from each paper.

In Part 1, Sheldrake consistently mixes up the sense of being stared at that derives from the normal senses of vision or hearing, with a putative sense that he claims can operate without normal sensory cues. He might have avoided confusion by giving a name to this proposed paranormal sense, and then made clear, throughout the paper, which he was referring to at different times, but he did not. The reader might try to understand the paper by assuming that when Sheldrake talks about ‘the sense of being stared at’ he always means this proposed paranormal sense, but this is not so; he uses the same phrase to refer to the normal sense. For example, in the opening quotation from Conan Doyle the man looks up to meet the eyes of the person staring at him, implying that the starer was probably visible in peripheral vision.

This confusion permeates the paper. Detectives should not stare at someone’s back for the obvious reason that they might not be able to avert their gaze fast enough, or convincingly enough, if the person happens to turn round. Detection with binoculars might seem impossible by normal means, but binocular lenses can have highly reflective surfaces and may look like eyes from a distance. The surveys discussed seem to include some questions that refer only to a paranormal sense and some that might include normal sensing. This means that when Sheldrake discusses the evolutionary function of the sense of being stared at the reader cannot tell whether he means a normal or paranormal sense. Obviously there would be an evolutionary advantage in being able to detect another’s gaze and in fact we, and other species, have visual systems designed to be good at this. A pair of eyes is a salient stimulus. We can pick it out easily from complex scenes, attention is automatically drawn to it, and eye movements are made towards it without prior identification. We can also tell from very small differences whether someone is looking straight at us and focussing on us, or not.

Sheldrake asserts that research has neglected the sense of being stared at because people believe it is impossible. Unless he makes it clear that he is referring to a paranormal sense, this claim is ridiculous. He also refers to a taboo against psychic phenomena. As a former parapsychologist, I do not believe there is any such taboo. The fact is that evidence for paranormal phenomena is weak and usually unreplicable, and there is no plausible theory to explain such evidence as there is. Most scientists choose not to investigate paranormal claims, not because of a taboo, but because they have more promising and exciting things to spend their precious time and research resources on.

There follows a review of experiments some of which were done under conditions that would rule out the use of the normal senses, and some of which would
not. Sheldrake admits to the flaws in some of these experiments but then goes on to use all of them — flawed or not — for his assessment of the overall pattern of results. He also gives detailed results of some highly flawed studies but then gives only a cursory description of experiments that would, if valid, be very impressive.

Finally, Sheldrake’s conclusion reveals the same confusion. Of course most people say they have sensed when they are being stared at. It is a normal, evolved, human talent. Whether there is also a paranormal ability to detect staring, as Sheldrake seems to believe, remains unknown, but we will not get closer to knowing the truth by reading this misleading and confused paper.

In Part 2 the entire discussion is marred by a confusion between ‘active v. passive theories’ and ‘intromission v extramission theories’. Sheldrake points out that intromission theories have tended to regard vision as passive while extramission theories have tended to regard it as active. This may be true as a historical fact but this is no reason to conflate two fundamentally different distinctions. All modern theories of vision are intromission theories; they assume that light enters the eye and that nothing leaves it. This fits with the physics of light, the structure of the eye, and the principles of sensory systems. Any theory that proposes, as Sheldrake does, that ‘An influence seems to pass from the observer to the observed’ (p. 32) is a paranormal theory. The problem for normal theories of vision is to understand how incoming information eventually leads to visually guided behaviour and visual experience, and they vary from more active to more passive theories. There has recently been a revival of Gibson’s ecological approach to vision and renewed enthusiasm for so-called active and embodied theories of vision, but these do not involve anything leaving the eye and projecting out into the world; their main point is to emphasise how much active processing of the incoming information has to be achieved.

The most extreme of these theories is probably O’Regan and Noë’s (2001) sensorimotor theory of vision. This does away entirely with any notion of a picture-like representation in the visual system and replaces it with the idea that vision is a kind of action; seeing is doing. As they put it, seeing is mastering sensorimotor contingencies, or playing with the relationships between one’s own actions and the changing input. This is an active theory par excellence but it contains no hint of extramission. Sheldrake praises Gibson’s theory because it ‘leaves open the possibility of an interaction between the perceiver and the perceived’ (p. 41). But this is crazy — for what theory does not? Vision is and must be an ongoing interaction between the perceiver and perceived. By confusing two fundamentally different distinctions between theories of vision Sheldrake has created nothing but an unhelpful muddle.

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According to a well-known adage of folk psychology, ‘where there is smoke, there is fire’. In his two-part contribution to this symposium on ‘the sense of being stared at’, Rupert Sheldrake examines some of the smoke (fictional allusions, anecdotal observations and reports, and the persisting lore) surrounding this alleged phenomenon and attempts to determine whether such smoke might indeed be accompanied by some actual fire (valid and reliable evidence for the existence of the staring phenomenon). In this effort, he provides an extensive and inclusive account of his own research findings and those of other investigators.

Many contemporary professional psychologists and consciousness researchers would qualify the smoke/fire metaphor. They could suggest that smoke might indicate where a fire may once have been rather than where one presently exists. In their typically skeptical stance, they might question whether there is even smoke at all, but rather a fog, a cloud descended to earth, or an artifact of blurry vision. In this commentary on Sheldrake’s contribution, I will attempt to clarify whether a valid and reliable staring detection process might indeed exist — as a true fire generating all of this smoke or, at least, as some smoldering embers — and offer suggestions about its nature.

Although Sheldrake uses the term ‘the sense of being stared at’, I will substitute the term *staring detection*. The latter is less awkward, and it does not contain the suggestion that a sense is involved. The term sense suggests a sensory process that staring detection might not involve. If sense is used as a synonym for feeling, this usually is most appropriate, in that staring detection often does involve such a subjective feeling, and it also is often accompanied by physiological indicators (e.g., tingling, burning, pressure, hair standing on end) that reflect feelings. However, it is possible for the phenomenon to involve what might better be termed a form of direct knowing, rather than a sensing or feeling. In still other cases, behavioural reactions might betray the presence of staring detection — with or without accompanying conscious awareness of the staring aspect itself.

In responding to Sheldrake’s papers, I organize my remarks into five categories. These will treat the possible fictional, physical (including physiological), perceptual (chiefly visual), and attentional/intentional aspects of the staring detection process, as well as the implications of staring detection studies.

**I: Is Staring Detection a Fiction?**

Sheldrake begins his contribution by indicating how staring detection has been reported in lived experience surveys, treated in fictional stories and novels, and described by police officers, surveillance personnel, soldiers, hunters, photographers, and television personalities. It would be of great interest to study such field reports more carefully and in greater detail. In addition to such anecdotal reports, however, Sheldrake reviews a not insubstantial number of careful
laboratory investigations of staring detection. After reviewing the research evidence that Sheldrake and others have collected, one cannot doubt that staring detection is a real and replicable phenomenon. Statistically significant staring detection effects have been repeatedly observed in Sheldrake’s own research and in studies he has conducted with, or prompted in, others. Several meta-analyses have indicated the validity and reliability of staring detection (e.g. Schlitz & Braud, 1997; Schmidt et al., 2004). In short, careful research has supported the conclusion that the staring detection effect is not fictional. It can even be pointed out that Sheldrake’s analyses of the reality of staring detection in his own studies are conservative ones. This is because Sheldrake’s use of nonparametric tests such as chi-square and sign tests on nominal data do not consider the strengths of his obtained effects, as would analyses based on ordinal, interval, or ratio scores. Of course, not every experiment yields positive outcomes. This is to be expected of a complex human phenomenon that would likely be influenced by individual differences, predispositions, history, set, and setting.

II: Is Staring Detection a Product of Physical or Other Artifacts?

Given that it has been shown that persons are able to accurately indicate when they are being stared at, by someone out of the visual range of the staree, the next step is to determine whether such accurate detection might or might not be contaminated by conventional artifacts or confounding variables such as coincidence, sensory cues (subtle sounds or other physical cues), or biases in the experimental designs. The analyses of Sheldrake and others indicate that such confounding factors can be ruled out as sources of the obtained effects. For example, sensory cues can be ruled out by having the starer and staree adequately shielded from each other through the use of one-way mirror staring or closed circuit television staring. Appropriate statistical analyses effectively rule out correct guesses attributable to ‘chance coincidence’. Sheldrake addresses well and properly dismisses suggested methodological artifacts such as reporting bias and staring/nonstaring period scheduling in experiments that might introduce systematic biases that could mimic a real staring detection effect. Particularly useful are Sheldrake’s observations that the initial findings of sceptical replicators of this work tend to be positive. Sheldrake convincingly argues that the sceptics then reexamine their data in attempts to explain away their positive findings. More trenchant still is Sheldrake’s indictment of sceptics who postulate hypothetical processes or changes in their or others’ data that might simulate real staring detection effects without demonstrating effectively that such processes or changes really have occurred. Sheldrake is careful to address studies in which possible artifacts have not been adequately controlled (e.g., the so-called NEMO tests), and he properly describes such results as suggestive only.

Additional evidence indicative of real staring effects, as opposed to artifactual ones, are the significant correlations that have been found between staring detection accuracy and scoring on personality tests and similar standardized assessments administered to starees. Such correlations have been found in studies by

III: Is Staring Detection a Perceptual (Chiefly Visual) Process?

Throughout the two Parts of his contribution, Sheldrake appears to treat staring detection as a perceptual — i.e., visual — process, in which the starer somehow ‘reaches out and touches’ the staree. This sort of spatial model/metaphor is an obvious first interpretation, given the circumstances and belief contexts in which this phenomenon originally was, and continues to be, observed. Such a view, however, does not plausibly account for laboratory findings that staring detection can successfully occur when the staree is viewed indirectly, via one-way mirrors or via closed circuit television. In such cases, it seems more appropriate to conclude that staring detection may be one of many manifestations of the causal efficacy of remote attention and intention (treated below). Also, the resultants of visual and other perceptual processes are definite subjective experiences; usually, these are specific and clear. Although such subjective experiences — albeit sometimes relatively vague ones — often accompany staring detection, this is not invariably the case. Staring detection can be indicated by physiological (especially autonomic) or behavioural (movements, turnings) reactions of which the staree might be unaware or only vaguely aware. Such reactions may be indicative of a form of knowing or a sensitivity that would seem to fit the outcome of remote attention or intention more closely than that of a visual or other perceptual process.

In my view, an interpretation that posits a kind of reaching out and touching process indicates a general discomfort with action at a distance. Just as nature abhors a vacuum, so, too, does human intellection and understanding abhor seeming gaps in observed phenomena. We are strongly disposed to fill such gaps with bridging, continuously connecting processes such as material substances, channels, and ‘energies’; and it is tempting to attempt to do this with respect to staring detection. There are, of course, cases in which action at a distance now is accepted as a truism in several areas of conventional, relativistic, and quantum physics, although such actions were vigorously opposed when first suggested. The positing of fields is one approach to explaining some instances of action at a distance. Sheldrake’s own notions of morphic or morphogenetic fields illustrate such an attempt. In cases of more conventionally recognized fields (such as gravitational and electromagnetic fields), however, the presence and strengths of such fields can be indicated mathematically and the fields can be operationally detected. One wonders what the notion of a morphic or morphogenetic field really adds, in an explanatory sense, if such fields cannot be detected, operationalized, or characterized apart from reactions or outcomes that such fields are invented to account for in the first place. Are such fields truly explanations or simply renamings of already observed outcomes and of things to be explained?
IV: Is Staring Detection an Indicator of the Efficacy of Remote Attention and Intention?

Staring at another person is one way of paying attention to that person. Often, this attention is mixed with a specific intention — perhaps getting the other person’s attention, an attempt to make contact with the person, or some other motivated observation. It is possible that such instances of strong, focused attention and/or intention may be sufficient to induce staring detection or ‘the sense of being stared at’ whether or not looking or staring actually occurs.

In our own laboratory research (Braud et al., 1993a,b), we began studying staring detection because of our curiosity about whether remote attention alone might be accompanied by distinctive physiological reactions of the remote object of such attention. We had been conducting studies in which we had found that specific, directional intentions of one person — e.g., intentions or wishes to calm or activate — were accompanied by those intended reactions in remotely situated other persons (Braud & Schlitz, 1989; 1991; Radin et al., 1995; Schlitz & Braud, 1997). We recognized that in these experiments, the influencer’s intention (for a specific physiological and subjective change) was mixed with her or his attention (i.e., paying attention to the person whom one wished to remotely influence). So, we designed experiments in which one person simply attended to another, remotely situated other person, without intending for any particular reaction in the latter. In these experiments, one person watched the image of the remote person on a closed circuit television monitor as a way of focusing attention on the remotely situated person during certain periods, compared to other periods in which attention was not deployed in this manner. We used measures of sympathetic autonomic nervous system activity (skin conductance reactions) as a measure of the detection of this increased attention by the ‘target’ person. As Sheldrake indicated in his contribution, and as in other similar studies, we obtained evidence for successful autonomic staring detection in these studies (Braud et al., 1993a,b). Several meta-analyses of these, and similar, experiments have indicated the significance and reliability of such studies (e.g., Schlitz & Braud, 1997; Schmidt et al., 2004).

