European Journal of Parapsychology

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*SPECIAL ISSUE*
*Memory & Psi*

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# European Journal of Parapsychology

## Special Issue: Memory and Psi

Volume 21.2

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# Editors' Foreword: Memory and Psi

Welcome to the first Special Issue of the *European Journal of Parapsychology* since we took over the editorship of the journal. The theme of "Memory and Psi" is based on papers from the 6th Bial Foundation Symposium earlier this year. By publishing these papers together in one volume, we hope that it will provide a valuable resource to researchers, hopefully provoking further research and debate in this somewhat neglected area.

It should be noted that the Special Issue papers have not undergone the usual *EJP* peer-review process: these papers were originally invited by the organising committee of the Bial Symposium, and then we extended an invitation to the authors to have their papers published in a themed issue of the *EJP*, subject to Editorial review. It should also be noted that the original remit outlined by the Bial Foundation was that the papers be accessible to a wide audience, and that the content of the papers reflects this.

This Special Issue would not have been possible without the co-operation of the contributors, and therefore we must thank all of the authors – John Palmer, Rex Stanford, Richard Broughton, Christopher French and Krissy Wilson, and Stephen Braude – for their efforts, and Caroline Watt for her introduction. We would also like to thank the Bial Foundation, and Luís Portela in particular, for kindly allowing us to publish these papers and for inspiring this issue. Finally, we would also like to extend a special thank you to Chris Roe for suggesting the idea of having a special issue of the *EJP* dedicated to this topic, and for initially approaching the authors on our behalf.

Ian S. Baker and Paul Stevens

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# Introduction to the EJP Special Issue on Memory and Psi

Caroline Watt
Organising Committee Member,
6th Bial Foundation Symposium
and Session Chair: Memory and Psi

About two years ago, the organising committee were discussing possible themes for the 2006 Bial Symposium. When the theme of memory was suggested, I was at first a little uncertain how this theme would link with the topic of psi. It is easy to see how memory is of interest to neuroscientists and neuropsychologists. But what role does memory play in psi? Memory is a normal and fairly well-understood human capacity, whereas alleged psi abilities extend *beyond* what is currently known about human capabilities. Can we really expect to find links between the normal and the paranormal? Between memory and psi?

Actually parapsychologists have for many years considered the role that memory plays in psi experiences. They have investigated the question of memory and psi using both theoretical and experimental approaches. However, most of this work into memory and psi rests on a basic assumption. Parapsychologists generally assume that once information that is of extrasensory origin reaches a person's brain, that information may be processed and responded to using normal cognitive mechanisms. You could think of it as a two-stage process. Firstly, extrasensory information reaches the brain through as yet unknown means. Secondly, that information is processed and responded to in much the same way as the brain processes similar sensory information. Just as a faint sound or a scent can evoke a memory, an extrasensory impression may also evoke a memory. Memory may therefore play a role

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in enabling the expression of information that is of extrasensory origin. Each of the papers in this special issue illustrates this point in different ways.

John Palmer reviews how parapsychologists have used laboratory testing to study the role of memory in the processing of extrasensory information. The studies have typically compared performance on a memory task with performance on a related psi task. Dr Palmer describes some of these studies and considers what their findings can tell us about memory and psi.

Rex Stanford reviews laboratory findings that suggest how 'receptive psi' interacts with memory-related structures and processes. Receptive psi refers to the organism's acquisition of information through currently unexplained processes. Dr Stanford also considers how cognitive psychologists' research on the cognitive unconscious may inform the modelling of receptive psi functions.

Richard Broughton takes a theoretical perspective to ask how people's memories and emotions may affect their spontaneous psi experiences — that is, psi experiences outside the laboratory. He uses ghost experiences as an example and asks: how can such waking hallucinations be interpreted by the experient as anomalous or psychic, and what role does emotion and memory play here?

Christopher French and Krissy Wilson consider how memory processes may lead to erroneous claims of anomalous experiences. In particular they discuss how research in the field of eyewitness testimony and false memory may be relevant for reports of anomalous events.

Stephen Braude takes a wider philosophical perspective and presents a critique of how ideas about memory, and particularly memory traces, have been applied in psychology and neurophysiology. He argues that this misuse of the concept of memory has crept into parapsychological theorising, for example in connection with the evidence for postmortem survival.

When I was investigating the parapsychological literature on memory and psi in order to consider which speakers to invite to participate in the 2006 Bial Symposium, it struck me that although it seems that memory may play a pivotal role in psi experiences, there has been very little research on this question in recent decades. I hope that in reading this collection of papers, researchers may be inspired to reverse that trend.

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# Memory and ESP — A Review of the Experimental Literature

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**Abstract**

*This paper reviews the experimental evidence relating extrasensory perception (ESP) to memory processes. Most of the experiments were intended to directly or indirectly test William Rolls' (1966) theory that ESP consists of the activation of memory traces. The test procedures have generally consisted of correlating scores on tests of memory with scores on forced-choice ESP tests that are related to, and sometimes embedded in, the memory test. The review found suggestive evidence for the following relationships: (a) participants score above chance on ESP test items linked to correctly remembered paired associates and below chance on ESP items linked to incorrectly remembered paired associates; (b) when participants score above chance on an ESP test in which they are asked to guess the second word of a pair of words to which they had not been exposed, incorrect guesses tend to be similar in meaning to the correct choice; (c) memory can be influenced by the presence of relevant ESP target material, at least among participants who believe moderately in ESP and are in a positive mood at the time of testing; (d) there is a linear relationship between total scores on an ESP test and a related test of memory, but the direction of the relationship is inconsistent. A positive relation tends to emerge when secondary memory is tested and a negative relationship when primary memory is tested; (e) in a word association test, persons with strong biases to respond with primary associates score better on a related ESP test when they give secondary responses, whereas persons with less or no such biases score better on the ESP test when they give primary responses.*

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## Introduction

As first noted explicitly by G. N. M. Tyrell (1946–1949), the ESP process can be divided into two stages. Stage one concerns how the information gets from the source to the receiver, and Stage one refers to how it is processed by the mind once it gets to the receiver. Stage one is primarily the domain of physics, whereas Stage two falls under psychology. Most process-oriented parapsychological research has concerned itself with Stage two, partly because most parapsychologists have been trained as psychologists, but also because the research questions in this domain are at present more amenable to scientific investigation. It is considered axiomatic that when an ESP signal reaches an organism, it undergoes cognitive processing that restructures the stimulus and often distorts it. If we are ever to gain a full theoretical understanding of psi, as well as improve its accuracy and reliability, it is essential that we understand these cognitive mechanisms in some detail.

It is reasonable to suppose that the cognitive processing of ESP stimuli involves memory to at least some degree. Parapsychologists' interest in a possible memory/ESP relationship was inspired by a theory of William Roll (1966) that ESP consists of the activation of memory traces. If this view is correct, memory plays not only a role in the cognitive processing of ESP, but a very central role. As is the case with most research in the psychology of psi, the methodology applied to the ESP/memory relationship has been primarily correlational. More specifically, scores on tests of memory have been correlated with scores on forced-choice ESP tests that are related to, and sometimes embedded in, the memory test. Various characteristics of these tests have often been manipulated to gain further insight into the process and sometimes to explicitly test inferences from Roll's theory.

## The first experiment

The first published experiment to compare the results on tests of memory and ESP was conducted at the Foundation for Research on the Nature of Man (FRNM), the heir to J. B. Rhine's famous Duke Parapsychology Laboratory, by Rhine's daughter, Sally Feather (1967). The exploration began quite by accident. In an effort to induce frustration in order to obtain below-chance scoring on a standard ESP card-guessing

test, Feather gave her participants only 15 seconds to memorize a randomized list of 25 of the standard ESP symbols immediately before the guessing task. To her surprise, she found that the memory and ESP scores were positively correlated ($p < .04$), but these results were not reported in detail. She followed this up with 3 additional series, in each of which 10 participants, mostly high school students, were tested individually. Between two sets of 2 or 3 25-trial card-guessing runs, participants were again given 15 seconds to memorize a sequence of 25 of the same ESP symbols used in the ESP test, but in a different order. Finally, they were asked to recall the ESP symbols they had attempted to memorize. The correlations between the ESP scores and the memory scores were positive in all three series, and the combined result was significant ($p = .012$). As noted by Rao, Morrison, and Davis (1977), the short time duration for the memory test could mean that it functioned more like an ESP test, in which case the positive correlation would be attributable to the fact that the two tests were essentially equivalent.

## ESP, recall, and association strength with paired associates

### *The Kanthamani experiments*

Several years later the thread initiated by Feather was picked up by another investigator at FRNM, the Indian psychologist H. Kanthamani. Her experiments differed from Feather's in two fundamental respects. First, paired-associate learning was substituted for the simple serial learning task used by Feather. Second, the memory-ESP association was based on the trial rather than the run, to better reflect the "possible interaction between the two processes" (Kanthamani & Rao, 1974, p. 287). It was predicted that ESP scoring would be better on those trials in which the associate was remembered correctly than on those trials in which it was remembered incorrectly or not at all. A preliminary experiment, used to refine the details of the methodology, was followed by a pilot series and two confirmatory series. In the preliminary series, six participants were given from 3.5 to 6 minutes to memorize a list of 20 word pairs that were the alternative English meanings of 20 Telegu words also listed on the sheet. Following a 2–3 minute interference period, participants were given a sheet containing only the Telegu words and asked to write down both English equivalents. Participants made their ESP guesses by circling which of their two memory responses they thought

was the randomly chosen ESP target for the trial. The target was really the location of the word in the pair (first or second), not the word itself. During each trial, the experimenter attempted to transmit the correct target telepathically from another room. ESP results were significantly below chance on recall-correct (RC) trials and lower than the scores on recall-wrong (RW) trials, opposite to the prediction.

A major change was introduced for the pilot series, in that the Telegu words were eliminated entirely and the 16 participants, all high school students, were given one of the two English words as the cue stimulus during recall. As this was intended to make memory easier, the learning time was reduced from 3.5 minutes to 3 minutes, and there was no sender. The RC/RW effect reversed, with significantly above-chance scoring on the RC trials and only slightly above chance scoring on the WR trials, thereby confirming the prediction. This result will hereafter be referred to as the RC/RW effect.

The two confirmatory series, each with $N = 20$ high school students, differed from the pilot series and from each other in the exact way participants made their ESP responses. In the first confirmatory series, the ESP target was defined as whether participants wrote their guess of the correct associate in capital or small letters. When no guess could be made, the letters C (or c) or S (or s) were substituted. In the second confirmatory series, the ESP targets were defined as on which of two adjacent lines participants wrote their guess of each associate. In both series, ESP scoring was significantly positive on the RC trials. In the first series, the WR trials were very close to chance and in the second series they were significantly negative and significantly lower than the RC trials, supporting the hypothesis.

