

## Assessing the Roles of Sender and Experimenter in Dream ESP Research

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### Abstract

*This study explored the role of the sender in a dream ESP task by considering the effects of presence of a sender (sender, no sender) and the receiver's expectancy that a sender was present. Forty participants each completed a sender and a no sender trial on consecutive nights by keeping a dream diary of all mentation they could recall when they awoke. The order of trials was randomised across participants. On no-sender nights a randomly selected video clip was played repeatedly from 2:00 until 6:30am; on sender nights a sender would also watch the clip between 6:00 and 6:30am and attempt to communicate its content. Both sender and no sender conditions produced above chance hit rates (30% and 35% respectively), but  $z$  scores for similarity ratings did not deviate significantly from chance (sender night:  $t_{(39)} = 0.92$ ,  $p = .18$ ; no sender night:  $t_{(39)} = 1.11$ ,  $p = .14$ , one-tailed). There was no difference in performance in terms of sender conditions ( $z = -0.22$ ,  $p = .41$ , one-tailed) or sender expectancy ( $z = -0.18$ ,  $p = .46$ , one-tailed), failing to support the proposal that senders play an active role in dream ESP success. Possible improvements in the manipulation of participant expectancy are discussed.*

## Introduction

J. B. Rhine's advice to those who hoped to study *psi* in the laboratory involved an analogy with making rabbit stew; "If you want to have rabbit stew, first catch the rabbit" (Stanford, 1993, p. 129).<sup>1</sup> Thus if we are to study the action of *psi* in the laboratory we need to ensure that all aspects of the laboratory situation are arranged so as to facilitate (or at least not inhibit) its occurrence. *Psi* phenomena are not renowned for their experimental reliability (e.g., Beloff, 1983, Milton and Wiseman, 1999, see Shapin and Coly, 1985 for an extended discussion), and all too often we have been left to feed on scraps. It might be naïve to expect replication on demand given the effect sizes typically involved (cf. Utts, 1991), but nevertheless there must be a suspicion that *psi* is sensitive to some factors that have not been adequately explored or typically are not controlled for effectively. If different laboratories differ in these subtle respects it could lead superficially similar experiments to generate different outcomes, as some enjoy rabbit stew while others settle for vegetable broth. Efforts to identify potential confounding factors promise to inform us of the necessary conditions to capture *psi* more consistently as well as perhaps offering some insight into its *modus operandi*.

In looking to map these necessary conditions we have recently been especially concerned to consider the sender-receiver-experimenter dynamic as a factor (or collection of factors) that moderates *psi* performance in Ganzfeld ESP trials (Roe, Sherwood and Holt, 2004; Sherwood, Roe, Holt and Wilson, 2005). In the first of these studies (Roe et al., 2004) we attempted to distinguish between the active contribution a sender might make and the positive effects of simply believing that a friend was viewing the target. The direct hit rate was exactly at chance (25%) and, although this was slightly better for sender trials than no sender trials (26.1% versus 23.5%) and better for trials on which the receiver believed there was a sender than when they believed there was not, irrespective of whether there actually was one (33.3% versus 18.2%), there were no significant differences between conditions based on *z* scores of target ratings (for sender status  $p = .632$ ; for sender expectancy  $p = .765$ ). This was disappointing given that of seven previ-

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<sup>1</sup>After presenting a version of this paper at the International Conference of the Society for Psychical Research in September 2006, Sean O'Donnell questioned Rhine's choice of analogy here, which seems to suggest that *psi* is something to be slain and dismembered by its investigators. We have some sympathy with his suggestion that a better analogy would be of *psi* as a butterfly, delicately to be caught and closely observed in its natural, intact and healthy state.