As a relatively pure test of the relative contributions of physical staring versus attention alone, a comparative experiment readily could be conducted. In such a study, persons would stare (via closed circuit television) versus not stare at others, whose physiological reactions would be monitored during multiple staring and nonstaring periods. In another part of the study, persons would strongly focus attention or not, upon physiologically monitored persons, but would do this without actually staring at the TV monitor images of the ‘target’ persons. The study could be extended even further by including four types of conditions: physical staring with no (or minimal) attention (this could be accomplished by a more passive form of staring, which would involve very nonmotivated, ‘witnessing’ looking); strong, focused attention but without physical staring; both staring and attention; and neither staring nor attention. Comparing results for the various
conditions could greatly illuminate the roles of (physical) staring and (psychological) attention in staring detection experiments.

Additionally, the variability of results in staring studies — both across different investigators and at different times and circumstances within the work of a given investigator — seems more consistent with an attentional/intentional interpretation than with a physically effective staring interpretation, in that physical staring would be expected to be rather straightforward and consistent across many experiments, whereas great variations might be expected in the ability of starees, starers, and investigators to strongly and consistently focus their attentional and intentional skills in these experiments.

In still other experiments bearing on attention, we have found evidence that it is possible for persons to facilitate the attention (in the form of concentration on some centering object) of other, distantly situated persons, through deploying their own attention in a similar ‘distant helping’ manner (Braud et al., 1995).

In all of the above, we are interpreting staring, intention, and attention as having causal efficacy in actively producing changes in distant persons. Such an interpretation, of course, can be qualified by the possibility that what we are witnessing in all of these experiments is the appropriately aligned simultaneous co-arising of the staring/intention/attention activities of the starers and the reactions of the starees, in ways that have been conceptualized as synchronicity (by Carl Jung and his followers) or as dependent origination (by those within various Buddhist traditions).

V: What Are the Implications of Staring Detection Findings?

The experimental results of Sheldrake and others indicate that accurate detection of an unseen gaze (called staring detection in this article) is an ability that seems to be relatively widespread in the human population. As in the case of other abilities or skills, this staring detection skill may be present to varying degrees. Differences found across various studies might be attributed to differences in the sensitivity of the starees and to differences in the staring skills of starers. These differences, in turn, may be due to differences in the abilities of starers and starees, alike, to fully deploy their attentional and intentional processes.

In the most general sense, the positive outcomes of studies of this kind extend our appreciation of the range of our human potentials and of the exceptional experiences and abilities of which we are capable, which cannot be explained adequately by the constructs, theories, and worldviews of conventional science. The studies suggest a profound interconnectedness among the participants in such studies. This interconnectedness, in turn, has important implications for our understanding of our full nature, as humans, and of the range and limits of the consciousness-mediated influences that we may exert on one another. In the view of the present writer, the most important of the consciousness-related processes implicated in the staring detection effect are the fundamental ones of attention and intention. Studies addressing a variety of forms of attention and intention, in connection with staring detection, can readily be designed and conducted, and such studies should greatly expand and illuminate our understanding.
of this curious effect and of the nature of consciousness itself. Perhaps the most important implication of staring detection findings for consciousness studies are their indications that, under certain conditions, consciousness may have nonlocal aspects.

I began this article with an allusion to folk psychology. I think this allusion is apropos in an additional way — in terms of Sheldrake’s ongoing championing, in many of his writings, of a popularization or democratization of psychology, research, and of science itself. He has done this by urging research on commonly experienced, albeit unusual, processes and phenomena, and by encouraging research by students and by members of the general public. Such more democratized inquiry can serve to complement and balance the typically exclusionary versions of research and inquiry practiced by professionals. Such an approach might encourage a greater interest in, and participation, in consciousness studies and in science and disciplined inquiry in general.

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Detection of Staring — Psi or Statistical Artifact?

*I will first discuss the evidence for detection of staring given in Part 1 of Sheldrake’s paper and then add a brief comment on the theory proposed in his Part 2.*

The term *detection of staring* refers to the ability to be aware that another person is staring at you, without any means of detection by sensory perception or any means whatever that can be described by presently known physical laws. This means that detection of staring is an aspect of psi, the latter being the general
name for phenomena that take place in accord with mental intention and cannot be accounted for by those laws. It is, of course, possible that an addition or extension to known physical laws could be made which takes into account the action of consciousness. Presently known physical laws were never intended to describe consciousness, but only matter. And the history of physics shows that radical additions to physics are sometimes made as new phenomena are taken into account. Nevertheless, if psi is real, a radical extension of physical laws, which takes into account the action of consciousness, would have to be made (Burns, 2003). So evidence for psi need not be considered conclusive if some alternative explanation which falls within the purview of presently known laws can reasonably be offered.

One possibility that could offer such an explanation is that of bias matching. This can occur if the target sequence is not properly randomized. In more detail, the starer is given a sequence of instructions, *stare or not stare*, with said list supposedly randomized. The staree then produces a sequence of responses, *stare or not stare*, according to his impression at the appropriate times. However, as is known by numerous experiments, if a person is asked to produce a sequence of binary choices, such a sequence is apt to be patterned, not random (Brugger and Taylor, 2003). Such sequences are especially apt to show alternation bias, i.e., they will show fewer doubles, triples, and higher n-tuples than a random sequence would, and therefore have more alternation from one choice to another than a random sequence. So if the target sequence happens also to have alternation bias, these sequences can match up (Brugger and Taylor, 2003). The effect will look like psi, but has an ordinary explanation.

As Sheldrake reports (p. 24), target sequences he had used from his website had been randomized with respect to other considerations, but not with respect to alternation bias. Colwell *et al.* (2000) analysed the first 12 sequences from that perspective. If these 12 sequences (of 20 targets each) had been random, they should have had a total of 114 two-element repetitions; there actually were 89. There should have been 54 same-element three-tuples; there were 22. There should have been 25.5 same-element four-tuples; there were 4. And so forth.

Colwell *et al.* (2000) used these sequences to perform the same experiments Sheldrake had done. When target-by-target feedback was given, participants performed at higher than chance level. However, this could be interpreted as a matching of the bias in the target sequence with the alternation bias of the staree, so this result need not be attributed to psi.

Sheldrake (p. 21) notes that pattern matching through implicit learning could account for above-chance results in his experiments with feedback, and he quite properly does not offer these results as conclusive evidence for the staring effect. However, Colwell *et al.* (2000) reported that a learning effect took place in their experiments with feedback — each successive block of three sequences showed improvement in detection of staring, but detection of non-staring remained at chance — and Sheldrake (p. 24) asserts that implicit learning could not account for this contrast between staring and non-staring sequences. But a staree might have a preferential interest in the *yes* response and, with feedback, use implicit
learning (rather than psi) to preferentially improve his response to that part of the target pattern. So the learning effect does not establish the existence of staring detection either.

Sheldrake (p. 21) asserts that pattern matching in the sequences could not take place without feedback and offers the experiments described in Sheldrake (2000), done without feedback, as evidence for the staring effect. However, the question of whether pattern matching can take place in the absence of feedback deserves a little more examination.

None of the sequences — target or human response generated — would have alterations which go on indefinitely. Sometimes an element will repeat, or there will be a triple or quadruple, and even-numbered repetitions put the new alternative out of phase with the previous one. If a set of response alternations is matched to a set of target alternations, and one set changes phase, the responses will now be mismatched. When there is feedback, there apparently is a tendency for the response sequence to go back in phase. So the target matching is higher than chance. Without feedback it would seem that the alternation portions of the target and response sequences go in and out of phase, with no net result.

Colwell et al. (2000) did Sheldrake’s staring experiment without feedback, using the above-described 12 sequences with alternation bias, and found chance results. This supports the idea that even when target and response sequences are both biased, without feedback there is no net matching. Sheldrake (2000) got above-chance results with no feedback, which he claims is evidence for the staring effect. He ascribes the chance results of Colwell et al. to the experimenter effect, i.e., that some experimenters produce psi more readily than others (Smith, 2003).

However, questions remain. For one thing Sheldrake (2000) reported that the experiments used 24 sequences, which suggests that he used the 24 sequences on his website that turned out to have defective randomization. If a target sequence is susceptible to bias matching with feedback, it is possible that a matching effect might also take place without it. So the above-chance results in these experiments do not confirm the existence of the staring effect.

This brings us to the question of what sort of randomization is needed for a staring experiment. Colwell et al. (2000) reviewed various randomization procedures and concluded that specific testing of target sequences for same-element n-tuple deficits should be done. In a second set of experiments they used 10 new target sequences of 20 binary digits each which were derived from a random number table. They tested for deficits in two- and three-tuple same-element repetitions and found the sequences satisfactory. A staring experiment with feedback using these sequences provided chance results, and they concluded that the difference from the earlier experiments with feedback, which gave an above-chance result, was the lesser degree of structure in the new sequences. However, there does not seem to presently be any standard procedure to ensure against alternation bias in target sequences.

There is also the question of what sort of matching biases could occur in the absence of feedback. Brugger and Taylor (2003) suggest that if the first element
of the response sequence can be correlated to the target sequence, a matching bias can be produced. On the other hand, Palmer (1996) investigated the possibility that local patterning within a random target sequence could match with patterns in a response sequence to give a net matching effect. He found, for the well-randomized target sequences he investigated, there was no significant effect.

It would seem reasonable to randomize target sequences sufficiently to guard against possible matching with biases in response sequences, even when there is no feedback. However, response biases can vary according to personality type and other variables (Brugger and Taylor, 2003), so guarding against possible matching is not a matter of dealing solely with alternation bias. At present there is no agreement as to what randomization procedures should be used.

As we have seen, Sheldrake’s experiments done with feedback cannot be used to demonstrate the existence of the staring effect because the target sequences were not sufficiently randomized to guard against alternation matching. And the experiments reported in Sheldrake (2000) cannot be used to demonstrate its existence because (a) the target sequences might not have been randomized against alternation bias and (b) not enough is known about what randomization procedures can guard against bias matching in general to make a definitive proof. Sheldrake also asserts that parapsychology experiments using closed circuit television (CCTV) and recording the subjects’ skin resistance can confirm the existence of the staring effect. Let’s examine this question next.

There are two types of parapsychology experiments which are relevant to the staring effect. One is called remote staring. In this experiment the agent (starer) and receiver (staree) are in two separated rooms, well insulated from any sensory communication. Once they are in these rooms, the agent follows a sequence of stare, non-stare instructions. He can view the receiver on CCTV, and for a stare instruction the agent stares at the image of the receiver on the monitor. For a non-stare instruction the agent looks in some other direction and thinks of something else. There are rest periods between instructions. The electrodermal activity (EDA) of the subject is recorded continuously throughout the session. When the trials are done, the EDA for the stare periods is compared with the EDA for the non-stare periods, and a statistically significant difference between these results would indicate the presence of psi.

The other relevant type of parapsychology experiment is called Direct Mental Interaction with Living Systems (DMILS). This experiment is very similar to remote staring experiments. However, the agent’s instructions are to have the intention to either activate or calm the autonomic nervous system of the receiver. And instead of the agent’s viewing the receiver on a monitor, he views the ongoing record of the receiver’s EDA.

The remote staring experiments show a difference in EDA between the staring and non-staring conditions, and the DMILS experiments show a difference between the calm and activate conditions. It would seem that these experiments, with their extensive shielding of any ordinary sensory communication and use of a physiological measure of the activity of the autonomic nervous system, could...
resolve the issue of whether detection of staring and DMILS exist. However, recent studies have shown that the methods used to measure the EDA in these experiments were not completely up to date with current psychophysiological methods (Schmidt et al., 2001; Schmidt and Walach, 2000). So a recent meta-analysis has concluded that while past experiments hint at the possibility of these psi effects, further experiments using high quality methods are needed before any definite conclusions could be drawn (Schmidt et al., 2004).

In summary, Sheldrake’s work, while interesting and suggestive, does not establish the existence of the detection of staring phenomenon. As we have just seen, parapsychology experiments have not established the existence of this phenomenon either. It remains an interesting possibility, but only that.

In Part 2 Sheldrake points out that even if a phenomenon is not established, one can go ahead and theorize about it. Certainly this is so, but Sheldrake seeks to make his theory of morphic fields explain a large number of things, not all of which need any explanation beyond the physical principles we already know. For instance, he asserts that his theory can explain protein folding. However, while it is difficult to predict the three-dimensional configuration of proteins, this is because a folded protein consists of a long ribbon which is held in its configuration because of numerous interactions between parts on neighbouring strands (Creighton, 1994). There is no reason to think the shape of a folded protein cannot be explained by ordinary physical laws.

On the other hand, Sheldrake proposes his theory as an explanation for detection of staring, a form of psi. If psi is real, its explanation must assuredly be different than known physical laws (Burns, 2003). In this respect, Sheldrake’s proposals can be viewed as a theory of psi.

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R.H.S. CARPENTER

Does Scopesthesa Imply Extramission?

The fact that an opinion has been widely held is no evidence whatever that it is not utterly absurd; indeed in view of the silliness of the majority of mankind, a widespread belief is more likely to be foolish than sensible.

— Bertrand Russell, *Marriage and Morals* (1929)

The most savage controversies are those about matters as to which there is no good evidence either way … Whenever you find yourself getting angry about a difference of opinion, be on your guard; you will probably find, on examination, that your belief is going beyond what the evidence warrants.