In two additional series, each with 20 high school students, the authors varied the association strength of the associates (Kanthamani & Rao, 1975). Association strength refers to how closely related the words are in meaning, but the relationship need not be similarity. For instance, antonyms (e.g., hot–cold) would also have high association strength. In the previous series, the associates, being the two meanings of Telegu homonyms, were generally if not exclusively of low association strength. The experimental task again consisted of 20 paired associates, which participants were given only 1 minute to memorize, compared to 3 minutes in the confirmatory series of Kanthamani and Rao (1974). The first member of each word pair was a trigram that would be meaningful to the participants (e.g., USA), whereas the sec-

ond was a word. For half the trials, the association strength between the trigram and the word was high, whereas for the other half it was low, as rated by a sample of 10 different high school students. The order of trial types was randomised within each list of 20 pairs. In Series 1, as expected, scores on the RC trials were significantly above chance, and significantly higher than on the nonsignificant RW trials. In Series 2 the overall mean shifted in the negative direction, but the ratio of scoring on the RC and RW trials remained about the same: scores on the RW trials were significantly negative and significantly lower than scores on the nonsignificant RC trials. In both series, the RC/RW effect was driven by the low-association pairs, for which the RC trials were significantly positive in Series 1 and the RW trials significantly negative in Series 2. The RC-RW difference was significant in both series. Thus, in this sense the hypothesis was confirmed, although it is awkward that the only significant evidence for psi in Series 2 (i.e., results significantly different than chance) occurred among the wrongly remembered associates.

The two articles cited above (Kanthamani & Rao, 1974, 1975) reported the results of six series, five of which obtained higher scoring on RC than on RW pairs, three significantly so. The one reversal was the small preliminary series of Kanthamani and Rao (1974), in which the memory task was markedly different than in the other series. Collectively, these experiments provide strong support for the RC/RW hypothesis, provided the word pairs have low association values. To assess the overall statistical significance of the Kanthamani experiments, I combined the $z$ statistics from all six series using the Stouffer method (see Table 1). The component $z$s were not weighted by sample size. The difference between RC and RW trials produced a Stouffer $Z$ of 2.82 ($p <$ .005). The RC/RW effect would have even been stronger had the analysed results been limited to low-association word pair and the components of the Stouffer $Z$s weighted by sample size. On the minus side, it should be noted that the authors computed their statistics using the trial rather than the participant as the unit of analysis, and I had to do likewise in computing the Stouffer $Z$. Using the trial as the unit of analysis provides a more powerful statistical test, but the results cannot properly be generalized to a population of persons because the effect in the sample may not have been distributed uniformly across the sample participants (see Stanford & Palmer, 1972). However, the obtained effect is so strong that it is unlikely an analysis with the participant as the unit would lead to a different conclusion. I was also able to calculate Stouffer

$Z$s for the RC and RW trials separately. For RC trials, the results were significantly positive ($Z$ = 2.59, $p < .01$), whereas for RW trials they were significantly negative ($Z$ = 2.52, $p < .01$).

Table 1: Total Number ($T$) and mean proportion of ESP hits on correctly and wrongly recalled trials in the Kanthamani and Rao paired-associate recall experiments, by year of publication

| | | Recall-Correct (RC) | | | Recall-Wrong (RW) | | | |
|---|---|---|---|---|---|---|---|---|
| Series | $N$ | $T$ | $M$ | $Z$ | $T$ | $M$ | $Z$ | $Z_{diff}$ |
| 1974 (Preliminary) | 6 | 58 | .328 | -2.62 | 62 | .468 | -.51 | -1.53 |
| 1974 (Pilot) | 16 | 150 | .607 | 2.61 | 170 | .482 | -.46 | 1.42 |
| 1974 (Series 1) | 20 | 279 | .574 | 2.45 | 121 | .496 | -.09 | .70 |
| 1974 (Series 2) | 20 | 242 | .558 | 1.80 | 158 | .411 | -2.23 | 2.30 |
| 1975 (Series 1) | 20 | 307 | .573 | 2.57 | 93 | .473 | -.51 | 2.01 |
| 1975 (Series 2) | 20 | 229 | .485 | -.46 | 171 | .410 | -2.37 | 2.00 |
| *Total* | *102* | *1265* | *.521*[a] | *2.59*[b] | *775* | *.457*[a] | *-2.52*[b] | *2.82*[b] |

[a]Unweighted mean
[b]Stouffer $Z$

*Attempted replications*

The RC/RW hypothesis was significantly confirmed in one of two paired-associate memory series reported by O'Brien (1976), in which words were paired with nonsense syllables, but only if the words were found to occur with low frequency in a normative population. Several other independent replication attempts failed to provide significant support for the hypothesis (Gambale, 1976; Gambale, Margolis, and Cruci, 1976; Harary, 1976; Lieberman, 1975). Gambale et al. reported significant below-chance scoring for RC word pairs, which suggests that the difference between scoring on RC and RW word pairs was most likely in the direction opposite the prediction. Nothing was mentioned about the association strength of the word pairs. Lieberman found a significant interaction indicating superior scoring on low-association pairs when participants were tested individually and superior scoring on high-association pairs when they were tested in groups. However, the effect was not reported separately for RC and RW trials. Nonetheless, this result suggests that perhaps the effect found by Kanthamani only occurs with participants tested individually, which is the procedure she used. Among the other studies listed above, only Harary used group testing. Finally, Parker (1976) obtained significantly higher scoring in RC compared to RW trials in one of two series using a digit-span test instead of the paired-associate test. In the digit-span test, partici-

pants were required to memorize and recall sequences of digits varying in length from three to nine.

Unfortunately, only abstracts are available as reports of these replication attempts, and they did not include specific means and significance test results. If the nonsignificant trends were generally in the predicted direction, a combination of all the results might continue to show a significant overall effect, but this, of course, remains speculative. In any event, it appears, as is often the case in parapsychology, that experimenters differ widely in their capacity to obtain a given relationship (Palmer, 1997; White, 1977).

*Relation to the memory-trace theory*

Kanthamani and Rao (1974) stated that the confirmation of the RC/RW hypothesis is in line with Roll's (1966) memory-trace theory because the theory "contends that memory traces of recently learned events serve as good vehicles of ESP" (p. 297). However, it is clear from the context in which this point was embedded that Roll meant that the memory trace itself becomes the ESP response. In the Kanthamani experiments, the memory traces that served as the 'good vehicles' were the correctly remembered paired associates, whereas the ESP responses consisted of such decisions as the line on which the word was written. The corresponding ESP targets were selected to be completely independent of the remembered words, and thus it is difficult to imagine the word memories mediating the location memories.

An alternative explanation of these findings is that successful recall created a momentary feeling of accomplishment, or positive mood, that somehow facilitated ESP success on the RC trials. One would expect this satisfaction to be greater if the recall was difficult, which could account for the superior scoring on RC trials being greater when the word pairs had low association strength.

## Proximate hits in a paired-associate test: The Rao experiments

At FRNM the Kanthamani research was extended by the Indian psychologist K. Ramakrishna Rao and colleagues (Rao, 1978; Rao, Kanthamani, & Palmer, 1990; Rao, Morrison, & Davis, 1977; Rao, Morrison, Davis, & Freeman, 1977a). The participants were high school students tested in groups in a classroom setting. They were given 5 minutes to memorize a set of 50 word pairs. Each pair consisted of

a 'high-association' trigram followed by one of 10 meaningful words that differed in semantic similarity (meaning) to one another. 'High-association' means that the trigram would tend to evoke a meaningful word (e.g., 'FAM' might lead one to think of 'fame' or 'family', whereas 'JEX', a low-association trigram, would have such a tendency much less). The words were then randomly matched to the trigrams, which suggests that the associations *between* trigrams and words were primarily if not exclusively low. After the learning period, participants were given a recall sheet with 25 of the original trigrams plus 25 new high-association trigrams, in random order. They were informed that the new trigrams constituted an ESP test, in which one of the 10 words had been randomly assigned as a target to each trigram. Thus, the recall sheet included a 25-trial memory test and a 25-trial ESP test. The recall sheet was affixed to an opaque envelope, inside of which was a sheet of paper containing the correct answers for all 50 trials. Participants were asked to write on a line next to each trigram on the recall sheet the word they thought had been connected to it on the learning sheet.

Up to this point, the procedure was similar to that used by Kanthamani in the earlier experiments. The primary difference is that instead of there being both a memory score and an ESP score for each trial, half the trials provided only memory scores and the other half only ESP scores. Thus the RC/RW hypothesis could not be tested in these experiments. Following the memory/ESP test, a new element was added to the procedure. Participants were given a sheet containing the 10 test words in a column followed by nine blank lines. They were asked to rank the other 9 words on the basis of their semantic similarity to the first word in the row by writing them in the appropriate order on the nine blank lines, thereby creating a $10 \times 10$ matrix. Each of the participants' incorrect responses ('misses') provided an 'association rank score' (ARS), which consisted of the rank given to the response relative to the target word for that trial. Thus, if the target for a given trial was 'ball' and the participant selected 'moon' as the response and wrote 'moon' on the second line following 'ball', his or her ARS for that trial would be 2. It was predicted, in effect, that participants with overall positive and negative ESP scores ('hitters' and 'missers') would achieve significantly different mean ARSs on their missing trials (Rao et al., 1977). Participants whose ESP scores were within $\pm$ 3 hits

from chance were eliminated from the analyses.[1] The hypothesis did not specify a direction for the relationship, and no rationale was given for it in the Introduction.

The first report (Rao et al., 1977) described two series, each of which consisted of seven classroom test sessions with different groups. The first two testing sessions for Series 1 differed from the others in that low-association trigrams were used and participants were not told that an ESP test was involved. Nonetheless, these sessions were pooled with the others in reporting Series 1. The hypothesis was confirmed, in that the mean ARS of the hitters was significantly lower than that of the missers. This means that on trials where hitters guessed wrong, they tended to guess with a word more similar in meaning to the target word than did the missers. The results were based on scores from 150 of the 162 participants, and results from the first two sessions confirmed the general trend. For Series 2, the effect was in the predicted direction but did not achieve significance. Results were based on 118 of the 132 total participants. The second report (Rao et al., 1977a) describes one series based on three classes with total $N = 76$. The comparison of ARS means between hitters and missers was very slightly in the predicted direction and nonsignificant. For the third series (Rao, 1978), the 95 participants were attendees at a lecture on parapsychology and thus, unlike the previous experiments, represented a wide range of ages. For unexplained reasons, the pool of meaningful words was reduced from 10 to five. The results for the AR hypothesis were again in the predicted direction but nonsignificant. The fourth experiment was reported only as a short abstract and only minimal data were provided (Rao & Weiner, 1982). The only significant statistic reported was overall negative scoring on the ESP test. However, Rao et al. (1990) reported that "the missers responded much like the hitters in that their missing responses had closer association with target words, $t_{(41)} = 1.57$" (p. 249). This sentence implies that both hitters and missers had low mean ARSs that were similar to each other. In the fifth experiment (Rao & Kanthamani, 1989; Rao et al., 1990), 104 high-school students were tested, with the ARS means of hitters and missers both well below the midpoint of the scale and nearly identical. A manipulation was introduced in this experiment, such that for some of the participants the answer sheet inside the opaque envelope was blank. Support for the AR hypothesis was actually stronger

[1] The authors specified in addition a more rigid criterion for inclusion, but I will base my review on the first criterion for all studies.

(and marginally significant, $p$ = .07) in this condition than in the standard condition. However, it appears that the two conditions did not differ significantly from each other, and thus it is best to combine them for the purpose of drawing conclusions about the status of the overall hypothesis.