ous Ganzfeld studies that directly compared sender and no-sender conditions within the same study, all reported better performance with a sender, significantly so for two studies (see Roe et al., 2004 for a more thorough review). We therefore intended to reconsider this aspect in the current study. In a subsequent study we explored whether participants' perceptions of the experimenter and of the experimenter's attempts to generate a warm social ambiance were predictive of performance (Sherwood et al., 2005). Here both participants and the experimenter completed an interaction questionnaire that asked about their mood, expectations of success and sense of rapport with the other participants. Responses on the interaction questionnaire suggested that participants were typically in a good mood, fairly relaxed, optimistic about the trial — though not confident of success — and had a positive perception of the experimenter, all of which were expected to be psi conducive features. Nevertheless, the direct hit rate for this study was nonsignificantly worse than chance (21.1%,  $z = -.015$ ). However, although relationships between these variables and trial outcomes (in terms of  $z$  scores for similarity ratings) did not exhibit a clear pattern and tended to vary somewhat from experimenter to experimenter, they did offer some significant overall relationships, such as with receiver mood (negative-positive;  $r_s = -.335$ ), sender optimism ( $r_s = .432$ ) and confidence of success ( $r_s = .398$ ) which we felt warranted further work.

We planned to follow up these findings by conducting a further study that would investigate the effects of both sender and experimenter upon the receiver's ability to identify a target video clip based on correspondences with their own mentation. However, using the Ganzfeld procedure as a method of eliciting psi has proved to be very labour intensive, and may have deterred some participants from volunteering because of the time commitment required and the coordination necessary when involving a sender and receiver. In looking for an alternative method we were encouraged to reconsider dream ESP as a paradigm, since the original Maimonides research programme was notably successful (Child, 1985; Ullman and Krippner, with Vaughan, 1973), and there is evidence of above-chance scoring among subsequent replication attempts with experimental designs using a simplified method that did not require REM monitoring or access to sleep laboratories (e.g., Dalton, Steinkamp and Sherwood, 1999; see Sherwood and Roe, 2003, for a review).

In considering previous dream ESP research we can derive some

encouragement for suggesting that sender and experimenter effects might be evident here too. For example, Ullman et al. (1973), in reflecting on the performance of participants in the Maimonides dream ESP series, commented that “[T]he active involvement of the agent [sender] is an important ingredient for success.” (p. 212). The majority of Maimonides studies investigated telepathy rather than clairvoyance, which might be interpreted as a tacit assumption that a sender can facilitate psi hitting in a dream ESP task. However, this presumption was not supported in Sherwood and Roe’s (2003) summary analysis, which suggested that overall the clairvoyance studies in this series had been more successful than those intended to study telepathy. Of course, making comparisons across studies may be misleading, since they could have differed in other ways besides whether or not they involved a sender, and we expected that a direct comparison within a single study should clarify this relationship.

There is also some suggestion in the database of post-Maimonides dream ESP studies that some groups of researchers have been markedly more successful than others (see Sherwood and Roe, 2003, pp. 102–104). Given the diversity of approaches used in these studies it is difficult to attribute these differences to one particular cause, but it may be worthwhile to consider whether differences in experimenter-participant interaction have some effect.

### *Planned analyses*

The main planned analyses are to consider the following hypotheses:

1. Participant performance, in terms of  $z$  score of target rating, will be higher than chance expectation for each experimental night
2. Participant performance in the actual sender condition will be higher than that in the no sender condition
3. Participant performance in trials for which the participant believed there was a sender will be higher than for trials for which the participant believed there was not a sender

We are grateful to an anonymous referee for suggesting that we consider differences in success between the two senders and we include this *post-hoc* analysis here. Following Sherwood et al. (2005), we also

planned to conduct exploratory analyses considering covariation of performance (using  $z$ -scores of target ratings) with experimenter interaction measures.

## Design

This study employed a repeated measures design to assess the role of the sender in dream ESP research, with each participant completing a sender and a no sender trial night. Participants remained blind as to which night was which but were asked to nominate on which night they felt there was a sender so as to allow us to consider expectancy effects. Potential psychological experimenter effects were assessed by correlating participant and experimenter ratings of their interaction against task performance. In all cases the dependent variable was pre-specified to be the  $z$  score of target clip ratings.

## Method

### *Participants*

Forty participants were recruited from a variety of sources including the undergraduate population, appeals to the media and an established research database. The sample consisted of 30 females and 10 males<sup>2</sup> (Mean age = 32.08; range = 19–62; mean score on belief measure = 80.32, range = 19–119, absolute range of scale = 19–133, midpoint = 76), of whom 21 had practised a mental discipline (e.g., meditation/relaxation techniques) at some point and 15 had practised a physical discipline (e.g., Yoga/martial arts). Participants were not selected on the basis of their gender or age; neither were they screened for prior experience or for ability to recall dreams. For trials 1–26, Louie Savva (LS) acted as experimenter; for trials 27–40 Louise Farrell (LF) acted as experimenter.