Nothing is better calculated to enrage a right-thinking scientist than popular belief in a phenomenon whose existence cannot quite be disproved, but which if true must bring into question the most fundamental axioms on which the scientific description of the world is constructed. The existence of a sense that one is being looked at — one might perhaps call it * scopesthesa*, which is briefer and sounds more scientific — is just such a belief. It is indeed popular. As Sheldrake says, ‘most people have had the experience of turning round, feeling that someone is looking at them from behind, and finding this is the case’ (p. 10): I have myself, quite often. However, I also know how very fallible my perceptions can be. Invariably, sitting in a stationary train when a neighbouring train moves off, I think it is me that is moving. When I see the moon near the horizon it looks twice as big as normal. I am a sucker for every kind of geometrical illusion — Müller-Lyer, Poggendorf, Zöllner — the lot. And as a professional psychophysicist I know very well how little correspondence there can be between even one’s strongest perceptual convictions, and what is actually the case.

Rupert Sheldrake is also well aware of these problems, and it is very much to his credit that he has made this attempt to assemble the kind of statistical evidence that is needed to convince scientists of the reality of scopesthesa. It is not his fault that he comes to this task with a personal conviction that the phenomenon exists: there can be very few scientists who do not have strong hopes and fears about the outcome of their experiments. And it is not his fault that some of the evidence that he brings forward is so very weak: although the nature of the Bayesian process that underlies scientific experimentation is that all evidence, whether weak or strong, is cumulative, nevertheless evidence that is feeble, or too tendentiously interpreted, has a strongly negative psychological effect, and he might more judiciously, if less conscientiously, have passed over it in silence.

So what does this evidence, in the end, amount to? Two inter-related phenomena are considered: (a) the sense of being looked at that is confirmed on turning round, and (b) the ability to cause people to feel looked at, and consequently to turn round, simply by looking at them. Clearly (b) is the stronger of the two in the sense that if it could be convincingly demonstrated it would provide a complete explanation for (a); on the other hand (a) on its own could be explained in many ways that would not violate current scientific orthodoxy, as Titchener pointed
out over a century ago. It may be that subjects tend to recollect those occasions when their scopesthesia was confirmed, and forget those when it wasn’t. In addition, the act of turning round is very likely to attract the gaze of someone behind: since it takes about a second to turn one’s head but the latency of a saccade is only about a fifth of a second, the viewee is more likely than not to find the viewer looking at them. Thus the popularity of the belief in (a) — undoubtedly widespread, as Sheldrake convincingly illustrates — provides only the weakest of evidence.

What about (b), the ability to induce scopesthesia by actively looking at someone? Sheldrake again adduces a quantity of anecdotal information, open as always to the criticism that favourable events are better remembered than unfavourable. But here at least there is the possibility of what a scientist would recognise as a proper controlled experiment. We need to eliminate those unsupervised tests where cheating was possible, those where individual participants could choose whether to contribute their results or not, those where covert cues could have been transmitted, and those where the experiments are insufficiently described to be able to evaluate them. From Sheldrake’s list, and using his very fair assessments of their value, it appears we are then left with just five published experiments (Coover, 1913; Poortman, 1959; Williams, 1983; Marks and Colwell, 2001; Wiseman and Smith, 1995 — see Sheldrake’s list of references for details). Of these, three gave negative results. Since there are many ways in which artefactual correlations can arise in such experiments, but rather few that can result in masking of a genuine effect, and bearing in mind that a positive result is more likely to be considered worth publishing, it is difficult for an unbiased enquirer not to conclude that scopesthesia is an illusion.

But Sheldrake clearly feels otherwise, and the second part of his paper is a description of a theory of vision that he believes to be a necessary consequence of the existence of scopesthesia. It is that vision is not simply the result of a one-way flow of information from the observed object to the brain; there are ‘perceptual fields that link the observer to that which is perceived … rooted in the brain but [extending] far beyond it’ (p. 32). As with scopesthesia itself, Sheldrake attaches much importance to popular (and ancient) belief, in this case that vision is something that proceeds from the eye out into the real world — ‘extramission’ — rather than vice-versa. We are told (p. 37) that 80% of a group of children in Ohio agreed that vision involved ‘rays, energy or something else’ being sent out from the eyes — and for good measure, 75% of them said they could ‘feel the stares of other people’. This less than compelling piece of evidence — in the same vein, I would predict (and Amazon might confirm) that those who believe in crystal healing also tend to believe in astrology — is reinforced by a long account of pre-scientific philosophers who professed a similar belief.

This notion is meant to solve what in conventional neuroscience Sheldrake seems to regard as an intolerable reductio ad absurdum: that when one looks at an elephant, the elephant is in some sense inside one’s head. How can this be? Obviously, most would reply, because in some other sense the elephant is
actually projected on to the outside world; the apparent contradiction need worry
us no more than the fact that the retinal image of the elephant is upside-down.
But on this pseudo-paradox Sheldrake erects a shaky edifice of ‘perceptual
fields, extending out beyond the brain … fields of probability … [resembling]
the fields of quantum field theory’ (pp. 42, 44). Inevitably, Schrödinger’s cat is
dragged out of his box, the mantra of quantum entanglement of observer and
observed is intoned, and one mysterious phenomenon is ‘explained’ by another.

Of course Sheldrake understands perfectly well the unsatisfactory flimsiness
of all this, and admits that such ‘speculations … are still vague’ (p. 48); his
defence is that neuroscientific explanations of the consciousness of qualia are
equally vague, and I would absolutely agree with him. If incontrovertible evi-
dence could be found that scopesthesia actually existed, then that would indeed
rule out a number of ‘scientific’ theories of consciousness; but no such experi-
mental support exists. Over-hyping weak evidence — as recent political events
have shown — does not always have the effect that is intended.

CHRIS CLARKE

The Sense of Being Stared At:
Its Relevance to the Physics of Consciousness

In my response I am taking it that Rupert Sheldrake has established a prima facie
case for this phenomenon, and I focus on his second — theoretical — paper,
where he proposes that the sense of being stared at involves a process in which
‘projection [of the perceptual visual image onto the external object] takes place
through perceptual fields’ (p. 42). The phenomenon thus concerns conscious-
ness studies because it is connected with a process of projection which many
authors see as central to the production of the visual content of conscious aware-
ness. I agree with a core aspect of this idea; but, as I shall argue below, I do not
think that Sheldrake’s notion of ‘field’ adds anything helpful to this core aspect.

An essential issue here is the nature of the ‘projection’, in which a perceptual
image or representation of the perceived object is formed by processing in the
brain, but then is located not in the brain but in a space extending out beyond the
skull to the external world. Sheldrake cites Velmans, in particular, as using the
term ‘projection’ in consciousness studies. Now, if this projection is a purely
physical function of the perceptual system, then we are in danger of falling into
precisely the same regress that Sheldrake rightly criticises in the case of the ‘Car-
tesian Theatre’, in which the brain forms an internal projected image which is
then viewed by some kind of homunculus inside the brain. Whether the projec-
tion is internal or external, if it is purely physical then we still have the problem
of who or what then perceives it in order for it to enter consciousness — poten-
tially leading to an infinite regress. Velmans steers well clear of this danger
because for him ‘projection’ is very much a metaphor, and the image is a strictly
non-physical construction within a conscious perceptual space that lines up with
physical space but is distinct from it.
For Sheldrake, the projection has, it would seem, to be both physical and perceptual. It has to be perceptual in order to avoid the regress of the Cartesian Theatre, but also physical in order to be a field (distributed in physical space and interacting with other fields) and to convey an influence to another person. His proposal is thus a kind of dual aspect theory in which the perceptual field (if not other systems) has both physical and mental (or noetic) aspects. The perceptual field links the starer and the staree into a single physical system that is itself possessed of consciousness. The conscious aspect, when focused down onto the starer, becomes the perception of staring; and when focused down onto the staree, becomes the perception of being stared at. This is the core idea that I find promising. Three issues arising from this must then, however, be addressed.

**I: Why is the Perceptual Field Necessary for Consciousness in Visual Perception?**

On the conventional view of vision, the perceiver and the object of perception are already linked together by an electromagnetic interaction (normally construed as one-way), so that they are no longer self-contained systems, but need to be considered as a whole. Suppose that we adopt a dual aspect view of this system, just as Sheldrake adopts a dual aspect view of the perceptual field. If consciousness is one of two aspects of such a combined system, then the object (or rather, aspects of it) will be part of this consciousness, associated with the place of the object, and it will be joined with a self-consciousness of the subject. These together will constitute the conscious perception of the object without the need for any projection or any further field. In a view such as this, the role of the information-processing system of the brain is to establish a physical (but one-way) link between specifically selected aspects of an external object and some particular subsystem of the brain associated with the ‘I’ of the perceiver.

The only thing added, in the case of ordinary vision, by doubling up the fields with a perceptual field in addition to the electromagnetic field, is an explanation of the subjective feeling that vision is somehow active. But that feeling is already explained by the fact that vision is indeed active: unlike the exposure of a photograph, it involves the complex activity of attention and analysis that Sheldrake himself describes.

In the case of staring, as far as the general contents of consciousness is concerned, the account just given seems adequate also to explain the sense of being stared at without the use of a further field. Just as the consciousness possessed by this joint system, when reduced to the starer, gives the perception of seeing, so, when reduced to the staree, it might give the perception of being stared at. At this level of generality, once a single system is established by an electromagnetic interaction, then no further sort of interaction seems required, either for normal vision or for the sense of being stared at. For the latter, however, an additional problem arises which I consider next.
II: How Is Information Transmitted?

It would seem that the only deficiency of an electromagnetic interaction for the staring phenomenon is that, in this case, it can only convey information in one direction: the back-reaction from the eye of the perceiver to the neck of the perceived is so minute as to be completely ignorable. (This remains the case with the Wheeler-Feynman theory considered by Sheldrake, which is designed to reproduce precisely the phenomena of ordinary electromagnetic theory, including unidirectional information flow.) Another field, going in the other direction, thus seems to be needed to explain the informational content of the sense of being stared at. But note that this move is now only a device to account for the physical fact of information flow in a parapsychology experiment and is not required as part of the theory of consciousness or of the theory of vision. It is thus not the case that the hypothesis of perceptual fields economically explains separate problems in parapsychology, vision and consciousness studies. Once reduced to the level of information flow, the elaborate series of properties assigned to the perceptual field in this paper seem to be doing no more than re-describing the phenomenon in other words, rather than explaining it, a point on which I enlarge in the next section.

I should add here that none of the quantum mechanical theories cited by Sheldrake overcome this problem of the direction of information flow, even though at first glance they seem to imply an action of the perceiver on the perceived. My own theory (Clarke, 2004), for example, considers the perceiver and the perceived to be entangled systems. But here it is well known that information cannot normally be conveyed between entangled systems without the existence of a parallel non-quantum channel of information flow.9 Admittedly the situation might be different in my theory because of its unconventional starting point. Following Whitehead, I consider a picture where pure states are regarded as the primary conscious elements in the universe and decoherence is held at bay by the Zeno effect of self-observation. Whether or not this circumvents the impossibility of communicating information through entanglement alone remains to be established, but it seems a long shot.

III: What Is a ‘Field’?

Sheldrake proposes that ‘the perceptual projection is not just analogous to but actually is a field phenomenon’ (p. 42). It would seem, therefore, that he has something quite specific in mind when using the word ‘field’, and that his concept of field is intended to have explanatory power rather than being merely descriptive of the phenomenon itself. To clarify this, let us compare the situation here with that of the magnetic field. At first this latter concept was merely descriptive: the magnetic field was a propensity, manifest at every point in space, for a compass needle to point in a particular direction. This was not a theory but merely a form of words describing the phenomenon. The situation changed

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9 A useful introduction to the quantum information theory involved here, including many references, can be found at http://www.imaph.tu-bs.de/qi/concepts.html
radically when it was noted that the magnetic field was governed by a simple law: that the flux of the field into any closed surface was precisely balanced by the flux out of that surface (or any of the equivalent versions of this statement). This immediately led to a host of predictions which could be tested, thereby moving from a description to a (partial) explanation.

I suggest that the term ‘field’, if it is to be explanatory and at all analogous to the situation with classical fields, needs to denote an entity that (a) is extended in space and has distinctive attributes at each point, and (b) is subject to some sort of law-like behaviour that is simpler than the phenomena that it is to explain. In enunciating (b) I require that the law-like behaviour is expressed directly in terms of the field, and not in terms of the field’s effects, because only then does the field itself become an explanatory concept.

Applying these criteria to the perceptual field, it seems to satisfy (a) in that it has, at each point, the property that any body placed at that point will enter the visual field at an appropriate position. Regarding (b) we could go on to elaborate laws for the perceptual field involving what happens to it if we interpose mirrors, prisms, lenses, etc. … but these would be none other than the laws of optics rephrased ‘in reverse’, adding nothing to what we already have in conventional theory. It is clear that Sheldrake is thinking of more than this, and he enumerates further properties of the perceptual field, shared by all morphic fields, which do indeed go beyond a reformulation of optics. I cannot, however, find here anything that would lead me to use the term ‘field’ for the carrier of these further properties. I will conclude by explaining my difficulty in the case of the clearest examples of the properties of morphic fields. Sheldrake writes:

First, by their nature they could connect together patterns of activity in different regions of the brain (p. 44).

I suggest that, since this holds of almost any interaction at all, it is too non-specific to be regarded as law-like for a field.

Second, they contain attractors (p. 44).

An attractor, we recall (e.g. from Arrowsmith and Place, 1992), is a subset of the state-space of a dynamical system which has a non-trivial neighbourhood from which all trajectories approach the attractor, and which is indecomposable with respect to this property. Thus it is a property of a given dynamical system, not something which in itself specifies a dynamical system. To say that a dynamical system ‘contains attractors’ says almost nothing about that system. For instance, any dynamical system not everywhere ergodic and with a compact phase space will contain an attractor. This statement is therefore not in any way law-like. (Nor indeed is it clear, since it is ambiguous as to whether it refers to the dynamical system describing the way the field changes, or, in the particular case of a vector field, to the dynamical system generated by the field itself.)