My name was added to the second report of this experiment because I discovered a possible artifact in the ARS measure. In the report, I gave an extreme-case example to illustrate the problem:

> "Imagine... that a particular subject ranked the word *love* as the closest associate (i.e., a rank of 1) to all the other 9 words on the association rank sheet. Imagine also that this subject used the word *love* as his or her response on all the ESP-missing trials. His or her [mean] association rank score would be 1 (the lowest possible value), even though ESP was clearly not involved."
>
> (Rao et al., 1990, p. 255)

Of course, if a participant were found to have responded in such an extreme manner as in the above example, their data would be discarded. However, a subtler version of the same response pattern could skew the results enough to affect their statistical significance. When a correction was applied to the data, the ARS means of both groups moved from an average of .484 to .489. The latter value, however, remained significantly below the midpoint of the scale ($p < .05$).

I was able to combine the results from all six of the Rao studies that tested the AR hypothesis (see Table 2). This time, results were reported by the authors as $t$-tests using the participant as the unit of analysis.[2] However, because $t$s were never reported for the subgroups and sometimes neither were the subgroup sizes ($n$), I at times had to estimate these statistics to compute the necessary $t$s.[3] I believe that my estimates

[2]Because the $t$s were virtually identical to the $z$s derived from their $p$-values, the $t$s were used to compute the Stouffer Zs.

[3]Only two of the six studies provided sufficient data for the necessary calculations. Five of the six experiments reported $t$ tests distinguishing hitters form missers, and this allowed computation of the overall standard deviations (*SD*s) and standard errors. The ratio of hitters' *SD*s to total *SD*s in the two completely reported studies were used to estimate the hitter *SD*s for the other three experiments in the group of five. The subsample $n$s for hitters and missers for the three experiments in which these were not reported were estimated from the average ratios of hitters to missers in the three studies where they were reported. In all three of these there were more hitters than missers. This result is expected, because the low expected hit probability of .1, combined with the small number of trials per participant,

are close to the true values and, more importantly, unbiased. For reasons of comparability, the uncorrected results from the last study were used.

The hypothesis of lower mean ARSs on miss trials among missers as compared to hitters was just on the borderline of significance (Stouffer $Z$ = 1.95, $p$ = .051). The unweighted estimated mean ARS of the hitters was 4.90, which differs significantly from the midpoint of the scale (5), Stouffer $Z$ = 2.79, $p < .01$. The mean for missers was 4.98, which is very close to the midpoint and nonsignificant. For both groups combined, the estimated mean ARS was 4.94, yielding a significant Stouffer $Z$ of 2.76, $p < .01$.

Overall, the results suggest that when positive ESP scorers make an incorrect guess, they tend to miss with a word relatively similar in meaning to the target word. However, it is unclear whether this finding reflects ESP or a response bias. This issue can only be resolved by employing the statistical correction applied to the data of Rao et al. (1990) to the data from the five preceding experiments.

If the AR effect is real and a manifestation of ESP, it makes sense intuitively. ESP is clearly not an all-or-none process, and one would expect that at times it would only approximate reality. The finding would also indicate that the ESP stimulus is processed semantically before being reported.

### Can ESP stimuli influence memory?

The general hypothesis tested by the research discussed up to this point is that ESP is mediated by memory. In a sense, the research asks whether memory causes ESP. However, this question can be reversed. That is, we can ask if ESP causes memory or more precisely, can ESP enhance or inhibit recall, such as in the memory tasks used in the experiments above? The most direct way to test this hypothesis would be to have someone attempt to send a telepathic message to facilitate the memorization of some words in a list, while either attempting to hin-

should produce a positively skewed distribution of ESP scores. As the skew should not be as extreme in the Rao (1978) experiment, where the hit probability was .2, the estimated ratio of hitters to missers was halved for this experiment. Results of the Rao and Weiner (1982) experiment were particularly difficult to estimate, as only the $t$-test for missers and the various $n$s were reported. The averages of the *SD*s reported for the total sample and each subsample from the other experiments, minus Rao (1978) because of the different scale, were used as estimates of the corresponding values for Rao and Weiner. From the estimated *SD* and the $t$ for missers, the mean for missers could be estimated. Based on the quote from Rao et al. (1990 — see text), it was estimated that the means for hitters and missers were identical.

Table 2: Means and standard deviations of association rank scores in the Rao ESP/paired-associate experiments[a]

| | Hitters | | | | Missers | | | Total Sample | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Series | $N$ | $M$ | $SD$ | $t$ | $N$ | $M$ | $t_{diff}$ | $N$ | $M$ | $SD$ | $t$ |
| Rao et al., 1977 (Series 1) | *66* | 4.85 | *.546* | 2.43 | *84* | 5.04 | 2.52 | 150 | *4.96* | .479 | *1.02* |
| Rao et al., 1977 (Series 2) | 50 | 4.88 | .598 | 1.42 | 68 | 4.96 | .80 | 118 | 4.93 | .543 | 1.40 |
| Rao et al., 1977a (Series 1) | *33* | 5.01 | *.672* | 0 | *43* | 5.02 | .11 | 76 | *5.02* | .589 | *-.29* |
| Rao, 1978 | *32* | [4.94] | *.326* | .57 | *38* | [5.08] | 1.07 | 70 | *[5.01]* | .286 | *-.29* |
| Rao & Weiner, 1982 | 29 | *4.88* | *.603* | 1.10 | 42 | *4.88* | *0* | 71 | *4.88* | *.529* | *1.87* |
| Rao et al., 1990 | 44 | 4.83 | .529 | 1.38 | 48 | 4.84 | 0.28 | 92 | 4.84 | .506 | 3.05 |
| *Total* | *2.54* | *4.90*[b] | — | *2.79*[c] | 318 | *4.98*[b] | *1.95*[c] | 572 | *4.94*[b] | — | *2.76*[c] |

[a]Estimated values are in italics. Bracketed means are double the reported values to make them comparable to the other means in the table. The midpoint of the ARS scale was only half as large in Rao (1978) compared to the other studies, as the response alternatives were five instead of 10 (see text).

[b]Unweighted mean

[c]Stouffer $Z$

der the recall of other words or simply not to try to influence memory of these words at all. No one has attempted such an experiment with proper controls. We could also ask a somewhat different question. Can the mere presence of ESP stimuli help or hinder the memory process? This more passive approach to the issue has been investigated in several experiments, to which I will now turn.

*The Stanford experiment*

The first of these experiments was conducted by the American psychologist Rex Stanford (1970). Stanford asked 30 volunteer participants to listen to a taped dream report, ostensibly to test whether they could make correct inferences about the dreamer's personality from their dreams. To support this cover story, participants were subsequently given a psychological test (Q-Sort) in which they were asked to classify a set of personality descriptors as applying or not applying to the dreamer. Citing the need to be sure the participant remembered the dream well enough to properly access it for the personality assessment, Stanford then gave each participant a 20-item quiz asking them questions about the dream. Eight of these 20 items had four response alternatives and definite answers based on the dream content. Unbeknownst to the participants, Stanford randomly assigned ESP-correct answers for each of these eight questions. On average, the ESP-correct and memory-correct answers would be identical close to 25% of the time. The main research question was this: In cases where the memory-correct answers and the ESP-correct answers differed, would the presence of the latter guide participants' recall away from the memory-correct answers and toward the ESP-correct answers? Two analyses can be cited in support of this hypothesis, both of which are based on the 32 times in which the subjects gave memory-incorrect answer to one of the eight key questions. Stanford called these answers 'counter-story responses'. First, 31 of the 32 counter-story responses (97%) were from cases where the ESP-correct and memory-correct answers differed, compared to the 75% expected by chance, corrected $\chi^2_{(1)}$ = 7.04, $p < .01$[4]. Second, 15 of these 32 counter-story responses corresponded to the ESP-correct target ($p <$ .01). Thus, in these cases, not only did the ESP-correct answer in effect 'pull' participants away from the memory-correct answer 97% of the time, thereby creating inaccurate recall, but in 47% of these cases the alternative 'pulled to' was the ESP target.

[4]I calculated this chi-square based on figures cited in the report.

*The Kreiman and Schmeidler experiments*

The same general hypothesis was subsequently tested in two experiments by the Argentine psychologist Noam Kreiman (1978, 1980). Participants were students in Kreiman's parapsychology classes. The specific hypothesis was that the presence of ESP stimuli would influence the successful recall of words only if the words were difficult to memorize; easy to memorize words, Kreiman theorized, would not require a boost from ESP to be recalled correctly, and thus no ESP effect would be revealed for such words. Participants were given 5 minutes to memorize a list of 50 words, 20 of which were randomly selected as ESP targets. Easy-to-recall words were defined as the first half of those recalled and difficult-to-recall words as the second half. The ESP score was the proportion of recalled words that also were target words. Because 20 of the 50 words were chosen as targets, the mean score expected by chance is .4. Confirmation of the hypothesis required a significantly higher score for the words remembered in the second half of the recall list than in the first half. Such confirmation was obtained in only the first of the two experiments (Kreiman, 1978). Several independent attempts to replicate the original finding, some incorporating small changes in methodology, produced no significant confirmations (Schmeidler, 1980, 1981; Weiner & Haight, 1980).

The American psychologist Gertrude Schmeidler studied the results of all these studies, as well as some unpublished pilot data of her own, and developed therefrom a refined hypothesis that she attempted to confirm in three new experimental series (Schmeidler, 1983).[5] Participants were college students and acquaintances of the testers. There were two main changes from Kreiman's procedure. First, the prediction of success was restricted to those participants who expressed *moderate* belief in the possibility of ESP occurring in the experiment and who were in a positive mood at the time of testing. As mood questions are susceptible to biased answers (so-called 'demand characteristics'), Schmeidler measured mood indirectly by asking two questions concerning how 'lucky' participants felt they were in general. A positive answer to both questions was required to enter the experimental group. The second change was to compare results from the first half of the recall list to results from the last *quarter* of the list, dropping results from the third quarter. Her three series, which were conducted by

[5]The first series was also reported in Schmeidler (1981).

different testers, were identical in methodology, except that only in Series 1 and 3 were participants told explicitly that ESP could help them remember the target words, a list of which was provided to each participant inside a sealed, opaque envelope in all three series. In each series, testing continued until 16 participants, the same $N$ as used by Kreiman, were obtained for the experimental group.

The hypothesis was significantly confirmed in Series 1 and 3, and a clear trend in the predicted direction was found in Series 2 (see Table 3). Combining these results yielded a Stouffer $Z$ of 3.65 ($p < .001$) in strong support of the hypothesis of more positive ESP scores in the last quarter of the recall list compared to the first half. The mean ESP score of .503 in the last quarter was significantly positive (Stouffer $Z$ = 2.66, $p <$ .01) and the mean of .372 in the 1st half was nonsignificantly negative (Stouffer $Z$ = 1.48). Schmeidler also reported the results of participants who did not qualify for the experimental group. These results are also summarized in Table 3, sans $t$-tests. The mean ESP scores were .390 and .369 for the first half and last quarter respectively. These values are both in the negative direction and most likely not significantly different from chance. The difference between these two means is clearly nonsignificant. The entire ensemble of means shows that the hypothesized effect is attributable to strong positive scoring by the experimental group in the last quartile of the recall list.