### *Apparatus*

This study used an automated program for selecting and playing video clips that was developed by Dr Paul Stevens and written in Microsoft Visual Basic.<sup>3</sup> Video clips are stored digitally as MPEG files, la-

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<sup>2</sup>One anonymous reviewer speculated that the gender bias toward female participants could have been problematic given that there may be gender differences at this task. *Post hoc* analysis revealed that males performed slightly better than females overall (mean  $z = .40$  and  $.06$  respectively) but this difference was not significant overall (Mann-Whitney  $z = -.93$ ,  $p = .36$ ) or for either night separately (Mann-Whitney  $z = -1.66$ ,  $p = .10$ ;  $z = -.41$ ,  $p = .70$ , two-tailed).

<sup>3</sup>For a more complete description of the program, see Roe, Sherwood, Luke and Farrell (2002).

belled 1a, 1b, 1c etc. The target set consists of 116 minute-long digital video clips arranged in 29 sets of 4. These were the same clips used in our previous Ganzfeld studies (Roe, et al., 2004; Sherwood et al., 2005) and have mainly been produced at the University of Northampton and are drawn from popular television programmes and commercial films, although some have been taken from the pool previously used at Edinburgh. Copies of the target pool are available on DVD from the first author upon request. Randomisation is achieved using the Visual Basic pseudo-random algorithm (rnd), having seeded it using the timer at the start of the program (RANDOMIZE TIMER). Once the "Start" button has been pressed, the computer first selects a target set, then selects one of the 4 clips within that set. The order of presentation of the four clips at judging is similarly randomised. The target sets were selected with replacement. All trials were run at the sender's home (CR or SS) using standard desktop PCs and could be set to play through the night.

### *Materials*

The *Participant Information Form* (PIF) is a 56-item measure that was constructed for general use with parapsychological research at the University of Northampton and was based partially on a version used previously at the University of Edinburgh. It includes questions concerning biographical and contact details (11-items); religious and parapsychological background (5 items); computer experience (2 items); practice of mental/physical disciplines (2 items); belief in luck (2 items); clumsiness and punctuality (2 items); competitiveness (1 item); absorption (2 items); sleep and dreams (4 items); imagination and fantasy-proneness (3 items); creativity (2 items); and physical and mental health (1 item). The remaining items relate specifically to knowledge, belief and experience of anomalous phenomena including telepathy, clairvoyance, precognition, psychokinesis, 'communication with the dead' and out of body experiences (19 items). The form concludes with questions about hypnagogic/hypnopompic experiences in a range of modalities (10 items) and an open question inviting descriptions of personal anomalous sleep-related experiences. Copies of all in-house measures are available from the first author on request.

Participants also completed the short extraversion and neuroticism subscales of the EPQ-R<sup>4</sup> (Eysenck, Eysenck, and Barrett, 1985). Each subscale has 24 items with a dichotomous yes/no response format. A

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<sup>4</sup>An analysis of these personality variables will not form part of this paper

belief in paranormal measure, adapted from Thalbourne and Delin's (1993) 18-item *Australian Sheep-Goat Scale* (ASGS) with an additional item asking about their performance in the current study and incorporating a 7-point Likert response scale ranging from strongly agree to strongly disagree, was also completed.

Given the apparent importance of experimenter-participant interactions and expectations of success, at the end of the pre-judging briefing, the experimenters and participants completed a short *Interaction Questionnaire*.<sup>5</sup> This contained six questions, concerning their personal feelings and expectations and perceptions of the quality of the interactions between experimenter and participants, which they were required to answer by giving ratings on 7-point scales (see Appendix).

### *Procedure*

Once recruited for the experiment, participants were sent — either through the post or by email — the PIF, and they returned this ahead of their trial or brought it in with them on the day of judging. Participants were also sent a paper-copy of a dream diary, which contained instructions to clarify the procedure, information about confidentiality and the experimenter's contact information, as well as space to record their dreams and their associations or comments on dreams that they could recall during the trial period. They kept the dream diary for three consecutive nights; night 1 was a practice night and nights 2 and 3 were experimental nights.