Third, they link into a single system the subject and the object (p. 44).

We have just noted, however, that these are already linked by existing physical fields, and by any other interaction between them, whether a field or not. We
need to know the nature of the linkage to judge whether it can be called a field and whether it adds anything to the conventional picture.

Sheldrake’s original picture of the morphogenetic field was indeed a field because it was subject to a law in which its strength increased with a memory effect. I suggest that this is not the case with perceptual fields, and that the concept is in need of clarification before it can seen how it explains the sense of being stared at.

References

RALPH ELLIS

The Ambiguity of ‘In Here/Out There’ Talk:
In What Sense Is Perception ‘Out in the World’?

Sheldrake presents evidence that subjects have a slight but significant ability to detect being looked at, even when they apparently cannot ‘see’ the person looking at them in any traditionally understood scientific sense of ‘see’. I shall leave to others the critique of his methodology, and focus on the implications of his findings. Sheldrake rejects several alternative explanations, offering instead a theory that there are perceptual fields that extend ‘outside the head’. I argue that we do not need to descend to the quantum level, or abandon traditional accounts of perception, in order to accommodate the findings. The idea of perceptual fields, while possibly true, is not necessarily entailed by the phenomenon of awareness of being looked at.

I: ‘Representations in the Head’ and Perceptual Fields

Sheldrake emphasizes that people have an ability to detect not only being stared at from behind, but also when looked at on a TV monitor by someone in a different room. Informally, he endorses anecdotal accounts of military personnel who are aware of being stared at through telescopes or binoculars from a great distance, and he wants his theory to be able to accommodate these experiences of being stared at from a great distance, through closed doors, and via TV monitors. As far as the actual data are concerned, none of it would contradict such reports, and none of it suggests that the ability to detect being looked at is more pronounced at shorter distances, or even when the subject is in the same room with the looker.

Consequently, if the perceptual fields that Sheldrake characterizes as ‘outside of our heads’ are to account for these facts, they would have to be fields that remain very strong even at a great distance from the head. To get around this kind of problem, Sheldrake proposes a novel account of perception in terms of a
theory of quantum effects in the perceived field which purportedly interact with the perceptual fields.

The move to a quantum-level hypothesis is motivated by the observed anomaly that the distance between subject and looker does not seem to affect the subject’s sense of being stared at. This means that, if the quantum-level explanation is to avoid functioning as an ad hoc hypothesis, two conditions must be met: (1) the quantum hypothesis must be the best way to account for the anomalous findings; and (2) the overall theory about perceptual fields outside the head, which motivated the quantum hypothesis, must itself be supported by cogent arguments indicating that it is the most plausible approach to explaining the general phenomenon of awareness of being looked at.

The second of these two conditions is the more basic one, so let’s start with it. I have some problems with Sheldrake’s reasons for endorsing the perceptual fields outside the head theory. The idea that perception takes place ‘outside the head’ seems to be based on an ambiguous philosophical claim that ‘the perceived object’, ‘the percept’, or perception itself is ‘out there in the world’ rather than ‘inside the head’. But in fact, the theory that there are representations ‘inside the head’, which bears the brunt of Sheldrake’s criticisms of alternative accounts based on theories of representation, is a cartoon caricature of that type of theory, and thus a straw man. Sheldrake speaks as if traditional accounts of representation held that representations are thing-like entities or static physical structures that are literally held in some spatial location inside the head. I doubt whether any traditional believer in representation would posit such a simplistic account. Most, at least if they have studied much neuroscience, would grant that representing is an activity that the brain stands ready to execute when triggered in some way (for example, Thomas, 1989).

It is true that analytic philosophers, prior to the past decade of ‘consciousness revolution’, had badly mangled their accounts of what a representation is. But Natika Newton (1996; 2000; 2001; 2003; 2004) — the one ‘enactivist’ who has systematically provided an adequate account of representation in several different places (including those just cited) — is notably missing from Sheldrake’s discussion of the enactivist alternatives. On Newton’s account, representation does not mean an abstract symbolic event isomorphic to and caused by its referent, as traditionally postulated by theorists in philosophy of mind (for example, Fodor, 1975; 1987). She also does not mean purely pictorial representation, as the term ‘imagery’ traditionally implies. Instead, representation on her account is an activity in which we imaginatively simulate embodied sensorimotor actions toward a symbolizing element, where the isomorphism, as Merleau-Ponty (1942/1963) suggested, is between the actions afforded by the symbol and those afforded by what the symbol represents. That which the symbol represents, on this view, is determined not by the stimulus that originally produced it, but by the imagined actions performed upon it. Consciousness exists to help organisms unify their efforts so as to achieve goals that require long-range planning. Goals such as appeasement of hunger, whose satisfaction mechanisms are not built into the system, must be explicitly represented in such a way that the representations...
can activate higher-order goal-seeking activity. Just as a feeding mechanism recognizes when it has achieved a state of satiation, and when it has not, so a conscious organism requires a way of recognizing when it has achieved its more abstract goal. Lacking a hard-wired system for identifying, say, the state of having successfully completed one’s PhD dissertation, we need to create a representation of that state, and then organize our multiple abilities to achieve it, and to recognize when it has been achieved. Mechanisms for mental representation are designed to make this behaviour possible, through comparison processes.

Traditional accounts of representation emphasize that the representing entity must be both isomorphic to and closely causally linked with the represented element. Neither isomorphism nor causation, of course, can make one thing represent another in a meaningful sense. As Thelen et al. (2001) point out, any dynamic internal event causally related to something external would then constitute a representation. Newton’s position is that representation does occur — through a subject’s use of it in context of embodied (organismically-purposeful) action. This formulation allows for both conscious and preconscious representation: R represents object O if, because of some appropriate isomorphism, R plays the role of O in a simulated action involving O, either consciously or preconsciously (for example, habituatedly).

Notice that, on this account, there is no need to posit that representations are fixed entities or static structures in the brain. Representing is something that we do, when triggered in the right way by purpose-defined conditions. Representations are not little physical objects inside the head. But the kind of representation Newton wants to allow for (because of the role it plays in action planning) does acknowledge that certain activities of the brain are necessary and sometimes sufficient substrates of the execution of the activities that we call imaging and representing.

We can image (and thus represent) something that exists nowhere in the world ‘outside our heads’. And even when red appears to be pasted to the surface of objects, we know from a vast collection of scientific evidence that the way red looks is largely a function of the way our brains are designed to ‘represent’ certain collections of quantum events (actions of photons, etc.). In fact, the red is not pasted to the surface of the object, as it appears to be, but rather created by our brain activity. So to insist that the red is ‘out there in the world’ rather than ‘in our heads’ is both ambiguous and an attack on a straw man. Suppose we distinguish between things that are simply physical objects or structures existing literally ‘inside the head’ on the one hand, and on the other hand conditions of the brain that make it ready and able to execute the action of representing something in the way defined by Newton. Those representations are ‘in the head’ in a less literal and more complicated sense: they are activities of constellations of brain states that enable us to represent something when triggered. We can designate these two different senses of ‘in the head’ as ‘ITH-1’ and ‘ITH-2’. So we can say that Newton’s representations are ITH-2, but not ITH-1.

The most traditional theorists of perception and representation do not think that the appearance of red means that there is a red replica of the object in my
head (ITH-1), or even an object that is isomorphic to the red object that is perceived. But in the richer sense of ‘in the head’ (ITH-2), it is true that many of the activities that create the appearance ‘red’ are executed by the brain, and in that sense they do take place ‘inside the head’.

Sheldrake’s jump from the thesis that representations or percepts are not ‘in the head’ to the conclusion that perceptual fields surrounding our brains must physically extend out to where the object is located is based on the very equivocation of the meaning of ‘in the head’ we have just been discussing. In one sense (ITH-2), the enactive execution of a representation is done by things in the head (brain events), but this does not in any way imply that the intentional objects of those representations are physical objects in the head. Husserl (1900/1913; 1913/1931) and the other classic phenomenologists are very careful to make and clarify the distinction between physical objects and intentional objects as they appear to us, as well as to acknowledge that there are psychophysical relations between them (Husserl, 1962). The phenomenologists are certainly quite clear in spelling out that what is meant by an ‘intentional object’ of a conscious act is not a little physical replica of any object inside the head; but Husserl is also well aware that this account of intentionality is perfectly consistent with various alternative accounts of the ways in which specific brain functions are needed to subserve the intentional acts executed by conscious subjects.

II: The Realm of Alternative Explanations

There is a set of alternative possible explanations of phenomena like the awareness of being stared at when not mediated by direct perception. Ironically, Sheldrake’s preferred ‘perceptual fields’ theory, with its accompanying hypothesis about quantum interactions with the field, falls within the set of possible alternative explanations. What is needed is some way for a subject to gain awareness of its environment in some way other than through direct perception. In my view, Occam’s razor would suggest that we do not commit ourselves to new theories if an equally adequate explanation can be given using already tested theories.

The general class of explanations as to how awareness of being stared at could exist without perceptual mediation has been well characterized by Gendlin (1992):

To begin philosophy by considering perception makes it seem that living things can contact reality only through perception. But plants are in contact with reality. They are interactions, quite without perception. Our own living bodies also are interactions with their environments, and that is not lost just because ours also have perception. . . . Our bodies . . . interact as bodies, not just through what comes with the five senses. . . .

Merleau-Ponty . . . meant perception to include (latent and implicitly) also our bodily interactional being-in-the-world, all of our life in situations. . . . The body senses the whole situation, and it urges, it implicitly shapes our next action. It senses itself living-in its whole context — the situation. . . .

From one ancient bone one can reconstruct not only the whole animal, but from its body also the kind of environment in which it lived. . . . The body even as a dead structure still contains all that implicit information about its environment. . . .
My warmth or hostility will affect your ongoing bodily being whether you perceive it or not. You may find it there, if you sense how your body has the situation (Gendlin, 1992, pp. 344–53).

The body is affected by its overall interrelations with the environment; so, if we sense how our bodies have changed from one moment to the next, we may be able to guess somewhat as to what kind of environmental changes may have occurred to make our bodies feel different. Such guesses may not be very accurate, because for any effect there are a variety of possible causes. But if the guess is based on the presumption simply that *something* has changed in the environment, the odds are much better. On Gendlin’s type of account, we can detect when *something* has changed much better than we can detect what has changed. If we are given the clue that the change, if there is a change, will consist of a person looking at us from behind, then our odds of success are better, because we have the ability to feel our bodies changing, and our bodies are in interaction with the world.

Of course, one of the ways that our bodies are in interaction with the world is at the quantum level. But on Gendlin’s account, the detection of the change in the body is not *perceptual*. We do not need to have vision of a person in order to have a sense that she is there. There are many ways that this can be known — for example, in the amount of carbon dioxide being replenished into the atmosphere, in the reaction of nearby animals and birds to the presence or absence of the distant person, and the list could go on and on. Many of these cues can be eliminated in carefully controlled laboratory circumstances, but we do not know how many of them there are, and thus it would be difficult to control them all.

My point is that, while the quantum apparatus of the elaborate theory of perceptual fields is a hypothesis consistent with the data, there are also other hypotheses that are consistent with it, and many of them do not require adopting entirely new theoretical proposals.

Let me conclude with a final question that would seem to present a problem for Sheldrake’s explanation as well as for the entire realm of alternative explanations for these kinds of phenomena: Suppose it is true that subjects can detect being stared at on TV monitors. And suppose we set up an experiment in which twins are in separate studios. Two different lookers in different rooms, in randomized intervals, see pictures of the back of one twin’s head, alternating with a view of the other’s, but do not know which one they are looking at in any given moment. On Sheldrake’s theory, would the twins pick up on the experience of being looked at by the incorrect looker as often as by the correct one? Or would the twin subjects be able to detect when they were the one actually being looked at? The perceptual apparatus of the lookers would presumably be exactly the same when looking at one twin or the other; and the image on the TV monitor would be exactly the same, regardless of which twin is actually represented on it. So it would seem that there is no real physical difference between being looked at oneself and having one’s twin looked at. If the twins would be able to detect being looked at (and thus discriminate which twin is being looked at by which looker), there would seem to be no possible physical explanation for such an
effect, since there is no physical difference between the two conditions being discriminated — being looked at oneself and having one's twin looked at.

If a subject cannot tell the difference between being looked at and having his or her twin looked at, then the subject is not really detecting when he or she is or is not being looked at. So, on the hypothesis that subjects can tell when they are being looked at, they must also be able to tell the difference between being looked and having their twin looked at; but in this case, we would seem to be back to a situation where there is no possible physical explanation for such a detection, since there is no physical difference between these two conditions. Since Sheldrake's explanation is a physical one, it would seem to be unable to explain this type of situation.

On the other hand, if we simply rule out the validity of the studies in which people can detect being seen on TV monitors, then it is much easier to explain the more proximate detection abilities of subjects in terms of subliminal awareness of the presence of another living being by means of slight changes in atmospheric and other environmental conditions, of the kind that Gendlin discusses. And in that case, it is not necessary to descend to the quantum level for an explanation.