Table 3: Proportions of correctly recalled words that were ESP targets in the first half and last quarter of the recall task for experimental and control participants for Schmeidler's (1983) study

| | *Experimental Group* | | | | | | *Control Groups* | | |
|---|---|---|---|---|---|---|---|---|---|
| | | First Half | | Last Quarter | | | | First Half | Last Quarter |
| Series | $n$ | $M$ | $t$ | $M$ | $t$ | $t_{diff}$ | $n$ | $M$ | $M$ |
| Series 1 | 16 | .36 | 2.12 | .51 | 1.71 | 2.66 | 30 | .353 | .337 |
| Series 2 | 16 | .355 | 0.44 | .49 | 0.97 | 1.18 | 27 | .402 | .315 |
| Series 3 | 16 | .40 | 0 | .51 | 1.93 | 2.48 | 28 | .415 | .456 |
| *Total* | *48* | *.372*[a] | *1.48*[b] | *.503*[a] | *2.66*[b] | *3.65*[b] | *85* | *.390*[a] | *.369*[b] |

[a]Unweighted mean
[b]Stouffer $Z$

*The academic exam paradigm*

Several experimenters have attempted to influence student's memory of test material by providing hidden answers to questions on an

exam. This technique was thought to produce especially high motivation in the participants, because the scores on the influenced items contributed to their course grade.

The first of these experiments consisted of three series (Johnson, 1973). In each series, from 35 to 37 students in a clinical psychology course took an 8 item short-answer essay exam on the course material. Answers to four of these questions were provided on sheets of paper wrapped in aluminium foil and placed in sealed envelopes, such that the answers lay directly underneath the corresponding questions on the exam sheet. I will call these the "key" questions. In the first two series the hidden answers to the key questions were correct and in Series 3 they were incorrect. It is unclear from the report whether the questions randomly chosen as key questions varied from student to student, or whether only the locations of the key questions were varied. The persons who scored the test were blind to which were the key questions. In Series 1 students scored significantly better on the key questions than on the others, and in Series 2 suggestively so. In Series 3, students scored significantly worse on the key questions than on the others. The combined results of the three series yielded a Stouffer $Z$ of 3.63, $p$ = .0003.

The second example of this paradigm was reported by Braud (1975). Forty-six students in a parapsychology course took a 3-part examination, 22 in a pilot series and 24 in a confirmatory series. The crucial second part consisted of 14 short-answer essay questions. The 14 questions were divided into two sets of seven (A and B) judged to be of equal difficulty. Half of the test packets contained answers to Set A questions wrapped in aluminium foil and sealed inside an envelope, on top of which was stapled the test sheet. The other packets included answers to Set B questions. The packets were then shuffled and distributed to the students. The tests were scored blind by the experimenter. The error scores were significantly lower for the key questions than for the others in each series ($p < .05$), confirming the hypothesis. Part three of the exam was a 4-choice clairvoyance test that produced neither a significant overall score nor a significant correlation with results from the hidden ESP test.

A third attempt to confirm the hypothesis was reported by Schechter (1977), who conducted three series. In Series 1, 26 students took a 12 item exam, in Series 2, 182 students took a 30-item exam, and in Series 3, 24 students took a 30-item exam. All items were multiple-choice. The test sheets were affixed to envelopes, which for half the

students contained concealed sheets with correct answers to odd numbered questions and for the other half correct answers to even numbered questions. Scoring of the tests was blind. In all three experiments, students scored higher on questions to which hidden answers were supplied than on the control questions. These trends were significant in Series 1 ($p < .05$) and Series 2 ($p < .005$). A Stouffer $Z$ calculated from the probabilities of the three hypothesis tests is 3.01, $p$ = .003. However, the fact that in Series 1 and 2 there were significant differences in the overall scores between students who received odd and even answers raises the possibility that the distribution of packets to students was biased. For example, in Series 1 the students who received odd answers might have been brighter than students who received even answers, thereby confirming the hypothesis artifactually (Schechter & Stanford, 1978).

Finally, a failure to obtain the hypothesized effect was reported by Willis, Duncan, and Udolfia (1974), who gave a 35-item multiple choice exam to 204 and 147 students in two psychology classes. Half the students in each class received correct answers to the first six items on a sheet concealed inside an envelope. For the remaining students the sheet was blank. It is not reported if scoring was blind. Unlike in the other studies, the students were told that "the test arrangement would facilitate scoring".

Taken collectively, the experiments reviewed in this section support the hypothesis that the presence of ESP target stimuli influences the extrasensory apprehension of these stimuli. In the Kreiman and Schmeidler experiments and those employing the academic exam paradigm, the salience of these targets was enhanced by putting them inside a sealed envelope given to the subjects, a procedure also used by Kanthamani and by Rao in their experiments described in previous sections. However, Stanford (1970) confirmed his hypothesis without resorting to such measures. It is also worthy of note that in Stanford's experiment, as well as in the four experiments representing the academic exam paradigm, the participants were unaware that they were being tested for ESP. Thus, such knowledge does not appear necessary for the effect to occur.

It is also noteworthy that Schmeidler could only obtain the predicted results with moderate believers in the paranormal who were in a positive mood at the time of testing. No such constraints were required in the other experiments reviewed in this section, or for that matter, in any other of the successful experiments reported in this paper. On the

other hand, it might be that results from these experiments would have been even stronger had they restricted their experimental samples as Schmeidler did. Finally, earlier in the paper I suggested positive mood as a possible mediating factor in the success of the Kanthamani experiments. This finding could be a further indication of the importance of mood in ESP testing.

### The correlation between memory and ESP scores

Recall that the purpose of the Feather (1967) experiment, described at the beginning of this paper, was to test for a positive correlation between scores on a memory test and a related ESP test. In this experiment, the correlation was found to be significantly positive. Some of the other experimenters reviewed above also examined their data to see if this relationship held. The results of these analyses provide a decidedly mixed picture. Rao et al. (1977) reported that a significant positive correlation between ESP and memory was found in Confirmatory Series 1 of Kanthamani and Rao (1974). Rao (1978) also reported a significant positive correlation between memory and ESP with paired associates. On the other hand, significant negative correlations were reported by Rao et al. (1977) in their second series, and by Parker (1976) in her first series using the digit-span test. Nonsignificant relationships were reported by Rao et al. (1977) in their first series and Parker (1976) in her second series. Data regarding this relationship are not available from the other experiments reviewed so far in this paper. The results are summarized in Table 4.

Table 4: Correlations between total memory and ESP scores

| Series | $N$ | $r$ | $p$ |
|---|---|---|---|
| Feather, 1967 | 30 | .455 (Av./3) | $<.02$ |
| Kanthamani & Rao, 1974 (Series 1) | 20 | .284 | $<.01$ |
| Parker, 1976 (Series 1) | 40 | -.347 | $<.05$ |
| Parker, 1976 (Series 2) | 40 | .107 | *n.s* |
| Rao et al., 1977 (Series 1) | 150 | .04 (Av./8) | *n.s* |
| Rao et al., 1977 (Series 2) | 118 | -.18 | $<.05$ |
| Rao, 1978 | 93 | .18 | $<.10$ |
| Kreiman, 1978 | 16 | -.54 | $<.02$ |
| Weiner & Haight, 1980 (Series 1) | 31 | ? | *n.s* |
| Weiner & Haight, 1980 (Series 2) | 31 | -.48 | $<.01$ |

*The Irwin analyses*

The Australian psychologist Harvey Irwin (1979) attempted to reconcile these contradictory findings by employing the well-known distinction between primary and secondary memory. Primary, or short-term, memory occurs immediately after a stimulus is recognized but lasts a maximum of about 20 seconds, provided no attempts are made to memorize it, a process called type 1 rehearsal. If such rehearsal occurs, the duration of the primary memory can be extended. Primary memories can be recalled with little additional cognitive processing. Some primary memories become permanently stored in secondary, or long-term, memory. Only at this stage are they processed semantically. Retrieval of memory traces from secondary memory is also more complicated than from primary memory.

Memory experiments often employ 'interference' tasks between the learning and attempted recall of stimulus material. The effect of such interference tasks is to block the prolongation of primary memories. Thus, tasks that employ effective interference procedures are more likely to be tapping secondary memory, whereas those that do not are more likely to be tapping primary memory, because they allow participants to engage in type 1 rehearsal.

Irwin interpreted Roll (1966) as intending his theory to apply to secondary memory. Thus the positive memory/ESP relationship should occur only if secondary memory is tested. On the other hand, the theory has no implications for a possible relationship between ESP and primary memory.

Irwin noted that the experiments reporting significantly positive memory/ESP tasks utilized interference tasks that would effectively block the preservation of primary memories. In Feather's (1967) experiment, this task consisted of 2–3 runs of ESP card guessing. In the Kanthamani and Rao (1974) experiment, the interference task consisted of listening to instructions about the next phase of the experiment, which the authors described as lasting 2–3 minutes. Parker (1976) also employed an interference task in her digit-span experiment — asking participants to name an example of each of two collective nouns — but it lasted only a few seconds, not long enough to affect the primary memory traces. Thus, Irwin concluded that her experiment, in which a negative ESP/memory correlation was found, measured primary memory.

The relevant experiments of Rao and colleagues (Rao et al., 1977;

Rao, 1978) gave mixed and contradictory results. These studies employed no interference task at all; recall followed immediately after the attempt to memorize the paired associates. Irwin argued that with such a design, whether the test measured primary or secondary memory would depend on how many participants chose to engage in type 1 rehearsal near the end of the learning phase. It is noteworthy that these two experiments sampled different participant populations. The study providing the negative correlation (Rao et al., 1977) tested exclusively high school students, whereas the study providing a positive correlation (Rao, 1978) used a more variable group of participants, attendees to a lecture. It is reasonable to assume that the lecture audience was more positively motivated than the high-school students, who were a 'captive audience'. If so, the lecture audience may have been more likely to engage in type 1 rehearsal, which would preserve primary memory.

*Additional experiments and findings*

Two of the experiments discussed under the previous subheading also examined the memory/ESP correlation, but they were published too late to be included in Irwin's review (Kreiman, 1978, Weiner & Haight, 1980). In the Kreiman experiment and in one of the two series reported by Weiner and Haight, there was a significant negative correlation between memory and ESP scores. The procedure was similar to the experiments of Rao et al. (1977) and Rao (1978), in that the recall task followed the learning task immediately, with no intervening interference. Recall that these are the studies for which Irwin declined to make a 'prediction'. With regard to my additional speculation about the difference in participant samples, those tested by Kreiman and Weiner/Haight were more comparable to the sample tested by Rao (1978), which yielded a positive correlation between memory and ESP, contrary to my speculation. Although he employed a much different memory/ESP task than the other authors, Stanford (1970) reported that his participants who scored above the mean on a test of 'incidental memory' he developed 'scored significantly better' on counter-story responses than did below average scorers, but only on those items that had definite correct answers. Stanford concluded from this comparison that "memory *per se* is not related to ESP performance; its effect is an indirect one in terms of its relation to response bias" (p. 177), a theme explored more fully in his experiment to be described below.

Beginning with the second of their reports testing association ranks

(Rao et al., 1977a), Rao and colleagues introduced the hypothesis that the mean ARSs on the memory and ESP trials should be positively correlated. This can be considered a further test of the generic hypothesis that ESP and memory scores should correlate positively. Whereas for the analyses described above the correlation was based on correct guesses, the present analyses test the hypothesis with respect to near misses. Rao et al (1977a) reported a significant positive relation between the association rank scores of ESP and memory misses in support of their hypothesis by a sign test ($p$ = .052). The Rao (1978) experiment provided a non-significant result in the predicted direction, this time based on a Spearman correlation of .15.