On each night participants slept at their own homes, as normal. Upon waking, either during the night or in the morning, they completed the appropriate section of their dream diary, writing down as much detail concerning their dreams as possible. There was space in the diary for participants to note any associations they may have identified between their dreams and events in their waking lives about which they may have been preoccupied or worried. Participants were also asked to indicate on which of the trial nights they believed there had been a sender and on which there had been no sender. It was intended that the participants make this judgement prior to their meeting with the experimenter but in practice this was not always the case.

A sender (either SS or CR) was allocated to each participant on the basis of availability. Both senders remained blind as to participant identities until after the study was completed. On the evening of Night 2 (the

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<sup>5</sup>Experimenters did not complete question 3.

first experimental night) the sender flipped a coin to determine whether Night 2 would be designated a telepathy or a clairvoyance trial. If Night 2 was a telepathy trial then by default Night 3 was a clairvoyance trial, and vice versa. The experimenter remained blind as to which condition had been selected for each trial night until after the participants' judgments had been made and recorded. On both experimental nights the sender initiated the experimental software at his own home, where a randomly-selected target clip was played from 2:00am until 6:30am. On the telepathy trial only, the target clip was played for the same length of time but the sender watched the target clip between 6:00am and 6:30am<sup>6</sup> and attempted to communicate its content to the participant by ESP. The sender had no contact with the experimenter concerning the experiment until the information was needed about the target sets used for the trial nights.

After keeping the dream diary for three nights the participant travelled to the university campus to take part in the judging.<sup>7</sup> Participants were requested to attend judging as soon as possible after completing the dream diary — ideally the morning after the last dream night — so that dream content might still be retrievable from memory; however, appointments were arranged at their convenience. On arrival they were met by the experimenter, who took them to a reception room where refreshments were available. They were then engaged in an informal conversation incorporating a brief discussion of their experiences. After this interaction the participant was asked to complete a measure that asked them to rate the experimenter along a number of dimensions, such as warmth, spontaneity, and optimism (see Sherwood et al., 2004). The experimenter also completed brief ratings of confidence about the trial and his/her assessment of the interaction with the participant. These forms were placed in a sealed envelope and sent to an independent researcher (IB). Participants were assured that we were only interested in their honest impressions and that at no time would the experimenter be aware of the ratings they had given.

Next, the participant read out their dream diary content; if they

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<sup>6</sup>The sending period was set at 30 minutes after initial pilot work suggested that it was difficult for senders to maintain their interest and focus on the clip over a longer time span. In this respect it is comparable with a typical sending period in a Ganzfeld session. Although there is no guarantee that a sending period of 30 minutes will necessarily overlap with a receiver REM period, Braud (1977) has previously reported successful dream ESP performance when sending between 6:00 and 6:30am.

<sup>7</sup>On some trials, where access to the reception rooms was not possible, judging was conducted in the experimenter's own office or the participant's own home.



had not remembered any dream material they still participated in the judging phase and were advised to use 'intuition' or to see if the clips prompted any dream recall. The judging phase commenced with the experimenter accessing an SMS text message from the sender that identified the target sets for nights 2 and 3 (but not the identities of the target clips). The sender was shown, via a laptop computer, the four clips that made up the target set (consisting of the target clip and three decoys in random order) for night 2, and these were rated and rank ordered for their degree of correspondence to the dream mentation for that night. This process was repeated for the target set and dream mentation for night 3. Once all judgements were recorded the experimenter contacted the sender via mobile phone to discover the identities of the two target clips and which night had been the sender night.

Once the series was completed, IB was provided with trial outcome data (but no personal information from participants) and analysed the relationships between these and the interaction data sent to him. The other members of the team only saw the results of analyses conducted by IB and were not at any stage provided with the raw data from the interaction measures.

## Results

To assess our prediction that participants would award a similarity rating to the target that was higher than the average rating for the three dummy clips for each experimental night,  $z$  scores were calculated (see Table 1). The overall mean  $z$  score for both nights was slightly positive (actual sender night mean  $z = 0.13$ ,  $SD = 0.86$ ; actual no sender night mean  $z = 0.16$ ,  $SD = 0.91$ ), indicating that the target clips tended to be awarded higher dream correspondence ratings than the other clips, but this did not differ significantly from zero (for no sender night: Wilcoxon  $z = -1.06$ ,  $p = .15$ ; for sender night: Wilcoxon  $z = .91$ ,  $p = .18$ , one tailed), and the hypothesis was therefore not supported. We did not plan to evaluate performance in terms of direct hits, but for information we note that the proportion of direct hits is nonsignificantly higher than mean chance expectation of 25% for both sender and no sender trials ( $z = 0.55$  and 1.28 respectively).