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In the absence of sensory clues, can we really be aware that someone is staring at us? The evidence, as reviewed by Rupert Sheldrake, suggests quite strongly that we can, yet many refuse to accept this evidence despite the fact that other human abilities have sometimes been acknowledged on the strength of rather less support. The problem that prevents the general acceptance of the staring effect is the absence of any known mechanism to explain how, without suitable sensory input, it can possibly occur. The absence of such a mechanism means that if we accept that it does happen we have necessarily to recognise the limitations of our known laws of science, and this we are reluctant to do. These laws stand us in good stead elsewhere and prompt many to insist that the existence of phenomena that contradicts them must be demonstrated to the satisfaction of even the sternest critic on the basis that exceptional claims require exceptional levels of proof. In spite of the fact that the staring effect has been demonstrated significantly in a number of well-controlled experiments, failure consistently to replicate these results is therefore readily accepted as convincing evidence that the former are flawed in some way, and that the staring effect is not to be taken seriously.

However, strict adherence to the need for consistent replication can have certain disadvantages in the context of human psychology. By their very nature, some human abilities tend to be fleeting and elusive, and if we demand they be demonstrated repeatedly — and to order — at the same evidential level expected from investigations in other areas of science we risk handicapping ourselves in our attempt to study them more deeply. It is notoriously difficult to replicate exactly the results of many psychological experiments due to the wide range of variables that influence human performance, particularly where different samples of individuals are being investigated or the same samples are re-tested under different circumstances. Thus the occasional failure in replication is normally not taken as an indication that the ability under scrutiny is illusory, but as an indication that we need to study the part played by these variables in inhibiting performance. In the case of the staring effect, unless we decide a priori that it cannot happen in the absence of a known explanatory mechanism, it seems clear that the positive results now yielded by a number of studies are sufficiently impressive for us to adopt as a working hypothesis the proposition that the effect and the ability responsible for it may be real. A working hypothesis is based upon the recognition that a particular body of evidence, however much it may go against existing models of reality, is strong enough to warrant further research and to allow discussion as to the implications it may have for the rest of human knowledge. Demonstration, in the context of the working hypothesis, is thus seen as acceptable evidentially even in the absence of explanation.

In addition to the experimental evidence that serves to support the hypothesis that the staring effect may be real, Rupert Sheldrake refers to findings suggesting
that belief in this effect is accepted not only by the great majority of children but by the great majority of adults. The studies concerned indicate that more than 80 per cent of women and more than 70 per cent of men believe not only that they know when others are staring at them, but that they can stare at others and make them turn around. Unless we are to consign without obvious reason some 80 per cent of our fellow men and women to the limbo of the irredeemably superstitious, we are bound as psychologists to take these findings seriously and enquire why people maintain this conviction. Do they do so simply in response to the beliefs of others, or are they speaking from personal experience? The latter would seem the more likely explanation. After all, the staring effect is something that each of us can readily put to the test. True we may be deceived about our personal experience — for example we may remember only the occasions when staring seems to work and conveniently forget all the many occasions when it does not — but it is equally true that we may be deceiving ourselves if we fail to credit our fellows with the ability to judge personal experience with at least some degree of accuracy.

In the case of the staring effect, this experience may be based not just upon quantity but upon quality, that is not just upon the frequency with which someone senses they are being stared at but upon the emotional reaction they experience when it happens. Similarly the starer may be aware, from physical clues, that the person at whom they are staring also appears to exhibit an emotional reaction of some sort. I may perhaps be permitted to offer a personal example of the latter experience, since it was one of the first occasions in my adult life when I became aware that the staring effect might be real. The person at whom I was (inadvertently) staring looked over her shoulder at me the moment she came into view with an expression of such startled surprise that I was equally taken aback when our eyes met. Even had I known and called out her name, her reaction could not have been more marked. At the time I was staring out of my study window at the empty road that runs virtually parallel to the left of my house (which is on a corner), and she came into view walking away from me. Unless she had eyes in the back of her head there were no sensory clues that would have betrayed my presence to her. The window at which I was seated was closed at the time, I was sitting still, and she would have had no reason to suppose I was there or any reason to wish to see me. The road is an exceptionally quiet one, and there were no distractions in the vicinity of my house that might have caught her attention. Anecdotal evidence such as this only carries conviction for the person who experiences it, but it can at times suggest ideas for further research. Thus it would be useful to establish if emotional reactions similar to the one witnessed by me on this occasion are common in the context of the staring effect. If they are, this may help explain why such an overwhelming number of adults believe in the reality of the effect — perhaps they know the emotional charge sometimes involved. Investigations into the differing nature of this charge from person to person might also help advance our knowledge of what is taking place and perhaps why it happens.
II: The Implications of the Staring Effect

It can hardly be doubted that the implications of the staring effect, if genuine, for our understanding of consciousness and of the mind–brain relationship are, to put it mildly, far-reaching. One of the most important of these implications has to do with the location (or non-localisation) of consciousness. Max Velmans in particular has long argued that consciousness extends throughout the body rather than being localised within the head or any other part of it. One of the examples he gives is that when we prick our finger the pain is felt in the finger and not in the head (Velmans, 2000). Further support for this argument comes from an exercise in the training of meditators that involves moving consciousness around the body in order to dispel the common misconception that it is located somewhere behind the eyes. This is an experiment that anyone can try, and involves placing the awareness exclusively in the sense of touch — first in the feeling of the feet against the floor, then in the ankles as they are gently flexed, and so on upwards through the legs and the body, the arms and the neck, and finally the head and the scalp. The object of the exercise is to allow the meditator to experience for him or herself the fluid nature of consciousness, and to recognise that the reason we habitually suppose it to be in the head is simply because five of the six senses are exclusively located there.

However, in spite of its fluid nature, consciousness in this meditation exercise (and in the pricked finger) remains linked to the body’s sensory — and therefore physical — mechanisms. The finding that it is not solely in the head complicates the process of establishing the true nature of consciousness, but a major speculative leap is required before we can suggest with any conviction that consciousness extends beyond the physical body and can embrace not only physical realities outside the unaided reach of normal sense perception but perhaps even non-physical realities such as the thoughts and emotions of others. Yet until the advent of scientific materialism in the eighteenth and nineteenth centuries this suggestion was accepted as a matter of course by most people in the West — and still is accepted by countless millions of our fellow beings elsewhere in the world. It is easy to dismiss such a belief as superstition, a pre-scientific effort to find meaning in a largely mysterious universe, but an alternative possibility is that for some people at least this belief was based upon direct experience.

Rupert Sheldrake takes up this point in his argument that consciousness is in fact a field phenomenon. The most striking feature of this argument is concerned with the third aspect of the mind–brain problem to which he refers, namely that the subject who perceives and the object of perception (i.e. the observer and the observed) are linked together into a single system. In his view the perceptual field of the observer extends ‘out beyond the brain to include or enclose the object of perception”. In the case of the staring effect, the perceptual field of the person responsible for the staring interacts with the perceptual field of the person at whom he or she is staring. At a stroke consciousness is thus presented not as a function solely (or even perhaps primarily) of brain activity, but as a thing in itself, obeying its own laws and not subject to normal physical constraints.
Amongst the various possible ways in which we might conceptualise this, the model proposed by Chris Clarke (1996) is perhaps the most appealing, namely that conscious perception depends essentially upon quantum entanglement between the quantum states of the observer and the quantum states of what is observed. Quantum entanglement, which refers to the propensity of each member of a pair of particles emitted from a common source to evidence correlations between their behaviour even when separated from each other by relatively large distances, is only partially understood (if indeed such phenomena can ever be ‘understood’ in our usual meaning of the term), but Rupert Sheldrake’s implication, following on from Radin (2004), is that as we develop more complex explanatory models of how entanglement takes place, the idea that mind (or consciousness) fields exist that are entangled with the rest of the universe may come to look increasingly plausible. If such is the case, the staring effect may be accommodated without undue difficulty.

Should the idea of entangled, interpenetrating mind fields prove increasingly to fit the models generated by quantum physics (and we have to be cautious and stress the if), then much of the distinction between subject and object will disappear. There are intriguing parallels between this possibility and the yogachara teachings of Mahayana Buddhism that originated in the fourth century CE. In terms of these teachings, things exist only as processes of knowledge. Thus there are no separate objects and no separate subjects, simply a flow of perceptions that produce the appearance of external objects within the mind (or rather within the mind field), together with the delusion of an individual self that is separate from these perceptions. As with the model of quantum entanglement, the staring effect would seem readily accommodated within these teachings. All mind fields are simply aspects of one interpenetrating field of perceptual experience, and the teachings insist that through intensive meditation practice one can become conscious of this fact and incidentally develop a heightened awareness of mind fields other than one’s own.

If the staring effect is real, this opens the way to a whole raft of questions about how the entanglement of mind fields is likely to work. For example, to what exactly is the recipient reacting? Do the physical actions of the starer as he turns his head and directs his attention register on this field, or does the field register only mental events? If the latter, what mental events are involved? The starer’s motivation to stare? The thoughts going through his head as he reacts to the person at whom he is staring? The emotions he or she arouses in him? Or is it a combination of all these things? Perhaps the answer to these questions may help us understand why it is that on many (probably the majority) of occasions we are not aware when someone is staring at us. Do some of us respond only if there is some perceived threat in the stare? Do some of us respond only if the stare is one of admiration or only if it is one of disapproval? Do some of us respond only if there is a measure of compatibility between our mind field and that of the starer? Are we always aware at an unconscious level of being stared at, but only aware consciously if we have a special sensitivity? Is there perhaps some form of mental filtering mechanism at work that prevents us from being invaded by the stares
and thoughts of others? Further research, taking in self-reports and autonomic reactions, might help answer some of these questions.

III: Conclusion

Whichever way we look at it, the staring effect is not one we can ignore. We should not be dissuaded from further research by \textit{a priori} convictions that the effect cannot happen. It might be too much to hope that research into it is ever likely to attract large grants. Research institutions (or rather those responsible for decision-making within them) are far too cautious for that. But if large-scale studies could be mounted there should be little difficulty in establishing once and for all whether the effect is genuine or not. And if it is genuine, we can then move on to investigate what light it throws upon our understanding of our own nature.

References


CHRISTOPHER C. FRENCH

A Closer Look at Sheldrake’s Treatment of Rattee’s Data

I will limit my comments on Rupert Sheldrake’s articles to two points, one specific and one general, both relating to his first article in which he reviews the available empirical literature on the alleged sense of being stared at.

The specific point relates to the brief description provided by Sheldrake of an experiment carried out by Neal Rattee in 1996 under my supervision. As Sheldrake points out, Rattee’s results would have just reached statistical significance if a one-tailed test had been employed instead of a two-tailed test. Two questions that Sheldrake does not address are (a) whether or not a one-tailed test is appropriate in this case and (b) if it is, what is the direction of the predicted difference in skin conductance levels between the stare and non-stare trials?

Every first-year psychology student is taught that it is appropriate to use a two-tailed statistical test when one is predicting a difference between two groups or conditions without predicting the direction of the difference and a one-tailed test if one is predicting the direction of the difference. In practice, most experimental psychologists prefer to avoid using one-tailed tests at all. One-tailed tests are less conservative in that one is twice as likely to obtain a statistically significant result if one applies a one-tailed test instead of a two-tailed test to a particular set of data provided that the difference between the two means is in the ‘predicted’ direction. One problem with the use of one-tailed tests (and the reason for the scare quotes around ‘predicted’ in the last sentence) is that it is sometimes possible to think up a superficially plausible reason for why one might have ‘predicted’ a difference in a particular direction \textit{after} one has carried out an
analysis and discovered that one has obtained a result which is only statistically significant if a one-tailed test is used. The suspicion might then arise that, had the difference in means been in the opposite direction, one could have produced an equally plausible reason for ‘predicting’ a difference in that direction. Such circumstances arise particularly for complex experimental designs involving lots of interacting factors, but also in situations where previous research has produced conflicting results. If one can find reasons to favour experiments producing outcomes in one particular direction as being methodologically superior to those finding the opposite pattern, one could again attempt to justify the use of a one-tailed hypothesis. It is precisely because most experimentalists are sophisticated enough to appreciate the ease with which such post hoc justifications can sometimes be conjured up that they mostly prefer to avoid the use of one-tailed tests altogether — unless, of course, their data absolutely forces them to make use of them! Experimental psychologists are only human after all, despite claims to the contrary.

Having said all that, there are also situations where the predicted direction of the difference between two means is so obvious on the basis of theoretical and previous experimental results that the use of a one-tailed test would hardly raise an eyebrow. So what was the situation with respect to Neal Rattee’s final year undergraduate project? Some commentators may feel that a two-tailed test was the more appropriate option simply because previous research had apparently shown different patterns of electrodermal reactivity in response to staring depending upon the nature of the participants being stared at (e.g., Braud et al., 1993; see below for details). But is it the case that closer examination of the theoretical justification and previous experimental results would show that Sheldrake was justified in recommending the use of a one-tailed test on Rattee’s data?