*Relation to the memory-trace theory*

Irwin concluded, based on the experiments he reviewed, that those testing secondary memory provided evidence of a positive memory/ESP relationship in support of Roll's theory. He has no explanation for why ESP should correlate negatively with primary memory.

Perhaps the most noteworthy feature of the experiments reviewed in this section is the relatively large proportion that yielded significant correlations in one direction or the other, even allowing for the likelihood that the correlation was nonsignificant in the experiments in which it was not reported. Another possible interpretation of the results is that the ESP/memory relationship applies to both primary and secondary memory, but for some unknown reason in some experimental circumstances the ESP of participants with good memory skills manifests as psi-missing, thereby yielding a negative ESP/memory correlation. Roll (1966) discussed psi-missing in his paper and considered it to fall within the rubric of his theory. He compared psi-missing to 'parapraxes' (e.g., slips of the tongue) and considered both to be caused by a motivation to avoid the correct response. Thus, the memory trace activated is a wrong one, but participants with good memory skills would be more likely to have these incorrect memory traces come into consciousness than would participants with poorer memory skills.

## ESP and primary responses in a word association test: The Stanford experiments

Stanford (1973) tested Roll's theory using a word association experiment that ironically did not include a memory test. He noted that "[Roll's] theory implies that frequently reinforced memory traces more

often serve as vehicles for ESP than less frequently reinforced ones" and, more specifically, that "responses in a situation likely to elicit ESP should more often be psi-mediated when those responses depend upon well-established associative connections…" (Stanford, 1973, p. 150). This, incidentally, is the direct opposite of the inference from Roll's theory made by Kanthamani and Rao (1975), who proposed that ESP scores should be more positive with low-association paired associates, because Roll's theory implies that "formation of associative habits inhibits the function of ESP" (Kanthamani & Rao, 1975, p. 2).

Stanford gave 60 volunteer participants a 66-item word association test, 36 items of which were used to test the memory trace hypothesis. Participants were asked to spontaneously respond to each word in the list with the first word that came to mind within 4–8 seconds. For example, if the stimulus word was 'cat', the participant might respond with 'dog'. Each word in the 36-word sublist had a distinctive primary (most common) and secondary (second most common) response according to population norms. For each of these 36 words, either the primary or the secondary response was randomly selected as the ESP target. A co-experimenter in another room attempted to 'send' the targets telepathically to the participant. The prediction based on Roll's theory was that participants should choose the ESP target a greater proportion of the time (more ESP 'hits') when they made primary responses than when they made secondary responses, under the assumption that the memory traces underlying primary responses have been more frequently reinforced in the past. Stanford added that this conclusion would apply regardless of how frequently the primary responses were chosen in this particular experiment. The prediction was significantly confirmed ($p <$ .025).

A complication arose from the fact that Stanford (1973) also proposed a seemingly contradictory hypothesis, based on the empirically supported proposition that participants who have a strong tendency to avoid a particular target alternative obtain a relatively high percentage of ESP hits when they do make such a response (Carpenter, 1977). The hypothesis seems to contradict Roll's theory in that these counter-bias responses are the ones that presumably have not been reinforced in the past. Stanford found a significant positive correlation between the number of primary responses on the 36-word list and the relative proportion of ESP hits on primary as opposed to secondary responses ($p <$ .03, one-tailed), thus confirming the response-bias hypothesis.

Stanford attempted to reconcile these two disparate findings by proposing that greater ESP hitting on primary vs secondary responses only applies for those participants who lack a strong bias in favor of giving the primary response. He confirmed this proposition by noting a significant negative correlation between the number of primary responses and the relative ESP hit rate on these responses ($p < .025$).

An attempted replication of Stanford (1973) by Stanford & Schroeter (1978) failed to yield enough secondary responses to test Roll's hypothesis by the principal method used in the first experiment. However, a secondary measure called 'sensitivity to primary targets', which Stanford and Schroeter reported as approaching significance in the predicted direction in the Stanford (1973) experiment ($p = .11$), again approached significance in the replication ($p = .10$, one-tailed). The response bias hypothesis failed confirmation in the replication attempt.

*Relation to the memory-trace theory*

Roll (1966) in fact devoted considerable attention in his paper to the response bias effect, which he labelled as 'associative habits', and he considered it to fall within the framework of his theory. In line with Stanford, he suggested that "other memory traces associated with the former [memory trace] will also be evoked, not because of their relationship to the ESP stimulus but because of their relationship to the first memory trace" (p. 508). Thus, if a participant has a strong bias to make a certain response, that bias will drown out the ESP stimulus as a response determinant and no ESP effect should be observed. On the other hand, participants who lack such biases would be assumed to be sufficiently spontaneous in their responding to allow an ESP-correct primary response to be activated by the ESP stimulus. This appears to be the same set of interpretations that Stanford (1970) had in mind to explain his data, although he did not present them as fully as I do here.

Nonetheless, Stanford's (1973) data in principle challenges the implication from Roll's theory that only strongly reinforced memory traces can be vehicles for ESP. This challenge can be answered for this experiment by noting that even secondary word-association responses are strongly reinforced in the culture, although not as strongly as primary responses. However, on the surface this conclusion does not appear to apply to sequential guessing tasks commonly employed in forced-choice ESP testing and from which most of the support for the response bias hypothesis comes. Research has consistently shown that in such sit-

uations participants have a strong bias to avoid calling the same symbol or number twice in a row (Wiegersma, 1982). It would seem therefore that such a response sequence has not been strongly reinforced. However, this is not necessarily the case. Roll (1966) defined what he meant by strongly reinforced when he stated that "we expect ESP responses to be expressed in terms of memory traces that are recent, frequent, and vivid" (p. 507). The key word here is 'recent', because the most recent relevant memory trace a participant has available in a sequential guessing task is his perception of his response on the immediately preceding trial. So insofar as recency plays a role in defining reinforcement, repetition of one's last response has some claim to being positively reinforced. But, if this is the case, why are participants in fact biased against such repetitions? Perhaps the first impression they get of the next target actually is the previous target, but this image is very quickly suppressed because of the common (but mistaken) belief that targets don't commonly repeat in a random sequence. I am not aware of any research testing this speculation.

## Summary and conclusions

This paper has reviewed several sets of experiments directed at specific questions about the relationship between ESP and memory. The following statistically significant relationships have emerged from pooling the results of the members of these sets of experiments:

1. Participants score above chance on ESP test items linked to correctly remembered paired associates and below chance on ESP items linked to incorrectly remembered paired associates.

2. When participants score above chance on an ESP test in which they are asked to guess the second word of a pair of words to which they had not been exposed, incorrect guesses tend to be similar in meaning to the correct choice.

3. Memory can be influenced by the presence of relevant ESP target material, at least among participants who believe moderately in ESP and are in a positive mood at the time of testing.

4. There is a linear relationship between total scores on an ESP test and a related test of memory, but the direction of the relationship is inconsistent. A positive relation tends to emerge when secondary

memory is tested and a negative relationship when primary memory is tested.

5. In a word association test, persons with strong biases to respond with primary associates score better on a related ESP test when they give secondary responses, whereas persons with less or no such biases score better on the ESP test when they give primary responses.

These conclusions must be treated with some caution however, primarily because for the most part they are based on consistent results obtained by just one investigator. In some cases there have been no independent replication attempts of the work of the investigator whose repeated series form the backbone of the evidence for the effect ([2] and [5]), and in [2] the results may reflect response bias, not psi. Such replications have been attempted in two cases ([1] and [3]). The results were not encouraging in [1]. They were somewhat more encouraging in [3], where the independent replications occurred with respect to the academic exam paradigm. However, two of these replications could have been artifactual, and the one experiment not conducted or supervised by a parapsychologist was unsuccessful. The case for [4] (and to a limited extent [3]) is built on results from a wider range of investigators using different methodologies, but [4] is the case where the results are most inconsistent and difficult to interpret. At this point all the results should be considered as suggestive, but they are promising enough to encourage further research, especially by independent investigators.

All the groups of experiments except [4] were designed to test Roll's memory trace theory, although the [1] experiments did not in fact do so. In all cases, the results can be taken as supporting the theory. However, this support can only be considered as strong as the evidentiality of these various results, which, as I have just noted, must be viewed with some circumspection. Moreover, the tests of the theory have been quite indirect. It would be more convincing if it could be shown more incisively that ESP responses actually are memories as opposed to direct representations of the ESP stimuli. Furthermore, sensory stimuli do not arrive in our consciousness as such direct representations; they undergo various kinds of cognitive processing that include interactions with memories. Thus, memory almost certainly plays a role in determining the conscious ESP imagery or thought, even if that cognition originated with some sort of quasi-sensory percept.

## References


Braud, W. G. (1975). Conscious vs. unconscious clairvoyance in the context of an academic examination. *Journal of Parapsychology, 39,* 277-288.

Carpenter, J. C. (1977). Intrasubject and subject-agent effects in ESP experiments. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 202-272). New York: Van Nostrand Reinhold.

Feather, S. R. (1967). A quantitative comparison of memory and ESP. *Journal of Parapsychology, 31,* 93-98.

Gambale, J. (1976). Word frequency and association strength in memory-ESP interaction: A failure to replicate [abstract]. *Journal of Parapsychology, 40,* 339-340.

Gambale, J., Margolis, F., & Cruci, K. (1976). The relationship between ESP and memory — an attempted replication with modifications [abstract]. *Journal of Parapsychology, 40,* 340.

Harary, S. B. (1976). A study of psi, memory, and expectancy [abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 121-126). Metuchen, NJ: Scarecrow Press.

Irwin, H. J. (1979). On directional inconsistency in the correlation between ESP and memory. *Journal of Parapsychology, 43,* 32-39.

Johnson, M. (1973). A new technique of testing ESP in a real-life, high-motivational context. *Journal of Parapsychology, 37,* 210-217.

Kanthamani, H., & Rao, H. H. (1974). A study of memory-ESP relationships using linguistic forms. *Journal of Parapsychology, 38,* 286-300.

Kanthamani, H., & Rao, H. H. (1975). The role of association strength in memory-ESP interaction. *Journal of Parapsychology, 39,* 1-11.

Kreiman, N. (1978). Memoria y precognicion [Memory and precognition]. *Quadernos de parapsicologia, 11*(2), 3-17.

Kreiman, N. (1980). Memoria secundaria y ESP [Secondary memory and ESP]. *Quadernos de parapsicologia, 13*(3), 3-12.

Lieberman, R. (1976). Role of varied time interval and association strength in memory-ESP interaction for group and individual testing [abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 126-129). Metuchen, NJ: Scarecrow Press.

O'Brien, D. P. (1976). Recall and recognition processes in a memory-ESP paired-associate task [abstract]. *Journal of Parapsychology, 40,* 57-58.

Palmer, J. (1997). The challenge of experimenter psi. *European Journal of Parapsychology, 13,* 110-125.

Parker, K. (1976). A study of immediate memory and ESP [abstract]. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 130-134). Metuchen, NJ: Scarecrow Press.

Rao, K. R. (1978). Further studies of memory and ESP. *Journal of Parapsychology, 42,* 167-178.

Rao, K.R., & Kanthamani, H. (1989). Explaining normal-paranormal interaction within a memory-ESP testing paradigm. *Proceedings of Presented Papers: The Parapsychological Association 32nd Annual Convention*, 158-166.

