

## CORRESPONDENCE

To the Editor,

The 'Psychic Pet' Phenomenon

In the January issue of the *Journal* Richard Wiseman, Matthew Smith and Julie Milton published a reply to my note (Sheldrake, 1999a) about their claim to have refuted the 'psychic pet' phenomenon. This claim was made in the *British Journal of Psychology* (Wiseman, Smith & Milton, 1998) and widely publicized in the media. It was repeated as recently as 2 February this year in a presentation given by the first author at the Royal Institution entitled "Investigating the Paranormal".

At my invitation, Wiseman and Smith carried out 4 videotaped experiments with a dog called Jaytee, with whom I have carried out more than 100 videotaped experiments (Sheldrake, 1999b). My experiments showed that Jaytee usually waited by the window for a far higher proportion of the time when his owner was coming home than when she was not. This occurred even when his owner, Pam Smart, came at non-routine, randomly-selected times and travelled by unfamiliar vehicles such as taxis. This pattern was already clearly apparent months before Wiseman et al. carried out their tests.

In the 3 experiments that Wiseman and Smith carried out at Pam's parents' flat, the pattern of results was very similar to my own. Their data show a large and statistically significant effect: Jaytee spent a far higher proportion of time at the window when Pam was on the way home than when she was not (Sheldrake, 1999a).

The difference between our interpretations of these experiments arose because Wiseman et al. had a different agenda from mine: I was engaged in a long-term study of this dog's anticipatory behaviour, whereas they seemed more interested in trying to debunk a 'claim of the paranormal'. They themselves defined a 'claim' for Jaytee's 'signal' and then judged it by disregarding most of their own data. They argue that since they specified their criterion in advance (or rather criteria, since they changed the criterion as they went along), the agreement of their pattern of results with mine is irrelevant. "Testing this claim did not require plotting our data and looking for a pattern" (Wiseman, Smith & Milton, 2000, p.46). Although they refused to look at the pattern shown by their data, I plotted their data for them, together with plots of my data showing the same obvious pattern. I gave them these graphs before they submitted their paper to the *British Journal of Psychology* in 1996. Both in their paper and in their sceptical claims in the media, they chose to ignore what their own data showed.

"We tried the best we could to capture this ability and we didn't find any evidence to support it," Smith was quoted as saying in an article in the *Daily Telegraph* entitled "Psychic pets are exposed as a myth" (Irwin, 1998). "A lot of people think their pet might have psychic abilities but when we put it to the test, what's going on is normal not paranormal," Wiseman asserted in the press release accompanying their paper. These are examples of the comments they now describe as "responsible and accurate."

Wiseman, Smith and Milton try to justify ignoring the pattern shown by their data on the grounds that it was "post hoc". I cannot accept this argument. First, I had been plotting data on graphs right from the beginning of my research with Jaytee. Second, their rejection of *post hoc* analysis would deny the validity of any independent evaluation of any published data. The whole point of publishing data in scientific journals is to enable other people to examine and analyse them. Of necessity, the critical analysis of published data in any field of research can only be *post hoc*. And third, the plotting of graphs is not normally regarded as a controversial procedure in science. Consequently I cannot agree with them that my representation of their results in my book (Sheldrake, 1999b, Figure 2.5) is "misleading".

In their recent note, they raise two scientific, as opposed to legalistic, points. First, they suggest that Jaytee may simply have gone to the window more and more the longer Pam was out, and hence been there most when she was on the way home. But a comparison of Jaytee's behaviour during Pam's short, medium and long absences shows that this was not the case (Sheldrake, 1999b). Moreover, in control experiments in which Pam did not come home, Jaytee did not go to the window more and more as time went on (Sheldrake, 1999b, Figure B.2).

Second, they say that my experiments "appear to contain design problems (Blackmore, 1999)". Susan Blackmore's comments were made in 'an article in the *Times Higher Education Supplement*, which concluded: "There are better ways to spend precious research time than chasing after something that lots of people want to be true, but almost certainly is not." She thought she had spotted "design problems" in my experiments with Jaytee (Sheldrake, 1999b) because "Pam was never away for less than an hour". (In Wiseman, Smith & Milton's experiments Pam was likewise never away for less than an hour.)

This is why Blackmore thought there was a problem: "Sheldrake did 12 experiments in which he bleeped Pam at random times to tell her to return. . . . When Pam first leaves, Jaytee settles down and does not bother to go to the window. The longer she is away, the more often he goes to look . . . yet the comparison is made with the early period when the dog rarely gets up." But anybody who looks at the actual data (Sheldrake, 1999b, Figure B.4) can see for themselves that this is not true. In 5 out of the 12 experiments, Jaytee did not settle down immediately she left. In fact he went to the window more in the first hour than during the rest of Pam's absence, right up until she was on the way home, or just about to leave.

In the light of Blackmore's comments, I have reanalysed the data from all 12 experiments excluding the first hour. The percentage of time that Jaytee spent by the window in the main period of Pam's absence was actually lower when the first hour was excluded (3.1%) than when it was included (3.7%). By contrast, Jaytee was at the window 55.2% of the time when she was on the way home. Taking Blackmore's objection into account strengthens rather than weakens the evidence for Jaytee knowing when his owner was coming home, and increases the statistical significance of the comparison. (Including the first 60 minutes of Pam's absence in the analysis, by the paired-sample *t*-test,  $t = -5.72$ ,  $p = 0.0001$ ; excluding the first 60 minutes,  $t = -5.99$ ,  $p < 0.0001$ .)

Blackmore's claim illustrates once again the need to treat what sceptics say with scepticism.

In conclusion, I agree with Wiseman, Smith and Milton (2000, p.49) that my analysis of their data "would not provide compelling evidence of psi ability unless it were supported by a larger body of research." It is in fact supported by a large body of research, summarized in my book (Sheldrake, 1999b) and soon to be published in detail in a peer-reviewed journal.

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