Sheldrake is curiously vague with respect to why measuring galvanic skin response (GSR) would be an appropriate technique to use in order to investigate the alleged ability to detect unseen gaze, limiting himself to stating that tests had been done in which GSR was recorded automatically ‘as in lie-detector tests’. It goes without saying that readers of the Journal of Consciousness Studies would know that the basic principle underlying the use of (so-called) lie-detector tests is that most people become more aroused when telling a lie. This leads to activation of the sympathetic nervous system and that leads to increased sweating. Sweating causes the conductivity of the skin to increase (or, to put it another way, the resistance to decrease) and this is what the GSR is measuring. Although not explicitly stated by Sheldrake, readers would implicitly assume that, if some of us can indeed sense when we are being stared at, this would result in higher levels of skin conductance for stare trials compared to non-stare trials on the assumption that being stared at would make us feel uncomfortable and therefore more aroused. It is clear from Sheldrake’s description of Robert Baker’s demonstration that he is implying that people typically do indeed feel uncomfortable when they sense that they are being stared at. So, is this what previous studies using GSR have actually demonstrated?
The answer is ‘not always’. It depends upon the nature of the person being stared at, the so-called ‘staree’. For example, in Braud et al.’s (1993) study, untrained starees showed increased arousal on stare trials compared to non-stare trials, as one might expect. Participants who had undergone ‘connectedness training’, however, showed the opposite pattern of results, i.e., they were less aroused for staring trials than for non-staring trials. This training involved ‘intellectual and experiential exercises involving feelings of interconnectedness with other people’ (p. 381). On the basis of previous experiments such as this, would it have been more appropriate to use a one-tailed test in analysing Rattee’s data?

If we are to do so, we must specify the direction of the predicted difference in means between staring and non-staring trials for the dependent variable employed, i.e., skin conductance. Rattee’s participants were 27 first-year psychology students participating ‘as a course requirement’ and three final year undergraduates. They did not receive any special training. Therefore, one could only reasonably predict that their arousal levels (and skin conductance) would be higher on stare trials compared to non-stare trials (or else use a non-directional two-tailed test). As stated in the experimental report, ‘A detect score was calculated for each subject by subtracting total SCL [skin conductance levels] during non-stare periods from that during the stare periods on the basis that the mean chance expectation would be 0.’ The actual difference in SCL was ‘–21.64’ (S.D. = ‘68.97’). Although the actual units of measurement are not specified, it is clear that the (untrained) participants were slightly more aroused during the non-stare trials than the stare trials. Thus, if we had used a one-tailed test predicting that they would have been less aroused for such trials, we would again have had to accept the null hypothesis. Whichever way you look at it, Rattee’s data cannot be said to support the hypothesis that people are able to detect unseen gaze even at the non-conscious level of autonomic activity.

The second, more general, point I would like to make is one that is likely to have occurred to many readers of JCS. In discussing possible artefacts that might explain the ‘repeatable positive results’ in studies of the detection of unseen gaze, Sheldrake takes each in turn (e.g., sensory leakage, cheating, implicit learning) and attempts to dismiss each of them by pointing out that there are studies which have controlled for each effect and have still produced positive results. However, without a reasonable number of studies that produce positive results while controlling for all of these potential artefacts simultaneously, it is not possible to rule them out as possible causes of positive findings. A recent meta-analysis of remote staring studies by Schmidt et al. (2004) concluded that ‘there are hints of an effect, but also a shortage of independent replications and theoretical concepts’ (p. 235). More pertinent to the current discussion is their comment that ‘one has to be careful when interpreting the remote staring data because there is a lack of high-quality studies and such studies may reduce the overall effect size or even show that the effect does not exist’ (p. 245). Unless and until such studies have been carried out and reported, no definitive conclusions can be reasonably be drawn regarding the reality or otherwise of the alleged sense of being stared at. The positive results reported to date, however, provide a very
strong justification for further and better quality investigations of this intriguing topic in the future.

References


DEAN RADIN

The Sense of Being Stared At: A Preliminary Meta-Analysis

Abstract: Meta-analysis of 60 experiments investigating the conscious sense of being stared at suggests that the reported effects may reflect a genuine ability. A subset of 10 of these studies, designed to preclude implicit learning of sensory cues, resulted in a homogeneous distribution of effect sizes and a weighted mean effect size substantially beyond chance expectation ($p = 5 \times 10^{-17}$).

Two types of experiments have been conducted to investigate the commonly reported ‘sense of being stared at’ — those based on conscious reports and those based on unconscious physiological responses. The first class, described by Sheldrake in this issue, studies the ability of a ‘staree’ to consciously detect being stared at, typically by a ‘starer’ located behind the staree. This paper reports a preliminary meta-analytic examination of these experiments.

The second class of experiments, reported by Schlitz and Braud (1997) and others, investigates whether a starer’s gaze over a closed-circuit video link, under conditions that rigorously exclude sensory cues, can be detected as unconscious fluctuations in a staree’s skin conductance (Braud & Schlitz, 1989; 1991). Schmidt *et al.* (2004) report a meta-analysis of 15 such experiments involving a total of 379 individual testing sessions. That analysis provided significant evidence for a distant staring effect, a homogeneous distribution of effect sizes, and no evidence of a selective reporting problem. Those studies suggest that an unconscious sense of being stared at is a genuine, independently repeatable effect, providing proof-of-principle for the effects reported by Sheldrake.

Method

I excluded from consideration a large-scale staring study conducted at the NEMO Science Centre in Amsterdam and also web-based staring studies, as trials collected in those studies were unsupervised (Sheldrake, this issue). From Sheldrake (1998; 1999; 2000a,b; 2003), and citations therein (e.g., Coover, 1913; Colwell *et al.*, 2000; Lobach & Bierman, 2004; Poortman, 1959; Radin, 2004), I identified 60 relevant experiments reporting a total of 33,357 trials collected under one or more investigator’s supervision. These studies involved three categories of control for implicit learning of sensory cues: tests conducted in close proximity with trial-by-trial feedback, tests conducted in close
proximity without feedback, and tests conducted by looking through windows and without feedback. Of the 60 studies, the majority (88%) fell in the first and third categories.

The raw data in each study was the number of times the staree successfully identified being stared at or not stared at (each success called a hit), and the total number of trials conducted in the study (N). Hits and trials were used to form a per study ‘hit rate’ as \( p_1 = \text{hits}/N \), and then \( p_1 \) was used to form a standard normal variable \( z = (p_1 - p_0)/se \), where \( p_0 = 0.5 \) (meaning an equal \textit{a priori} likelihood of being in a staring or non-staring condition), \( se = \sqrt{p_0 q_0 / N} \) and \( q_0 = 1 - p_0 \). Effect size per trial (per study) was determined as \( e = z / \sqrt{N} \), and a weighted mean effect size assuming a fixed effects model (FEM) was formed with a weighting factor based on the inverse of the squared standard error, \( w = 1/se^2 \), which in this case is equivalent to \( w = 4N \) (Hedges, 1994). Then a weighted mean effect size was formed as \( e = \sum (w \times e) / \sum w \), where the sums are taken over \( N \) studies. To assess the homogeneity of effect sizes, the statistic \( Q \) was determined, where \( Q = \sum (w \times e^2) - \left[ \sum (w \times e) \right]^2 / \sum w \) with \( N-1 \) degrees of freedom. \( Q \) follows the chi-squared distribution.

**Results**

Figure 1 shows the cumulative mean hit rate across the 60 studies, with one standard error bars. It is clear that the mean hit rate stabilizes to just over 54%, where 50% is expected by chance. The FEM weighted mean effect size was significantly above chance, \( e = 0.089 \pm 0.003 \) (mean ± standard error), \( z = 32.5, p = 10^{-23} \), and the distribution of effects was significantly heterogeneous, \( Q = 763.3 \) (58 df), \( p = 10^{-123} \). Because of the heterogeneity, a more conservative random effects model was also determined (REM, Hedges & Vevea, 1998). The

![Figure 1. Cumulative mean hit rate for 60 sense-of-being-stared-at experiments, with one standard error bars.](image_url)
REM weighted mean effect size remained significantly above chance, $\bar{e} = 0.114 \pm 0.010$, $z = 10.9$, $p = 10^{-28}$, and the distribution of effects also remained heterogeneous, $Q = 167.8$ (58 df), $p = 10^{-12}$.

**Discussion**

One possible explanation for the heterogeneity is a selective reporting bias. A common way to visualize whether selective reporting might be a problem is with a ‘funnel plot’ graphing effect size vs. sample size. If such a plot is positively skewed, it indicates that small sample-size studies with negative outcomes were probably not reported. Use of a technique known as ‘trim and fill’ can be applied to this plot to algorithmically identify and fill in the potentially missing studies to make the plot symmetric (Duval & Tweedie, 2000). A new weighted mean effect size can then be formed using the missing studies to form a conservative estimate of the ‘true’ effect size.

The black circles and white diamond in the funnel plot in Figure 2 show the effect size for each of the 60 reported studies. The positive skew in this plot indicates that this database probably has a selective reporting problem. The trim and fill algorithm identified that six studies were required to make the plot symmetric; these are shown as the white circles in Figure 2. The dashed vertical line indicates the FEM weighted mean effect size. Recalculating both the FEM and REM means effects after adding the six estimated studies resulted in slightly smaller but still highly significant mean effect sizes: $\bar{e}$ (FEM) = $0.078 \pm 0.003$, $z = 28.9$, $p = 10^{-184}$ and $\bar{e}$ (REM) = $0.072 \pm 0.010$, $z = 7.15$, $p = 10^{-13}$.

*Figure 2. Funnel plot for 59 sense-of-being-stared-at experiments (black dots) and six studies identified by the ‘trim and fill’ algorithm (white dots). The arrow pointing to the white diamond indicates the effect size measured in a recent replication attempt using a computer-based, automated recording system (Radin, 2004).*
By eliminating 19 studies with the largest Q values, the effect size heterogeneity becomes homogeneous. All of the eliminated studies involved close proximity designs with trial-by-trial feedback. With the remaining homogeneously distributed studies, the FEM and REM mean weighted effect sizes remain significant; the more conservative $\psi$ (REM) = 0.063 ± 0.007, $p = 10^{-19}$.

To examine the possibility that studies with especially large outcomes might have been due to poorer controls for sensory cues, two funnel plots were formed, as shown in Figure 3. The black circles in this figure show 42 studies involving designs with close proximity and trial-by-trial feedback, and the white squares show 10 studies conducted through windows without feedback. It is evident that both the heterogeneity and the selective reporting problem are due to the close-proximity studies. The 10 better-controlled studies are homogeneously distributed and do not show evidence of selective reporting. The FEM weighted mean effect size for the through-the-window studies (which is the appropriate effect size model given the homogeneity of effect sizes) is highly significant ($p = 4.8 \times 10^{-17}$). Comparison of the FEM and REM models is shown in Table 1.

Comparison of the close-proximity vs. the through-the-window studies shows that the latter had significantly lower mean effect sizes in both fixed and random effects models ($z = -4.96, p = 10^{-4}$, two-tailed; $z = -2.00, p = 0.05$, two-tailed, respectively). This suggests that the mean effect size for close-proximity studies might be inflated due to implicit learning of subliminal cues, but that explanation is confounded by the likelihood of selective reporting, as shown by the funnel plot and suggested by the heterogeneity of this class of experiments.

Figure 3. Funnel plot comparing 42 experiments conducted in close proximity and with trial by trial feedback (black circles), and 10 experiments conducted at a distance, looking through a window, and without feedback (white squares).
Table 1. Comparison of fixed and random effects models for sense of being stared at studies conducted with higher controls for implicit learning of sensory cues (staring through windows without feedback) and lower controls (staring at close proximity with feedback).

<table>
<thead>
<tr>
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<th>Through window</th>
<th>Close proximity</th>
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<tr>
<td><strong>Model</strong></td>
<td>FEM REM</td>
<td>FEM REM</td>
</tr>
<tr>
<td><strong>e</strong></td>
<td>0.060</td>
<td>0.064</td>
</tr>
<tr>
<td><strong>se</strong></td>
<td>0.007</td>
<td>0.030</td>
</tr>
<tr>
<td><strong>z</strong></td>
<td>8.31</td>
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</tr>
<tr>
<td><strong>p</strong></td>
<td>$4.8 \times 10^{-1}$</td>
<td>$6.4 \times 10^{-17}$</td>
</tr>
<tr>
<td><strong>Q</strong></td>
<td>10.74</td>
<td>648.75</td>
</tr>
<tr>
<td><strong>df</strong></td>
<td>9</td>
<td>41</td>
</tr>
<tr>
<td><strong>p</strong></td>
<td>0.29</td>
<td>2.4 $\times 10^{-11}$</td>
</tr>
</tbody>
</table>

Table 2. Filedrawer estimates using Rosenthal and Scargle/Hsu methods.

<table>
<thead>
<tr>
<th>Method</th>
<th>Filedrawer estimate</th>
<th>Trials</th>
</tr>
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<tbody>
<tr>
<td>Rosenthal</td>
<td>7,729</td>
<td>3,045,210</td>
</tr>
<tr>
<td>Scargle/Hsu</td>
<td>1,417</td>
<td>800,885</td>
</tr>
</tbody>
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As more of these studies are conducted, new meta-analyses will undoubtedly assess in finer detail design elements such as the types of controls employed, how conditions were randomized, how the tests were supervised, how data were recorded, and so on. Examination of such moderator variables will help explain...
The overall heterogeneity. Based on Schmidt et al.’s (2004) meta-analysis of similar experiments involving rigorous controls against sensory cues, it seems likely that future meta-analyses will continue to show that some proportion of these effects reflect a genuine but poorly understood ‘sense’.