Rao, K.R., Kanthamani, H., & Palmer, J. (1990). Exploring normal-paranormal interaction within a memory-ESP testing paradigm. *Journal of Parapsychology, 54,* 245-259.

Rao, K. R., Morrison, M., & Davis, J. W. (1977). Paired-associates recall and ESP: A

study of memory and psi-missing. *Journal of Parapsychology, 41,* 165-189.
Rao, K. R., Morrison, M., Davis, J. W., & Freeman, J. A. (1977a). The roll of association in memory-recall and ESP. *Journal of Parapsychology, 41,* 192-197.
Rao, K. R., & Weiner, D. H. (1982). Memory and ESP: An attempted replication [abstract]. *Journal of Parapsychology, 46,* 56.
Roll, W. G. (1966). ESP and memory. *International Journal of Neuropsychiatry, 2,* 505-521.
Schechter, E. I. (1977). Nonintentional ESP: A review and replication. *Journal of the American Society for Psychical Research, 71,* 337-374.
Schechter, E. I., & Stanford, R. G. (1978). Correspondence: On the interpretation of some reported evidence for nonintentional ESP. *Journal of the American Society for Psychical Research, 72,* 289-291.
Schmeidler, G. R. (1980). Does ESP influence the recall of partially learned words? [abstract]. In W. G. Roll (Ed.), *Research in parapsychology 1979* (pp. 54-57). Metuchen, NJ: Scarecrow Press.
Schmeidler, G. R. (1981). ESP and memory: Support for Kreiman's summary hypothesis [abstract]. In W. G. Roll (Ed.), *Research in parapsychology 1980* (pp. 118-120). Metuchen, NJ: Scarecrow Press.
Schmeidler, G. R. (1983). ESP and memory: Some limiting conditions. *Parapsychological Journal of South Africa, 4,* 51-69.
Stanford, R. G. (1970). Extrasensory effects upon "memory". *Journal of the American Society for Psychical Research, 64,* 161-186.
Stanford, R. G. (1973). Extrasensory effects upon associative processes in a directed free-response task. *Journal of the American Society for Psychical Research, 67,* 147-190.
Stanford, R. G., & Palmer, J. (1972). Some statistical considerations concerning process-oriented research in parapsychology. *Journal of the American Society for Psychical Research, 66,* 166-179.
Stanford, R. G., & Schroeter, W. (1978). Extrasensory effects upon associative processes in a directed free-response task: An attempted replication and extension [abstract]. In W. G. Roll (Ed.), *Research in parapsychology 1977* (pp. 52-54). Metuchen, NJ: Scarecrow Press.
Tyrrell, G. N. M. (1946-1949). The modus operandi of paranormal cognition. *Proceedings of the Society for Psychical Research, 48,* 65.
Weiner, D. H., & Haight, J. (1980). Psi within a test of memory: A partial replication [abstract]. In W. G. Roll (Ed.), *Research in parapsychology 1979* (pp. 52-53). Metuchen, NJ: Scarecrow Press.
White, R. A. (1977). The influence of experimenter motivation, attitudes and methods of handling subjects in psi test results. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 273-301). New York: Van Nostrand Reinhold.
Wiegersma, S. (1982). Can repetition avoidance in randomisation be explained by randomness concepts? *Psychological Research, 44,* 189-198.
Willis, J., Duncan, J., & Udolfia, J. (1974). ESP in the classroom: Failure to replicate. *Psychological Reports, 35,* 582.

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# Making Sense of the "Extrasensory" — Modeling Receptive Psi Using Memory-Related Concepts

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**Abstract**

*Although the physics of psi-mediated information acquisition has not been scientifically elucidated, both spontaneous-case reports and laboratory psi research suggest that some unknown ("psi") process preconsciously activates extant knowledge structures (i.e., constructs, schemata, and their associative linkages, including those to affective and behavioral response). Such activation may thereby elicit overt response. The nature and extent of prior learning relative to specific informational inputs is proposed to determine whether, in what way(s), and in what degree this preconscious activation may initiate behavioral response, including adaptive (or even maladaptive) response. It is proposed that preconsciously-mediated response to psi information is typical and that, contrary to popular assumption, conscious awareness of the triggering information may be the exceptional case. The specifically psychological assumptions of this model of preconscious, potentially adaptive, receptive-psi influence would seem to derive support from cognitive psychologists' findings on preconscious sensory influences, including subliminal ones. The findings on preconscious sensory priming provide suggestions of how preconscious psi functioning might be more effectively conceptualized and studied. Additionally, laboratory findings from psi research are reviewed that suggest how receptive psi interacts with memory-related structures and processes.*

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## Introduction

"Receptive psi" is said to have occurred when the organism acquires information other than through known sensory processes. The term *receptive psi* may be deemed preferable to the more traditional one, *extrasensory perception* (or ESP) because it does not bias the consideration of these events by suggesting something analogous to the conscious experience associated with sensory-perceptual function, which seems inappropriate. My own theoretical position is that receptive psi is not an intrinsically perceptual function but is, instead, a mechanism that can support adaptive response to preconscious psi-mediated information. This implies that organisms can respond to psi information in an adaptive fashion without the necessity of developing conscious cognitions or perceptions relative to the information to which they are responding. Studies of subliminal and of otherwise-preconscious (e.g., unattended) *sensory* stimulation provide evidence of psychological and even adaptive behavioral response to preconscious information, including the creation of goals (see, e.g., Chartrand & Bargh, 1996). Such effects often may occur through the activation of automatic, pre-established associative connections, often concerned with evaluation or affect, which can elicit behavioral dispositions in current situations (e.g., Ferguson, Bargh, & Nayak, 2005). The evidence of adaptive response to preconscious sensory information indicates the existence of cognitive mechanisms that also might support adaptive preconscious response to extrasensory information. If so, contemporary research on adaptive response to preconscious sensory information may usefully inform the investigation of receptive psi. Subsequently, this paper will address how psi information about a given situation (or object) may activate that situation's (or that object's) stimulus-response associations in memory, thereby releasing the associated response, potentially for adaptive purposes, but it also will address how psi-activated response tendencies sometimes may have to interfere with or block prior response inclinations in order to produce an adaptive outcome.

## A real-life example

The following incident involved my wife and me (for a fuller account, see Stanford, 1982). It illustrates how preconscious information may have an adaptive influence on behavior in everyday life. I do not know whether the information that apparently elicited the following

experience was subtle, unattended, preconscious sensory information or whether this incident involved, alternatively or additionally, information conveyed by psi. Late one evening we were driving eastward along a major expressway in the Borough of Queens, New York City, heading home from an operatic performance. We had soon to turn off that route onto a southbound expressway in order to get home quickly. Although the time was close to midnight, shortly before I was to turn toward the southbound route, I felt a very powerful urge. The urge was to ignore my former driving plan and to continue eastward to look for ducks and geese on a bay along this more easterly route. (We are avid bird-watchers, hence the possibility of this sudden desire.) Just as I was about ready to yield to that urge, I realized how bizarre and inappropriate it really was. How does one study distant waterfowl in darkness? It might even be impossible to spot them. Following the irrational impulse would have involved getting home tired and later than we had wished, after, quite possibly, seeing no waterfowl at all. Having contemplated for a few moments the irrationality of this impulse, it seemed so peculiar that I believed it might have been prompted by implicit (i.e., preconscious) knowledge of some kind of danger ahead on our planned and otherwise optimal route. I had published (1974) a paper detailing a theory of unconscious, adaptive, psi-mediated behavior. What was happening seemed very much like the sudden, impulse-driven responding that I had discussed in that paper as capable of mediating adaptive response to preconscious need-relevant information, including psi-created information. I now felt so certain that my odd impulse was a response to implicit knowledge, quite possibly of an extrasensory character, that I told my wife about the impulse and my conclusion that it might mean there was danger ahead on the originally intended route. I proposed that, despite my urge, I would, in order to test my hypothesis, very cautiously follow the originally planned route to determine if there truly was a dangerous circumstance present there. If so, it might help to explain my strange impulse to deviate from the most appropriate route at what seemed a most inappropriate time. We therefore took the originally planned southbound expressway and exited, slowly and cautiously, at our usual exit only to find, a very few feet ahead of the exit, lying across much of the access road, a lamp pole that had just been knocked down by another vehicle. There also was considerable glass in the road. Had I gone this route and exited at my usual pace at that late hour with little traffic, I might easily have run my vehicle into or across

the fallen pole, damaging it or its wheels and tires and possibly injuring us. My special caution had obviated a potentially bad mishap.

I do not tell this story to prove a point but to illustrate a proposed theoretical principle of which it may be an example: Preconscious knowledge, whether sensory[1] or psi in character, may be presumed to activate previously learned response dispositions related to the implicit knowledge. I knew, ahead of time, both the eastbound and southbound routes home and knew about the ducks and geese that frequent the bay along the eastbound route. We had even, on previous trips, deviated, during daylight hours, from the shorter southbound route because of the possibility of seeing ducks and geese along the eastbound route. Implicit knowledge of danger on the planned route might, therefore, automatically have activated existing knowledge (i.e., a schema) of the alternate route, including its possible attractions. Interestingly, had this impulse struck us during daylight hours, it would not have seemed irrational, I never would have inferred that psi (or other preconscious knowledge) might have been the cause of the impulse, and I probably would have taken the eastbound route home, happily scanning the bay for waterfowl and not objecting to the longer route. This suggests how subtle and fully unconscious may be the adaptive response to preconscious knowledge, at least under suitable circumstances.

Elsewhere I have proposed a detailed model for adaptive response to preconscious psi-mediated (and, also, preconscious sensory) information (Stanford 1990), but full details of that model are beyond the scope of this discussion. The model does, though, assume that information entering the system in preconscious form by an unknown means labeled "psi" (or the psi process), activates related, preexisting knowledge structures, thereby priming response dispositions (e.g., goals or affect) that are habitually related to such knowledge. These dispositions, then, potentially can mediate adaptive response to that information without there being conscious awareness of it.

Any testable idea must indicate boundary conditions for effects, and this conceptual model of psi function makes a number of very specific assumptions about the psychological circumstances that should in-

[1]The preconscious sensory possibility involved in this impulse-to-see-birds incident derives from the possibility that the lamp pole's being knocked over, at nighttime, about 2.5 miles from my car's location might have caused either a flash (or some other signal) to which I paid no attention but that elicited in me preconscious apprehension. At the time I certainly had no experience of having noticed a flash in that direction, but if there had been an unattended one visible at my considerable distance from the accident, it might have had a preconscious effect upon me.

hibit or block the influence of psi upon behavior. These important assumptions about boundary conditions are beyond the scope of this paper, but they and related research have been discussed in detail elsewhere (Stanford, 1990).

This model of psi function has been termed the psi-mediated instrumental response model (PMIR model). That model proposes in some detail how the subtle, but potentially powerful, influence of receptive-psi information (and, also, preconscious sensory information) may manifest in everyday life, based on preconscious, automatic activation of preexisting cognitive structures.