Contrary to expectations, the mean  $z$ -score for ratings was greater for no sender trials compared with sender trials (mean  $z = 0.16$  versus 0.13), but a Wilcoxon Signed Ranks test found that the difference was

Table 1: Target rank frequencies and  $z$ -score based upon ratings for actual sender and no sender conditions ( $N = 40$  in each case), with similarity rating (SR) mean  $z$ -scores and standard deviations ( $SD$ )

	Rank				SR	
	1	2	3	4	mean $z$	$SD z$
Sender trials	12 (30.0%)	11 (27.5%)	10 (25.0%)	7 (17.5%)	0.13	0.86
No sender trials	14 (35.0%)	7 (17.5%)	13 (32.5%)	6 (15.0%)	0.16	0.91

not significant ( $z = -0.22, p = .41$ , one-tailed), suggesting that having a sender did not help participants' performance. Neither condition deviated significantly from the null value of  $z = 0$ .

As well as looking for gross differences between sender and no sender conditions we were able here to consider sender differences, since this study involved two senders who were quasi-randomly allocated to participants and who contributed 20 trials each. Exploratory analysis of the results by the different senders are given in Table 2, and reveal an interesting but unexpected interaction effect: SS's participants performed as predicted, with greater success on sender nights compared with no sender nights, although this difference is not significant (Wilcoxon  $z = 1.23, p = .22$ , two-tailed); however, the reverse was true for CR's participants, with better performance on no sender nights compared with sender nights, to a degree that was suggestive (Wilcoxon  $z = -1.66, p = .10$ , two-tailed). Thus, whether or not having a sender try to send the target is advantageous may depend on who the sender is. This full interaction trend, which could explain the lack of an overall sender effect, is further illustrated in Figure 1.

Table 2: Target rank frequencies and  $z$ -score based upon ratings for actual sender and no sender conditions ( $N = 20$  in all cases), with similarity rating (SR) mean  $z$ -scores and standard deviations ( $SD$ )

	Rank				SR	
	1	2	3	4	mean $z$	$SD z$
CR						
Sender	5 (25.0%)	5 (25.0%)	5 (25.0%)	5 (25.0%)	-0.04	0.91
No sender	9 (45.0%)	4 (20.0%)	5 (25.0%)	2 (10.0%)	0.32	0.91
SS						
Sender	7 (35.0%)	6 (30.0%)	5 (25.0%)	2 (10.0%)	0.30	0.79
No sender	5 (25.0%)	3 (15.0%)	8 (40.0%)	4 (20.0%)	0.002	0.83

We also planned to consider the effects upon performance of partic-

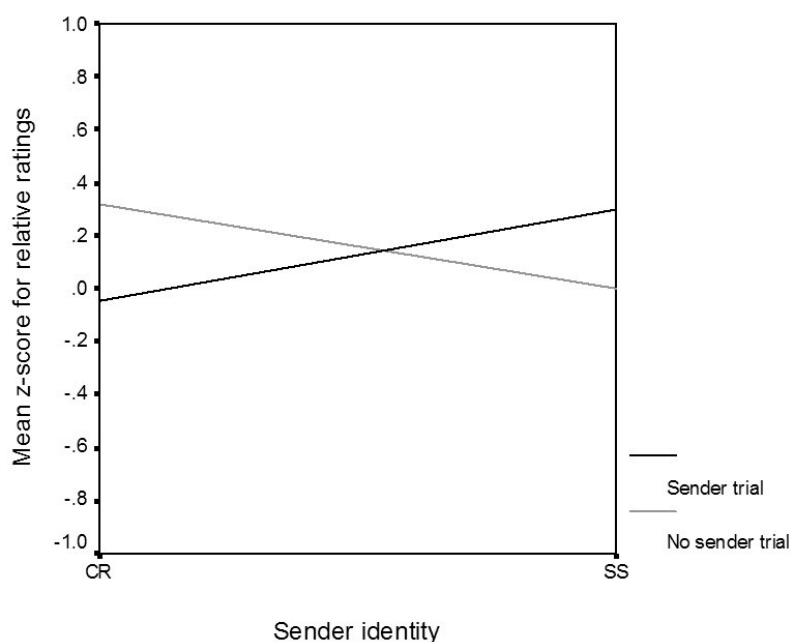


Figure 1. Mean  $z$ -scores based upon ratings for actual sender and no sender conditions according to sender