References


Science is a dominant paradigm in Western thought and action. As members of modern society we frequently turn our trust to the experience and purported knowledge of scientists to shape our understanding of the world around us. In this epistemological capacity, scientists are understood to be objective truth-seekers guided by pre-established methods and procedures for demarcating that which is true from that which is false. In a public sphere free from domination, such as that proposed by social philosopher Jurgen Habermas, in his now classic works (1979; 1981), communication should move toward understanding among members of differing but equal social segments. Research in the sociology and anthropology of science, however, reveals the subjective and often arbitrary nature of scientific practice. Scientists are involved in a form of cultural exchange in which preexisting assumptions, belief systems, social relations, and power positionings guide their views of reality. From this vantage point, science with a capital S is better expressed by a relativist set of small s’s that correspond to differing social positions and subcultures. What is truth to one group may be false to another, and vice versa, although some views clearly dominate over others. Social norms influence the knowledge constitutive interests of science (Habermas, 1971) — shaping what is or is not acceptable as reasonable research. All understanding is constrained by borders which structure the communication genre (Bahktin, 1986). Discourses about scientific truth are shaped and reshaped as epistemology and politics become one.

Debates over controversial areas of science are especially useful for deconstructing the ideological nature of truth in Western society. Disputes over the legitimacy of specific research topics, such as the studies of remote staring by Rupert Sheldrake, debated in this special issue of the Journal of Consciousness Studies, provide a useful testing ground for mapping social assumptions about science and the interplay of power and language in scientific discourse. As we consider this specific area of research, we can see Dr. Sheldrake’s work and the ensuing critique within the context of the sceptic–proponent debate in parapsychology. One way to understand this debate is within a communication model that posits ideal speech situations and an open exchange between equals (Habermas, 1971;1979). How and why does communication fail in the context of truth construction in scientific discourse? These are questions we must ask as we complete our reading of the various positions articulated in this volume.

Parapsychology is a fledgling discipline committed to the scientific study of psi phenomena, including extrasensory perception (ESP) and psychokinesis (PK). While parapsychology researchers have adopted scientific methods, standards, and language, the research occupies a marginalized position relative to the scientific establishment (McClenon, 1984; Mauskopf and McVaugh, 1980). It represents a heterodox discipline in comparison to the scientific orthodoxy (Hess, 1993). A discourse has been constructed over the past century in which
scientists have debated the pros and cons of parapsychology research as a legitimate domain of scientific inquiry (e.g., Jastrow, 1900; Hansel, 1980; Honorton, 1985; Hyman, 1985; Alcock et al., 2002).

Proponents and sceptics alike draw on common forms and rhetorical styles to argue the case for and against the existence of psychic phenomena. Nowhere is this more clear than in this special issue, which provides a forum for the debate over the nature and potential reach of consciousness. This forum provides evidence of a shared scientific speech community. At the same time, however, the juxtaposition of opposing sides within the debate reveals fundamental tensions within society — suggestive of what C.P. Snow (1959) referred to as ‘two cultures’. Embedded within the parapsychology debate are oppositional relations and social alignments that have emerged in the struggle to control what is acceptable or unacceptable as legitimate science or valid knowledge. Lines have been drawn between the proponents, who argue that there is evidence to support remote staring, and sceptics, who argue that the purported results are due to artifacts, wishful thinking, or outright fraud on the part of the experimenters. Both proponents and sceptics compete to be the scientific voice of reason when dealing with claims of the paranormal. In constructing a self/other opposition, the two sides portray one another as different as well as morally wrong (Hess, 1993).

The discussion of a common forum, such as we find in this journal, implies an orientation toward reaching understanding among those engaged in the discourse on consciousness and the role that psi phenomena may or may not play. Again following Habermas, we may examine the role of language in the development of consensus between opposing sides within the debate. Habermas (1981, p. 101) states that communicative rationality involves the ‘unifying consensus-building force of argumentative speech, in which different participants overcome their merely subjective views and, owing to the mutuality of rationally motivated conviction, assure themselves of both the unity of the objective world and the intersubjectivity of their lifeworld’. As we look at the nature of this special issue, we see that perhaps reciprocity in communication provides a consensual standard for the resolution of conflict. Or does it?

The formal parapsychology debate, of which Sheldrake’s remote staring research is part, centres on issues of methodology, experimental controls, data analysis, and theory construction. It is at the levels of interpretation and reciprocity related to these issues that aspirations and realities of communication break down. This can be seen in the excerpts from members of the sceptical community, cited by Sheldrake in his lead essay, who have dismissed the data. Ideology and on-going power relations represent serious challenges to the ideal speech situation (White, 1988). Guidelines in scientific practice are frequently vague. Still, there are assumptions about what falls within the boundaries of truth.

At the level of the scientific academy, there are efforts to maintain boundaries between what is acceptable or unacceptable within the confines of scientific practice (Gieryn, 1983). Access to scientific forums requires conformance to standards set by the orthodoxy, which exclude psi phenomena from the realm of possibilities. The opportunity to publish in accepted journals, to participate in
selected conferences, and to join professional societies are the product of on-going social relations based on existing power relations. That this debate is happening in the pages of this journal speaks to the vibrancy that comes when voices of difference reveal the sociological nature of truth construction. In most academic journals, however, those who fail to pass the accepted criteria, even in the face of work that is based on preestablished standards, are frequently excluded from the process of open and effective communication (see McClenon, 1984). Validity claims, so central to Habermas’ agenda, change in the face of new information — often reinforcing preexisting assumptions and power relations. Fairness, impartiality, and equal distribution of opportunities are often lost in the course of scientific practice.

Even in the face of collaborative speech events, such as we find in a meta-critique of this special issue, the democratization of communication frequently fails. It is clear, especially in areas of controversy such as parapsychology, that speakers do not always reach agreement. A failure to communicate is based on assumptions that precede specific speech events. While Habermas (1979) argues that people must take a position relative to specific claims made in the spirit of communication and understanding, there are myriad ways in which communication breaks down. As Ross and Stillinger (1991) have observed, there are both strategic and psychological barriers to dispute resolution. In many instances, the speakers’ are unwilling or unable to accept terms that might lead to agreement. Such limitations may be beyond the participants’ conscious awareness-revealing ideological obstacles to communicative understanding (Bahktin, 1986). This is true, even in the evaluation of ostensibly objective scientific data such as remote staring. This is ironic given that a deeper reading of the discourse finds common forms and rhetorical styles that reveal a shared speech community that could lead to shared meaning and interpretation.

As I offer a critique of the psi research discourse, I include my own view as both a participant and an observer. Over the past 25 years, I have been an active researcher in the field, collecting data on remote staring and other psi related phenomena. I know most of the principal actors within the debate and have been actively involved as an advocate for research in this area. This position provides a vantage point from which to view the style and content of the controversy, as well as some of the personal tensions and frustrations that I have experienced. In this process I am engaged in a form of what Stewart (1991) referred to as ‘contaminated critique’ — exposing the limitations of dichotomies between subjective and objective ways of knowing — as well as the arbitrary nature of categories such as ‘sceptic’ and ‘proponent’.

In my own research on remote staring, I have been engaged directly in the debate over valid knowledge and valid ways of knowing. In two formal experiments, I found statistically significant evidence for a remote staring effect using close circuit television, objective measures of autonomic nervous system activity, and the use of a randomized, double blind protocol (Schlitz and Laberge, 1994). My colleague, Richard Wiseman (a card carrying member of the sceptical community) conducted similar experiments and found chance results in his
laboratory (Wiseman and Smith, 1994; Wiseman et al., 1995). Based on these differences under similar experimental protocols, we decided to conduct a joint study. We carried out two identical experiments making use of the same equipment, same randomization, same subject population. The only difference was that Wiseman or I did the staring. In these experiments, we each replicated our initial findings (Wiseman and Schlitz, 1997). The unusual effect was further replicated in a second collaborative study, in which we again replicated our original findings (Wiseman and Schlitz, 1998). A third collaboration, which pushed both investigators far beyond our boredom threshold, found no overall significance to support the remote staring hypothesis (Schlitz et al., 2005). How do we walk away from these confusing results? Can we reach a firm conclusion from our studies about the existence of remote staring or the nature of the experimenter effect which seems to stand in the face of objective truth claims made within the paradigm of randomized, double blind conditions? Certainty is obscured by the equivocal results. And yet, both Wiseman and I have confirmation for our original positions — I found significance in 4 out of 5 studies, while he found it in none. Despite this, our collaboration speaks to the value of cooperation between alternative positions and between holders of different truth claims. Our collaboration speaks to a playing field in which open discourse is possible, even in the face of competing data. Good will, mutual respect, and an open minded search for truth can be addressed when we move beyond rhetorical posturing that polarizes more than it informs.

By positing an ideal speech situation, we increase our awareness of the specific factors that constitute direct and indirect manipulations of consciousness (White, 1988). This vantage point links structural constraints within the scientific establishment with everyday dialogues between scientists of differing orientations. I believe we really have more in common than we might think. More opportunities to find a middle ground may reveal important insights into the epistemological and ontological implications of psi phenomena for understanding the nature of truth in western culture.

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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 widespread 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 where 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.

References

MAX VELMANS

Are We Out of Our Minds?

We experience physical objects as being out in the world, not in our heads or brains. Although we might accept that there are neural causes, neural correlates and neural representations of those perceived objects in our brains, we do not experience them as being in our brains. This is deeply puzzling, and has been a source of debate for philosophers and scientists for around 2,500 years. As William James (1904) put it ‘… the whole philosophy of perception from Democritus’s time downwards has been just one long wrangle over the paradox that what is evidently one reality should be in two places at once, both in outer space and in a person’s mind’.

At first glance, one might not notice what the fuss is about, given that a common, naïve realist way of dealing with this paradox is to assume that what we see out in space is the object itself and that we have an additional, veridical experience of that object in our brains. Why is this form of realism naïve?

Firstly, because science tells us that the perceived colour, shape, location in phenomenal space and other visual features of an object are just surface representations of what the object is like, constructed by our visual systems. A microscope is all that is needed to convince one that these surface appearances are not all there is to an object. These surface appearances are also very different to the descriptions of the deeper structure of those objects and the space in which they are embedded given by physics, for example by relativity theory and quantum mechanics. So, although we normally think of the perceived object as the ‘physical object’ it is nevertheless how that object looks to us, and not how it is in itself. Similarly, although we normally think of the 3D phenomenal space in which the perceived object is embedded as ‘physical space’, it too is how space looks to us rather than space itself. Note that it follows from this that while perceived objects are in one sense ‘physical’ they are in another sense ‘psychological’ (they are appearances constructed by our visual systems).

Secondly, we don’t have any experience of the object in our brains in addition to the object as perceived out in the world. The perceived objects are what we experience — and in terms of their phenomenology, an object as perceived and our experience of the object are one and the same. When looking at this print, for example, the print that one sees out here on the page is the only ‘print experience’ that one has. So naïve realism is wrong in two ways — it is neither consistent with third-person science, nor first-person experience.

How then are we to make sense of the fact that objects seem to be ‘out there’ while our brains and what they contain are, so to speak, ‘over here’? As Sheldrake (Part 2) points out in his fascinating review of ancient and modern thinking on this subject, theories of vision have ranged from ‘extramissive’ theories that posit some active influence emanating from the eyes that both illuminates and influences the external world, ‘intramissive’ theories that stress the

[10] I give a full analysis of how phenomenal features and theory-driven physical descriptions relate to each other and to the thing itself in Velmans (2000) ch7.
influence of the external world on the (passive) brain, and theories in which intramissive and extramissive influences combine. As Sheldrake notes, up to the twelfth century, extramissive theories were dominant, but with an increasing understanding of the way light reflected from an object is focused on the retina by the lens of the eye, intramissive theories have become dominant. While Kepler’s theory of vision stopped at the retina, modern investigations of vision have extended this understanding considerably into what is going on in the depths of the brain, in terms of visual feature analysis, the way different regions of the brain combine together to form representations of the object, the areas of the brain involved and so on. This ongoing research programme is highly productive and we have good reasons to hope that, in time, it will yield a good understanding of what it investigates: the neural causal antecedents of visual experiences (the necessary and sufficient preceding neural conditions) and the NCC — the neural correlates of given conscious experiences.

But that isn’t the full story of perception. What about the experiences themselves? It is well accepted that one cannot directly observe another’s first-person experience by a third-person investigation of their brain, and it goes without saying that the antecedent causes of experiences are not the experiences themselves (causes should not be confused with their effects). Nor should we confuse experiences with their neural correlates. Even if we found that each unique experience is accompanied in 1:1 fashion by a unique set of neural correlates, we could not conclude that those first person experiences were nothing more than those correlates. The relationship would of course be intimate, but the apparent differences in third-person neural features and first-person phenomenal features would remain. One might for example know all there was to know (from a third-person perspective) about the neural features without knowing anything about the accompanying phenomenal features, unless one had already established this neural/phenomenal correspondence by combining neural investigations with first person phenomenological reports. If so, an exclusively third-person account of perception cannot be complete.11 For completeness, one has to add a veridical description of first-person perceptual effects.

This was the motivation for the reflexive model of perception that I introduced in Velmans (1990) and developed into a more general Reflexive Monist account of how consciousness relates to the brain and the physical world in Velmans (2000)— see Figure 1 below.