## Theories and research on preconscious sensory priming

The idea of adaptive response mediated by subtle information (psi or sensory) is very plausible in light of contemporary developments in cognitive psychology. Recent theoretical and related empirical developments in cognitive psychology have demonstrated that there exists preconscious sensory priming (i.e., activation) of *goals*, of *evaluations*, and of *perception-related ideas* (such as social stereotypes or trait inference) (see, e.g., the review by Bargh & Chartrand, 1999; see, also, Bargh, 1997) without the individual's becoming aware of the process of this happening. Hence, no attention is devoted to it. Preconscious priming appears to be of short duration, but it can influence mental activity and even overt behavioral response for a brief period of time. The ideas and research just cited involve, of course, subtle sensory, not extrasensory effects. John Bargh has been one of the leading figures in conceptualizing the role of priming-related automaticity in everyday life, and many of the findings that emerge from his (and his students') laboratory work provide evidence for easily triggered, automatic mechanisms that can subserve even some fairly high-level mental functions. This recent evidence for preconscious sensory influences documents the existence of the very kinds of mechanisms that I long ago (Stanford, 1974, 1990) proposed could accomplish the proposed adaptive influences of preconscious psi information. This is the case, at least, if we are willing to assume that the preconscious priming of mental structures that occurs sensorially also can occur due to psi influence. The non-psi theorization and research of a number of investigators has provided substantial experimental evidence that internal, automatic mechanisms exist that

can subserve adaptive needs, mechanisms that have, in a number of instances, shown to be triggered even by extremely subtle, including presumably subliminal, stimulation. This makes it the more plausible that these memory-related mechanisms also may be utilized by psi influence.

As Bargh and Chartrand (1999) noted, some of the automatic mechanisms they have posited do not require prior experience to function, whereas others do require it. Mechanisms that presumably do not require learning include the tendency to mimic behaviors that we perceive in others. On the other hand, many other automatic influences depend upon the individual's learned associations that develop and are regularly used or accessed during life. Some of these learned associations are evoked by social stimuli and some by other kinds of situations. Those involving social stimuli have been more extensively studied by psychologists, especially social psychologists. Information that triggers learned associations can support the automatic activation of goals and motives, if a goal or a motive has earlier become associated with the situation or with the object whose internal representation is being preconsciously (or, of course, consciously) activated. These generalizations have, of course, been developed from studies of sensory priming. The psychologists who have demonstrated them have not studied psi processes.

*Evidence of preconscious sensory activation of learned associations*

Preconscious activation of existing mental structures can activate response dispositions or even activate specific goals. Bargh, Raymond, Pryor, and Strack (1995) examined the hypothesis that men likely to sexually harass women have a strong, pre-existing association between power and sex, unlike men who are unlikely to engage in such behavior. In such a case even subliminal or other subtle sensory cues related to power may activate an inclination toward sexual relations with a woman who is close at hand. The hypothesis of Bargh et al. was that in such men power automatically primes the idea of having sex. In one study they demonstrated that men, known from other evidence to be disposed to sexually harass or to sexually aggress, exhibited automatic associations between power and sex, as assessed by a *subliminal priming methodology*. This automatic association of power and sex was not in evidence with men who were not dispositionally inclined toward sexual harassment. In another study these investigators very subtly primed the

idea of power (and contrasted this with a neutral priming condition). Men likely to sexually aggress, as determined by independent evidence, and who had been subtly primed with the idea of power were more attracted to a female confederate who was present at the time than when the prime was neutral. Men who had been exposed to the same power prime but who were not likely to sexually aggress as pre-determined by other evidence failed to show this facilitation of attraction on account of the power prime. *The fundamental message here is that in individuals with pre-existing strong associations between two constructs, even preconsciously activating one construct can activate the other.* Therefore, a situation, including one subtly or unconsciously primed, can activate an associatively related disposition or goal, and that activation may eventuate in actual behavior.

As a more socially desirable manifestation of preconscious priming, consider the fact that priming a sense of socially-based security by subliminally presenting names of personal attachment figures (e.g., the name of a parent), as contrasted with subliminally presenting the names of close relatives or acquaintances who are not attachment-related, fosters compassion and the readiness to help someone who needs help (Mikulincer, Shaver, Gillath, & Nitzberg, 2005). These findings were observed in both Israeli and USA samples. This research, interestingly, shows the value of preconscious stimuli being custom-selected as personally relevant. Personal attachment figures have been associated in one's life with safety and security. They are presumed by attachment theorists to become incorporated into a *secure base schema*. Subliminal presentation of the name of the significant other may, Mikulincer and colleagues reasoned, produce a sense of personal safety and security that supports altruistic caring that is threatened neither by empathically experiencing others' suffering nor by the level of interdependence that can be engendered by caregiving. I personally wonder if this security-figure-priming-yields-compassion effect might be mediated, partially or alternatively, by entirely unconscious activation of an inclination to abide by the social norms that the named attachment figure had taught the individual in earlier years. Nonetheless, the general principle of eliciting previously learned inclinations by preconsciously activating past associations would still explain these findings, and that is the main point here, not to advocate for or against a specific application of attachment theory.

Another of the methodologically sophisticated subliminal-priming

studies of Mikulincer and colleagues illustrates a different facet of personal-association-based priming. Mikulincer, Gillath, and Shaver (2002) demonstrated that a subliminal prime with possible consequences for self-esteem (e.g., *failure* in Hebrew, *nichshal*) can selectively enhance the accessibility of the mental representation of personal attachment-relevant individuals. Presumably it is the learned tendency of think of attachment figures when one feels threatened that underlies this finding. Once again we see the use of personalistic information — often primes or, sometimes, as here, as targets of priming — as having great value in the study of preconscious processes. I strongly suggest the potential value of these personalistic strategies in the study of receptive psi. It comes back to tapping into the associations that the individual has personally learned that relate to feeling, reacting, and striving.

*Preconscious activation of affect- and goal-relevant dispositions*

The research and writings of Bargh and colleagues have strongly emphasized preconsciously apprehended circumstances and unconscious processing in the development of feelings, judgments, and behavior, including goal development (Bargh & Chartrand, 1999). From an evolutionary perspective and in light of contemporary research (see later citations) it makes sense to think that preconscious processing of unattended or weakly-presented sensory information might be particularly attuned to getting the individual ready to respond dispositionally, in terms of approach or avoidance (i.e., positive or negative reaction), to related circumstances that subsequently might be encountered in a less equivocal sensory way. This possibility seems compatible with the outcomes of some recent studies involving subliminal affective priming, including those now to be considered.

Ferguson et al. (2005) showed that subliminal stimulation with affectively positive or negative primes (i.e., nouns with pleasant or unpleasant connotations) can, immediately afterward, influence: (a) the interpretation of ambiguous words (i.e., homographs with positive and negative meanings; Experiment 1); (b) the categorization[2] both of affectively ambivalent social objects (persons or employment categories) and of affectively ambiguous nonsocial objects (Experiment 2); and (c)

[2]This was not spontaneously-generated categorization. Participants first saw an object displayed on a computer screen and then saw two affectively-distinct category names applicable to that object. They were asked to select the categorization that seemed more applicable to the object and to do so as quickly as possible. They were informed that their responses would be timed. This discouraged rumination.

the tendency to emphasize positive or negative trait judgments[3] of people shown in photographs (Experiment 3). In all three experiments the expected priming effect was observed even though the sole shared element between the primes and the subsequent targets was affect. The absence of shared semantic attributes between the primes and the targets of priming did not prevent affective priming.

Ferguson et al. advanced the intriguing suggestion that preconsciously-driven affective interpretations of target objects might become encoded into memory and thus affect future reactions and decisions related to those objects. These investigators' bold suggestion of potentially long-term consequences of such priming is novel and worthy of investigation. Many cognitive psychologists believe that the consequences of priming tend to be very short-lived. Of course, preconscious affective priming could have very adaptive significance in the "real world," even if its effects should turn out to be exclusively short lived. Given that the organism seems built to support this kind of priming, evolutionary considerations suggest that it likely has adaptive roles to play outside the laboratory. This adaptive significance could derive, in major degree, from getting the organism inclined, on the basis of "early information" (i.e., weak or very transient), for a response such as flight/fight or approach. As was noted earlier, in real-world settings preconscious affect-related priming may often occur due to brief or inadequate sensory exposure to an object or circumstance of known affective relevance due to previous encounter(s). The cognitive meanings of these objects or circumstances may never enter conscious awareness, but their potential to elicit appropriate affect or an adaptation-relevant response disposition may nonetheless be very real.

**Preconscious priming without conscious affect?:** Preconsciously-evoked affect and dispositions, as contrasted with priming consequences of a more cognitive character, may, because of their action-relevance, have special importance in relation to adaptation. This should not, however, be taken to imply that the consequences of affect-related preconscious primes — and perhaps, especially, their initial consequences — need always be conscious. Indeed, contemporary research is providing evidence that those early consequences of

[3]These were not spontaneously-generated traits. Participants used an 8-point scale to rate the applicability of a named trait. They were asked to give their first response and to decide quickly because their responses would be timed.

affect-relevant preconscious stimulation may at least sometimes be nonconscious (Winkielman & Berridge, 2004; Winkielman, Berridge, & Wilbarger, 2005; Winkielman, Zajonc, & Schwarz, 1997). The unconscious, automatic character of early response to preconscious, affect-relevant inputs may be important in adaptive response, for their speed and automaticity presumably help to get the organism ready, in timely fashion, for adaptive response. This may happen even before there is an opportunity for consciousness to intervene in this adaptation-relevant response. This is not, however, to suggest that such early, preconsciously primed affect-related influences always will be unconscious. Research has not as yet illuminated the boundary conditions for their being unconscious.

Let us briefly consider some of evidence suggestive of the possibility that consciously-perceived affect may not always accompany successful subliminal affective priming. In two studies by Winkielman et al. (1997), despite evidence of subliminal affective priming, participants reported themselves to be unaware of feelings being elicited by the primes, and some ancillary evidence would seem to have supported the interpretation of there being a lack of awareness of feelings associated with the affective primes. Although these authors were unable on the basis of their studies to decide between some competing theoretical interpretations of their data, they were able to conclude that future consideration should be of models of affective priming that do not imply, and hence do not require, mediation of the observed effects by consciously experienced feelings. Two more recent studies by Winkielman et al. (2005) may be deemed compatible with that conclusion. They examined three conceptually interesting predictions about subliminal affective priming. This work involved happy, neutral, and angry faces as subliminal primes. It examined predicted effects of the affective priming upon the pouring, consuming, and affective response to a novel sweetened drink. Mood and arousal also were assessed. These investigators had predicted that (a) the affective priming would be most effective on measures (e.g., amount consumed and desirability of the drink) that reflect the drink's pleasure-related qualities; (b) the influence of the affective priming would be accomplished without changing consciously perceived mood; and (c) the effect of a prime, be that prime happy or angry, upon behavior would be greatest when the participant was thirsty. The two types of primes were expected to have opposite effects, which they did, namely enhanced hedonic value of the drink with

positive primes and reduced hedonic value when the prime was negative. Those effects were, as expected, moderated by degree of thirst. (Discussion later will return to the potential conceptual significance of this moderation phenomenon for a central thesis of the present paper.) Perhaps of immediate special interest here is the fact that no evidence was found that the affective priming influenced any of the numerous measured attributes of subjective experience. These observations are, at least, compatible with the supposition that the affective consequences of the primes were unconscious.