participants' belief that a sender had been operating, and so participants were asked to nominate on which night they believed there was a sender and on which night there was not (of course, they were blind to actual sender status). Participants correctly identified the order of sender and no-sender conditions for their trial on just 18 of the 40 trials, which is slightly less than chance expectation of 20 and suggests that participants could not accurately identify the sender condition. This allows us to consider the effects of 'expectancy' separately from actual condition, since the two variables are unrelated. A summary of participant performance on nights when they believed there was a sender operating compared with nights when they believed there was no sender are given in Table 3. Contrary to expectations, the mean  $z$ -score for ratings was slightly greater for the trials when the participants did not believe there had been a sender compared to the trials when they believed there had been a sender, although a Wilcoxon Signed Ranks test found that this difference was not statistically significant ( $z = -0.18$ ,  $p = .46$ , one-tailed). Thus, judgements regarding sender status did not affect performance in the dream ESP task.

We also conducted a combined analysis that looked for possible interactions between actual sender status and participants' judgements that there had been a sender. These data are summarised in Table 4, and

Table 3: Target rank frequencies and  $z$ -score based upon ratings for conditions deemed to be sender and no sender ( $N = 20$  in all cases), with similarity rating (SR) mean  $z$ -scores and standard deviations ( $SD$ )

	Rank				SR	
	1	2	3	4	mean $z$	$SD z$
Deemed Sender	12 (30.0%)	9 (22.5%)	13 (32.5%)	6 (15.0%)	0.13	0.92
Deemed No sender	14 (35.0%)	9 (22.5%)	10 (25.0%)	7 (17.5%)	0.16	0.86

show that, in terms of our planned outcome measure of  $z$ -scores for similarity ratings<sup>8</sup>, the worst performance was for the condition that was expected to be optimal, where participants believed there had been a sender and there in fact was, although the differences across conditions are not significant (Wilcoxon  $z = -.24$ ,  $p = .81$ , two-tailed).<sup>9</sup> Where participants wrongly identified sender and no sender conditions there is no difference whatever in performance between the two trials (Wilcoxon  $z = 0.00$ ,  $p = 1.00$ , two-tailed). Taken together, these results give little indication of any overall sender or sender expectancy effect in this study but there is some indication that this may be influenced by who the sender was; indeed, based on the planned outcome measure of  $z$  scores, these results do not give evidence of ESP at all (despite the percentage hit rates being reasonably consistent with others' Ganzfeld findings).

Finally we conducted exploratory analyses to see whether performance covaried with measures of the quality of interaction between experimenter and participant. Spearman rank order correlations between the different questionnaire ratings and the experimental results are summarised in Table 5, and include separate analyses for each experimenter. Generally, there is little indication from these that performance at the dream ESP task can be predicted by scores on interaction measures, with only three of 90 correlations achieving significance. Although all of these were associated with experimenter or participant mood, the effects seem to be reversed for sender and no sender trials, making interpretation difficult; since the analyses have not been cor-

<sup>8</sup>In terms of direct hits, the best performance occurred with no sender trials in which the participant believed there had been no sender and worst performance is for sender trials where participants believed there had been a sender. In terms of mean ranks, performance is again worst with the sender-sender condition that was expected to be optimal; performance in the other three conditions is very similar.