This (highly schematised) model starts in the conventional way with a third-person account of the stimulus, in this case, a cat as perceived by an external observer. Light rays reflected from the cat’s surface activate the subject’s visual

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11 To get over this problem, physicalist and functionalist theories of consciousness have tried to argue that, in spite of their apparent differences, experiences are nothing more than (are ontologically identical to) their neural causes and/or physical or functional correlates. However, the formal differences between causation, correlation, and ontological identity in terms of symmetry and Leibniz’s law blocks this reduction. Given that third person science is restricted to the discovery of neural causes and correlates, this block cannot be removed by any conceivable third-person investigation of the brain, forming a major stumbling block for third-person reductionism that is seldom addressed, and, to my knowledge, has never been overcome (see full discussion in Velmans, 1998a, 2000 ch3).
system, producing neural representations in the occipital and associated regions of her brain. If the subject experiences the cat, the neural causes and correlates of that experience also form in her brain. That is where the (intromissive) third-person story ends. The reflexive model then adds what the subject actually experiences to the model, namely a phenomenal cat out in the world. Central to the model is the recognition that an entity in the external world is (reflexively) experienced by the subject to be a phenomenal object in the external world — not in the brain, as reductionism would have it, and not ‘in the soul’ as the ancients supposed — in the same way that this print is seen to be out here on this page. In short, when attending to the cat, the subject experiences more or less what the external observer experiences when he looks at the cat. The subject just sees it from a different angle.

The same of course applies to other phenomenal objects and events that, together with the perceived space in which they are embedded, make up the external phenomenal world. Where are these phenomenal objects and events? Out in the phenomenal world. The phenomenal cat in Figure 1 is out in the phenomenal world, a pain in the foot is in the experienced foot, and this perceived print on this visible page really is out here on this page. Technically, this is a form of phenomenological externalism,\(^{12}\) which immediately presents us with the problem of perceptual projection: given that the proximal neural causes and

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\(^{12}\) To relate the reflexive model to Sheldrake’s analysis, I will focus on phenomena that have such apparent external location and extension, but the reflexive model is not externalist (for any doctrinal reason) about all experiences. Whether an experience is located in external phenomenal space, on the body surface, inside the head, or nowhere, is an empirical matter that is entirely dependent on its phenomenology. For example, the phonemic imagery that accompanies the thought that 2+2=4 does not have a clear location, or might seem, at best, to be roughly located inside the head (see Velmans, 2000, ch. 6).
correlates of what we experience are in the head or brain, how can we explain the fact that various sensations and experienced objects seem to be beyond the brain?

I: Perceptual Projection

As Sheldrake notes, this is an ancient problem, and one ancient solution was that some extromissive physical influence emanates from the eyes to light up or otherwise influence the world. Given the evidence from staring experiments in his target paper (Part 1), this is a view that Sheldrake himself adopts in his Part 2. In the reflexive model, however, ‘perceptual projection’ simply refers to an empirically observable effect, for example, to the fact that this print seems to be out here on this page and not in your brain. In short, perceptual projection is an effect that requires explanation; perceptual projection is not itself an explanation. We know that preconscious processes within the brain produce consciously experienced events that may be subjectively located and extended in the phenomenal space beyond the brain, but we don’t really know how this is done. We also know that this effect is subjective, psychological and viewable only from a first-person perspective. In the reflexive model, nothing physical is assumed to project from the brain.\(^\text{13}\) This raises a vexing question: some experiences might seem to be beyond the brain, but if they are not physically ‘projected’, are such experiences really where they seem?

II: How Phenomenal Space Relates to Real Space

No one doubts that physical bodies can have real extension and location in space. Dualists and reductionists nevertheless find it hard to accept that experiences can have a real, as opposed to a ‘seeming’ location and extension. They do not doubt, for example, that a physical foot has a real location and extension in space, but, for them, a pain in the foot can’t really be in the foot, as they are committed to the view that it is either nowhere or in the brain. For them, location in phenomenal space is not location in real space. According to the reflexive model however, this ignores the fact that, in everyday life, we take the phenomenal world to be the physical world. It also ignores the pivotal role of phenomenal space in forming our very understanding of space, and with it, our understanding of location and extension in measured or ‘real’ space.

What we normally think of as the ‘physical foot’ for example is actually the phenomenal foot (the foot as seen, felt and so on). That does not stop us from pointing to it, measuring its location and extension and so on. If so, at least some phenomenal objects can be measured. While a pain in the foot might not be measurable with the same precision, few would doubt that we could specify its rough location and extension (and differentiate it for example from a pain in the back). \(^{[13]}\)

\[^{[13]}\] Although we don’t have a full understanding of how perceptual projection works, there is a large experimental literature about the information that is used by the brain to model distance and location. There are also many ways to demonstrate perceptual projection in action, for example in hallucinations, phantom limbs, stereoscopic pictures, holograms, and virtual realities. I have discussed this literature elsewhere, along with some potentially useful models to explain it (holography and virtual reality) in Velmans (1990; 1993; 1998b; 2000).
What we normally think of as ‘space’ also refers, at least in the initial instance, to the phenomenal space that we experience through which we appear to move. Our intuitive understanding of spatial location and extension, for example, derives in the first instance from the way objects and events appear to be arranged relative to each other in phenomenal space (closer, further, behind, in front, left, right, bigger, smaller and so on). We are also accustomed to making size and distance estimates based on such appearances. This print for example appears to be out here in front of my face, and THIS PRINT appears to be bigger than this print. However, we recognise that these ordinal judgments are only rough and ready ones, so when we wish to establish ‘real’ location, distance, size or some other spatial attribute, we usually resort to some form of measurement that quantifies the dimensions of interest using an arbitrary but agreed metric (feet, metres etc.), relative to some agreed frame of reference (for example a Cartesian frame of reference with an agreed zero point from which measurement begins). The correspondence, or lack of correspondence, between phenomenal space and measured space is assessed in the same way, by comparing distance judgments with distance measurements in psychology experiments. For example, I can estimate the distance of this phenomenal print from my nose, but I can also place one end of a measuring tape on the tip of my nose (point zero) and the other end on this print to determine its real distance.

Such comparisons allow one to give a broad specification of how well phenomenal space corresponds to or maps onto measured space. There are of course alternative representations of space suggested by physics (four-dimensional space–time, the 11 dimensional space of string theory, etc.) and non-Cartesian geometries (e.g. Riemann geometry). However, a comparison of phenomenal to measured (Cartesian) space is all that we need to decide whether a pain in my foot or this perceived print on this page is, or is not, really in my brain. According to the reflexive model, phenomenal space provides a natural representation, shaped by evolution, of the distance and location of objects viewed from the perspective of the embodied observer, which models real distance and location quite well at close distances, where accuracy is important for effective interaction with the world. My estimate that this page is about 0.5 metres from my nose, for example, is not far off. However, phenomenal appearances and our consequent distance judgments quickly lose accuracy as distances increase. For example, the dome of the night sky provides the outer boundary of the phenomenal world, but gives a completely misleading representation of distances in stellar space.

Note that, although we can use measuring instruments to correct unaided judgments of apparent distance, size and so on, measuring tapes and related instruments themselves appear to us as phenomenal objects, and measurement operations appear to us as operations that we are carrying out on phenomenal objects in phenomenal space. In short, even our understanding of ‘real’ or measured location is underpinned by our experience of phenomenal location. And crucially, whether I make distance judgments about this perceived print and judge it to be around 0.5 metres in front of my face, or measure it to find that it is only 0.42 metres, does not alter the phenomenon that I am judging or measuring.
The distance of the print that I am judging or measuring is the distance of this perceived print out here on this visible page, not some *other* ‘physical print’ in some *other* ‘physical space’, and not some other ‘experience of print’ in my brain.

III: Paradigm Clash

In recent years the spatially extended nature of much of conscious experience (in the form of an experienced phenomenal world) has been increasingly accepted in the psychological and brain sciences, and its implications for an understanding of consciousness has once more become a topical issue. For example, Karl Pribram (1971; 2004), one of the first scientists to address this problem, has continued to develop his earlier theories of holographic representation as an explanatory model of how consciousness relates to brain; Antti Revonsuo (1995) developed the suggestion that the phenomenal world is a form of virtual reality (see also Velmans, 1990; 1993; 1998b); and Steven Lehar (2003) has attempted to develop a mathematical model of how objects *appear* as they move in phenomenal space (as opposed to how they really *are* as they move in phenomenal space). As these, and other scientists (such as Jeffrey Gray, 2004) have pointed out, the 3D nature of the phenomenal world is likely to have important consequences for neuroscience, for the obvious reason that the brain has to be organised in a way that supports such spatially extended experiences.

These theorists nevertheless remain divided on the issue of whether some of these experiences are outside the brain. Pribram (2004) takes the view that they are, and develops a broad theory of perception that he explicitly links to the reflexive monism developed in Velmans (2000). By contrast, Revonsuo, Lehar and Gray adopt an intromissive form of biological naturalism, arguing that the entire 3D phenomenal world, stretching to the horizon and the dome of the sky, is a form of virtual reality that is literally located inside the brain. For them, this ultimately reduces the problems of consciousness to problems of neurophysiology — a reduction that Lehar claims to be more ‘scientific’ than the notion of perceptual projection. However, Lehar (2003) also points out that biological naturalism forces one into a surprising conclusion: if the phenomenal world is inside the brain, the real skull must be outside the phenomenal world (the former and the latter are logically equivalent).

Let me be clear: if one accepts that

(a) The phenomenal world appears to have spatial extension to the perceived horizon and dome of the sky.

(b) The phenomenal world is literally inside the brain.

It follows that

(c) The real skull (as opposed to the phenomenal skull) is beyond the perceived horizon and dome of the sky.

Although Lehar accepts this conclusion, he admits that this consequence of biological naturalism is ‘incredible’. I agree, and, in my view, this casts an entirely different light on the so-called ‘scientific’ status of biological naturalism and the
so-called ‘unscientific’ status of perceptual projection (in the reflexive model). Decide for yourself. Put your hands on your head. Is that the real skull that you feel, located more or less where it seems to be? If that makes sense, the reflexive model makes sense. Or is that just a phenomenal skull inside your brain, with your real skull beyond the dome of the sky? If the latter seems absurd, biological naturalism is absurd.

Note that the differences between reflexive monism (RM) and biological naturalism (BN) largely hinge on how they conceive the nature of the ‘brain’. RM adopts critical realism — the conventional view that, although our experiences do not give us a full representation of how things really are, they normally provide useful approximations. As a first approximation, ‘brains’ are what one finds inside the skulls that we feel sitting on the top of our necks, that one can find pictures of in neurophysiological textbooks, and that are occasionally to be seen pickled in jars. Although I accept that these ‘brains’ are really phenomenal brains, these mental models are roughly accurate. Consequently, the location and extension of the phenomenal and real brain closely correspond.

Lehar also accepts that phenomenal brains are mental models of real ones, but BN forces him to claim that the real skull is beyond the perceived dome of the sky. If so, our assumption that the real brain is more or less where it seems to be (inside the perceived skull) must be a delusion. The alternative is that biological naturalism is wrong. Not only is the notion of a skull beyond the perceived universe unfalsifiable (it would always be beyond any phenomena that one could actually perceive), but it is also hard to know in what sense something that surrounds the perceived universe could be a ‘skull’ (it certainly isn’t the skull that we can feel on top of our necks). Nor is it easy to grasp in what sense something that contains the perceived universe is a ‘brain’ (it certainly isn’t the brain that one can perceive inside the skulls on top of our necks).

IV: Paradigm Convergence: Are We Out of Our Minds?

There may however be a different way of understanding some of the implications of BN and RM that allows for a measure of convergence — with some interesting links to Sheldrake’s theories. BN and RM agree for example that the entire phenomenal world is part of conscious experience, and that it makes sense to suggest that there is a ‘vehicle’ or ‘ground’ of some kind that carries or contains it. As Sheldrake notes in his second paper, projective theories accept that experienced objects are in the mind, although not inside the brain. But what kind of ‘mind’ could contain a 3D phenomenal world? While RM adopts the conventional view that the immediate carriers of conscious experience are unconscious and preconscious mental processes within the human brain, it also accepts that the brain does not operate in isolation. Rather,

human consciousness is embedded in and supported by the greater universe (just as the tip of the iceberg is supported by the base and the surrounding sea). The contents of human consciousness are also a natural expression or manifestation of the embedding universe. In humans, the proximal causes of consciousness are to be found in the human brain, but it is a mistake to think of the brain as an isolated
system. Its existence as a material system depends totally on its supporting sur-
round, and the contents of consciousness that it, in turn, supports arise from a reflex-
ive interaction of perceptual processing with entities, events and processes in the
surrounding world, body and the mind/brain itself (Velmans, 2000, p. 229).

In a broader sense, then, the universe itself both contains the objects and events
within it and our conscious experiences of those objects and events — and in this
special sense the universe itself might be thought of as the supporting vehicle,
ground, or ‘mind’.

How does this relate to findings from staring experiments? RM provides a way
to unify a range of phenomena (the 3D phenomenal world, hallucinations, virtual
realities, etc.), and their consequences for understanding consciousness, but
Sheldrake is right to note that it makes no predictions about the extromissive
influences that staring experiments suggest. However, if these influences do
depend on unconscious and preconscious processes in the mind/brain interacting
with the surrounding world. So if there are connections between human individ-
uals of the kind suggested by staring experiments, it is in the unconscious pro-
cesses that support conscious experience rather than in the phenomenology of
experience itself that one should look. If bi-directional information flows oper-
ate unconsciously, that would explain the finding that skin resistance measures
indicate stare detection more sensitively than subjects’ verbal reports — and
it would also make sense of the fact that one might ‘feel’ another’s focused
attention without having any knowledge of how that feeling comes about. Bi-
directional information flows of this kind could then be thought of as properties
of an extended, unconscious mind.

Time will tell. But let us suppose that such unconscious connections between
individuals are found to exist. Would that mean we are out of our minds? Not
really. If such interconnections were found, we would be in our minds — but
partly out of our brains.

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