<sup>9</sup>An omnibus test of these data would not be appropriate given their non-independence (all participants contribute to two of the  $2 \times 2$  cells)

Table 4: Target rank frequencies and  $z$ -score based upon ratings for actual sender and no sender conditions separated by participants' judgments of sender status, and similarity rating (SR) mean  $z$ -scores and standard deviations ( $SD$ )

	Rank				SR	
	1	2	3	4	mean $z$	$SD z$
Deemed Sender						
Sender present <sup>a</sup>	5 (28%)	4 (22%)	6 (33%)	3 (17%)	.053	.927
No sender present <sup>b</sup>	7 (32%)	5 (23%)	7 (32%)	3 (14%)	.187	.922
Deemed No Sender						
Sender present <sup>b</sup>	7 (32%)	7 (32%)	4 (18%)	4 (18%)	.184	.821
No sender present <sup>a</sup>	7 (39%)	2 (11%)	6 (33%)	3 (17%)	.128	.924

<sup>a</sup> $N = 18$

<sup>b</sup> $N = 22$

rected for multiple analyses they may simply reflect random noise and would need to be replicated in future work before they should be interpreted as a real effect. Some other associations generated relatively large coefficients, particularly with confidence, although this is in the opposite direction to prediction. There do seem to be some differences between experimenters, which perhaps reflect their different personalities and interaction styles.

## Discussion

Although hit rates of 30% and 35% over two sets of 40 dream ESP trial nights is somewhat above the mean chance expectation of 25%, and represents an improvement on the overall hit rate of 21.1% from our previous study that used Ganzfeld stimulation, deviations from chance expectation based on  $z$  scores of similarity ratings (our pre-specified dependent variable) were not sufficient to give statistical significance (respectively, mean  $z = 0.13$ ,  $t_{(39)} = 0.92$ ,  $p = .18$ , one-tailed; mean  $z = 0.16$ ,  $t_{(39)} = 1.11$ ,  $p = .14$ , one-tailed), and so we are unable to reject the null hypothesis. Of course, we may not expect to see evidence of psi in any summary measure since this study included conditions that were hypothesised to show differential psi performance, although we did not confirm our prediction that participants would perform better on sender trials than on no-sender trials. This failure to capture any sender effect is consistent with Sherwood et al.'s (2005) failure to find a sender effect in earlier Ganzfeld work and with Sherwood and Roe's (2003) finding that post-Maimonides dream ESP studies that had investigated clairvoyance had in fact been more successful than those ostensi-

Table 5: Spearman correlation co-efficients for  $z$ -score experiment results for the sender and no-sender conditions correlated against the sender and experimenter questionnaire ratings. The co-efficients are shown for the overall results, and also broken down by experimenter

Questions	<i>Experimenter</i>						
	Overall ( $N = 40$ )		LS ( $N = 26$ )		LF ( $N = 14$ )		
	Sender	No Sender	Sender	No Sender	Sender	No Sender	
<i>Participant</i>	Mood	-.03	.08	-.13	-.14	.08	.64*
	Feeling	-.14	-.14	-.27	-.11	-.46	-.14
	Optimism	.23	.18	.20	.10	.22	.28
	Confidence	-.09	-.09	-.04	-.07	-.52	-.15
	Rapport	-.07	-.03	-.03	.07	.00	-.13
	Warmth	-.05	-.04	.00	.08	-.18	-.16
	Spontaneity	.08 <sup>a</sup>	-.13 <sup>a</sup>	.02 <sup>b</sup>	-.14 <sup>b</sup>	.05 <sup>c</sup>	-.01 <sup>c</sup>
	Positiveness	.11 <sup>a</sup>	.02 <sup>a</sup>	.15 <sup>b</sup>	.11 <sup>b</sup>	-.04 <sup>c</sup>	.02 <sup>c</sup>
<i>Experimenter</i>	Mood	-.34*	.22	-.28	.04	-.12	.62*
	Feeling	-.07	-.05	-.08	.10	.14	-.11
	Rapport	-.11	.20	-.21	.27	-.08	.09
	Confidence	-.11	.05	.13	-.10	-.50	.26
	Warmth	.04	.16	-.09	.36	.16	-.23
	Spontaneity	.04	.15	-.06	.31	.31	-.09
	Positiveness	-.10	.23	-.10	.34	-.04	.11

\*  $p < .03$  (2-tailed).

<sup>a</sup> $N = 38$  due to missing values.

<sup>b</sup> $N = 25$  due to missing values.

<sup>c</sup> $N = 13$  due to missing values.

bly investigating telepathy — despite earlier researchers advocating the involvement of an agent (e.g., Ullman et al., 1973). One might speculate, then, that it may be desirable to concentrate on clairvoyance designs in future research, given the advantages this would offer in recruitment (since participants could be scheduled one at a time instead of having to co-ordinate across two people) and in security (with sensory leakage and cheating made less likely when no-one needs to be aware of the target until after judging is completed), and apparently minimal disadvantages in terms of impact upon effect sizes.

One form in which senders might play a role without being essential to any psi process is if they make the task seem inherently more plausible or help diffuse responsibility for any psi that occurs (see Roe et al., 2004, for a fuller discussion), and indeed Roe et al. did report a sender expectancy effect, whereby participants performed better when they believed that a sender was involved compared with when they believed there was no sender (with hit rates of 33.3% and 18.2% respec-

tively), irrespective of actual sender status. In this study there was no indication of any sender expectancy effect, with better performance actually occurring where participants believed there was no sender. This may not be a legitimate comparison, however, since in Roe et al.'s (2004) study participant expectancy was manipulated by giving either true or false information at the beginning of the session, whereas in this study participants decided retrospectively for themselves, typically by reflecting on their dream mentation, whether a trial involved a sender. In this latter case then, participants might have no particular expectancy as to whether or not the first trial will involve a sender, since at that point they have no material on which to base their judgement, and it may be that any expectancy effect would only be expressed on the second trial night (although this too may be problematic, since some participants in the current study only made their judgements after completing both nights).<sup>10</sup>

In any case, any potential advantages of involving a sender may have been undermined here by our decision on security grounds to keep the senders and participants isolated from one another to the extent that senders and participants were given no information whatsoever about one another. Several of the participants recruited and run by LF expressed doubt that telepathy would be possible under circumstances in which the people involved had never established any rapport, indeed had never met each other or even knew each other's names. Likewise the senders found it a handicap to have no shared experiences or knowledge about the participant on which to base their sending strategy. We should note, however, that although Honorton et al. (1990) reported a suggestive advantage for friends as senders, Broughton and Alexander (1997) found that participants with a lab-assigned sender achieved a much higher hit rate than those who had a spouse or friend serve in that role. Nevertheless, on reflection, this security measure seems Draconian, and in future studies greater effort would be needed to ensure that some degree of rapport is possible, for example by adopting aspects of the methodology used in remote healing studies (e.g. providing a photograph or the first name of the other party, as in Sicher, Targ, Moore and Smith, 1998).

Analysis of participant-experimenter interaction data did not re-

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<sup>10</sup>We are grateful to our referees for suggesting that it would be useful in future studies to have participants rate the confidence they have in their estimates and also to ask participants how they decided whether trials had been sender or no sender.

veal any strong trends, and given the scope for committing Type I errors, no confident claims are made from these results. If taken at face value, the best predictors seem to be participant and experimenter mood, although these only emerge in some conditions and not others, and do not confirm our earlier finding of a negative relationship between outcome and receiver mood (Sherwood et al., 2005). Previous promising findings with optimism and confidence of success were not confirmed here. It may be unrealistic to expect experimenter-participant interaction findings to replicate across studies given that the participants (and experimenters in this case) are different, and efforts need to be made to ensure continuity across studies. In any case, in the present study there may have been limited opportunity for experimenter interaction effects to occur; with the switch from a Ganzfeld to a dream ESP design most of the recruitment and running of the study was conducted by telephone, email and by post, so that there was a large reduction in the amount of face-to-face interaction until the judging session, by which time any psi performance is likely to be over (unless it occurs during judging). There might, therefore, have been little opportunity for the interaction to affect performance unless it could act retrospectively. Future tests of interaction effects would need to ensure that there is sufficient interaction between the parties so as to provide an opportunity for this to be influential.

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## Appendix

### *Interaction questionnaire*

1. How would you rate your current mood?

(Negative — Positive)

2. How do you feel at this moment?

(Tense — Relaxed)

3. How do you feel about the prospect of participating in this experiment?

(Pessimistic — Optimistic)

4. How confident are you that today's experiment will be a success?

(Not at all confident — Extremely confident)

5. How would you describe the quality of rapport that you have with the Experimenter?

(Extremely poor — Extremely good)

6. How would you rate the quality of the interaction between experimenter and participants?

(Very cold — Very warm)

('Rehearsed' — Spontaneous)

(Very negative — Very positive)