**Can preconscious priming effect behavioral changes?:** The Winkielman et al. (2005) work may have special relevance for the central theme of this paper, namely that preconscious activation of mental representations can eventuate in actual changes in behavior, not just changes in arousal or in internal experience. In the Winkielman et al. (2005) work, preconscious affective priming resulted in changes in overt, motivated behavior, namely the drinking of flavored, sweetened water. My PMIR model, which describes a theory of adaptive response to preconscious psi-mediated activation, requires that the effects of preconscious activation go beyond just internal consequences. It requires that preconscious activation must be able to generate behavior (or changes in behavior) that allow either approach to a favorable event or outcome or avoidance of an unfavorable event or outcome. The fact that the Winkielman et al. (2005) paper, which used subliminal affective priming, showed clear behavioral consequences of primes may be deemed evidence — from non-psi research, of course — that the psychological machinery exists that can mediate unconscious effects on actual need-related behavior. In fact, a number of receptive-psi studies have produced evidence that effects such as this can occur via psi activation (reviewed by Stanford, 1990). My PMIR model, which has been the major impetus for this kind of work, made the assumption that the kinds of psychological (non-psi) processing occur in the organism that could support adaptive, preconscious psi-mediated response. Contemporary findings from cognitive psychology are supplying substantial evidence that the mechanisms needed for automatic, nonintentional, preconsciously-driven — but potentially adaptive — response exist, questions of the reality of psi all aside.

There is substantial evidence that affect- or evaluation-related con-

sequences of stimuli may have their own, privileged channel of processing that does not depend upon cognitive evaluation of the affective relevance of the stimulus and that, indeed, may be elicited while a stimulus is still preconscious (for a brief review and related references, see Zajonc, 2001; also, Zajonc, 1980, for a fuller view of this theory). Again, this makes sense, adaptively speaking, given the need for early and rapid determination of the potential of objects relative to personal threat or benefit.

*Adaptive response and the ongoing needs and goals of the organism*

The PMIR model of adaptive psi-mediated response to preconscious information posits that the ongoing needs of the organism must somehow interact with and influence response to incoming information, including preconsciously apprehended information. To go back to my personal anecdote early in this paper, my wife and I were driving home late at night and planned on taking a short route to get home early. It would appear that when I (apparently) became influenced by preconscious knowledge of the accident up the planned route, the influence of that knowledge emerged, not as an entirely new goal and certainly not as any direct knowledge of the danger, but as a goal that included getting home while enjoying birds along an alternate route as we traveled home (albeit by a somewhat longer route). This was possible because I had stored in memory a schema (mental map) of the area that indicated alternate routes and their known consequences.

Both psychologists and parapsychologists — indeed, all who wish to make sense of adaptive response to preconscious, implicit knowledge — will have to consider how this kind of subtle priming can interact with the ongoing goals and needs of the organism. It therefore was with considerable pleasure that I read the previously cited studies by Winkielman et al. (2005). These investigators broke important ground by examining, guided by theory, how the individual's motivation (level of reported thirst) interacted with the affective-priming manipulation (angry, neutral, and happy faces). This kind of non-psi research provides psychological evidence for the interaction of preconscious activation with ongoing needs to influence behavioral outcomes. Evidence for this kind of psychological functioning provides a degree of support for an underlying assumption of the PMIR model, which requires that preconscious affect-related activation somehow interact with the ongoing needs of the organism in order for adaptive behavioral response to

occur. Of course, much more research is needed on this important matter, both by psychologists and by psi researchers interested in the potentially adaptive, unconscious functions of psi. It is worth noting that this problem area represents an instance in which psi researchers might have the opportunity both to contribute to cutting-edge psychological knowledge and to psi-related knowledge. Indeed, additional psychological knowledge of this area is a prerequisite for advancing psi research (of this particular kind) and for refining existing models (e.g., the PMIR model). That kind of multi-disciplinary contribution is not to be despised!

*Elicited responses can attenuate or block other responses*

Of substantial pragmatic and theoretical interest is the finding by Mikulincer and Shaver (2001) that subliminal priming of the secure base schema (i.e., using primes that all relate to socially-derived security or closeness) can reduce the normal tendency to react negatively to out-group individuals, even when one's self-sense has been affronted by an experience of failure, something that can enhance out-group discrimination (e.g., Fein & Spencer, 1997). The security-and-closeness primes, which are intended to evoke the feelings that one experienced with an attachment figure, can be presumed to have helped in the self-affirmation process. (Having been the recipient of caring by an attachment figure presumably makes one feel that one must have been worthy of such care. That feeling presumably was activated by the security-and-closeness primes). Feeling positive about oneself, which is not the same as being in a good mood, can reduce the tendency toward discrimination against outgroups (Fein & Spencer, 1997). Very important, Mikulincer and Shaver showed that the effect of the secure base primes was not due to mood elicited by those primes. More generally, this study illustrates the very important point that priming can elicit previously learned responses that can modify or even obviate one's usual responses to present situations.

The latter is a conceptually important conclusion that bears upon a major assertion of my PMIR model. According to this model, psi can have adaptive effects either by directly fostering responses that themselves are adaptive or by eliciting competing responses that tend to counter or to block inclinations toward a response that would be non-adaptive, given the situation at hand. (That is presumed to have been the case in the go-see-the-birds impulse anecdote mentioned earlier).

Thus, if the results of the work of Mikulincer and colleagues (and other similar work in the non-psi literature) can be generalized to psi-related priming, they lend a degree of credibility to the proposal that psychological mechanisms exist whereby psi-mediated adaptive response might sometimes occur through preconscious psi-mediated facilitation of ready responses that counter potentially maladaptive responses that otherwise would have occurred.[4] The Mikulincer and Shaver (2001) work makes it clear that preconscious sensory priming can facilitate responses than can attenuate or block the appearance of otherwise-expected responses. This, of course, is an important development in the study of preconscious sensory priming, the relevance to the PMIR model aside.

This idea of adaptive psi-mediated response competition was directly investigated in a study that led up to my development of the PMIR model (Stanford, 1970). This study provided evidence, in a context that carefully controlled for sensory cuing, that psi information could block the recall of information that was clearly stated in a story heard shortly before by the research participants. I call this "psi incursion." The study had been set up such that responding in accord with psi information, rather than on the basis of actual memory would, on some trials, give the subject a presumably desired advantage.

## Summary and integration of conclusions to this point

The already well-documented conclusion that preconscious sensory priming can influence thought, affect, motivation, and goal-setting indicates that mechanisms exist that might respond similarly to psi-initiated preconscious information. If so, the possibility of adaptive preconscious psi-mediated response becomes more understandable and psychologically more plausible. Granted, then, the assumption that psi-mediated priming of memory structures is possible and should operate by laws similar to those observed with preconscious sensory priming, it may not be surprising that a number of the assumptions and implications of the psi-mediated instrumental response (or PMIR) model already have received some degree of support from studies of preconscious psi function (see Stanford, 1990), as well as from studies that involve a deliberate effort to consciously use receptive psi. Discussion

[4] I also have collected some additional spontaneous cases of possible PMIR that might be deemed to be of this sort.

shortly will turn to some of those efforts, for they, too, relate to memory functions, the topic of this conference.

The effort to generalize the findings of preconscious sensory priming to preconscious receptive psi might be very useful. It is evident that doing so will require psi investigators to more carefully study the memory structures (e.g., schemata and associative linkages) of their individual research participants, using techniques used for such purposes in non-psi research. The more information that can be gleaned about the individual participant's existing cognitive structures, including associative linkages, the better will be the chance, in my view, of using those cognitive structures to produce evidence of preconscious (or even conscious) receptive psi. Maximizing the results of studies with preconscious priming, either sensory or extrasensory, may be aided by (a) using stimuli that are individually meaningful to the participant and (b) learning something in advance about the nature of this meaningfulness and about the motivation and affect-related memories with which they are cognitively embedded.

The possibility of adaptive (or even potentially maladaptive) response to unattended or even subliminal sensory information was, up to very recently, considered by most experimental psychologists as something close to unthinkable, but it has found recent empirical support from studies of the preconscious priming of goals and affect. Many psychologists' views about such matters recently have changed, for two reasons (see, e.g., the review by Kihlstrom, 1987, in the journal *Science*): First, conceptual developments in cognitive psychology, related largely to ideas and work on procedural knowledge and on connectionist models (e.g., parallel distributed processing or PDP), have made it plausible that fairly sophisticated processing can occur without the need for attention to guide it. Second, experimental findings in cognitive psychology, including in cognitive social psychology, have shown that both goals and affect may be activated preconsciously. We need to apply these methods and principles to research on receptive psi, and, in fact, this kind of things has already been going on, albeit sporadically, for some years by several investigators, as reflected in lectures presented here by other participants and by the studies that I have reviewed in depth elsewhere (Stanford, 1990). Let us now see what studies of receptive psi seem to be telling us about the various roles of memory in receptive-psi function.

## Some roles of memory in spontaneous and laboratory psi

*Awareness of when receptive psi has occurred*

Unlike the sensory modalities, receptive psi function has no specific or unique qualitative representation in consciousness. What is consciously represented, if anything, appears to be the consequence of preconscious activation of aspects of cognitive structures that exist in memory (Roll, 1966; Stanford, 1974, 1990; Tyrrell, 1946). This surely is a major reason that it is extremely difficult for subjects in deliberate, conscious receptive-psi tasks to have reliable insight into the accuracy of their responses.

In a substantial proportion of spontaneous cases of potential receptive psi there is, nonetheless, a sense of conviction that an unexplained communication has occurred (Rhine, 1967, Ch. 5, "Belief in the Truth of ESP"). This sense of conviction may derive from an inference that one's prior knowledge and experiences cannot possibly have elicited the experience. This may be presumed to lead to the interpretation that psi influence has occurred. I know of many spontaneous experiences where this kind of attribution appeared to have played a role, and similar inferences on the part of subjects can occur in laboratory work in which they try to extrasensorially perceive a work of art or a photograph. Really unexpected experiences often seem to elicit the inference that psi might have produced the experience. Inferences aside, new information, including psi information, entering the mind may logically be expected to elicit unanticipated experiences or otherwise unexpected responses, precisely because new information is forthcoming. Hence, the relative novelty of response sometimes may be a marker of informational, including psi-informational, influence.

In line with this consideration, researchers often have found that in forced-choice receptive-psi tests, response choices that occur relatively infrequently are more likely to be correct responses. (Forced-choice ESP tasks involve the subject in indicating which of two or more alternative target categories is thought to be the target on a given trial.) There is substantial evidence that low-probability responses on receptive-psi tasks often show more statistical evidence of having been psi-mediated than do higher-probability response alternatives, and this seems to be the case whether or not the task requires the subject to intentionally try to use receptive psi (reviewed by Stanford, 1975). There have been many

reports in the literature that have supported this response-bias hypothesis, but there have been some inconsistencies (i.e., failures to find the effect). Also, these findings have been mostly correlational in character. Palmer has reviewed some of these response-bias findings earlier in the symposium. In what probably is the most systematic examination of this hypothesis, I (1970), in a study of preconscious, non-deliberate receptive psi, experimentally manipulated the likelihood of a subject making a counter-bias response. This was a study of how recognition memory might be modified by receptive psi, and the clarity of some information in the remembered story had been experimentally manipulated. When subjects' responses were contrary to information clearly stated in the story, those responses were significantly accurate. Those same responses also were significantly more accurate than counter-story responses related to something that had only been suggested in the story. The validity of the response bias hypothesis also was strongly supported by the fact that subjects shown by an independent test of incidental memory to have better incidental memory scored better on their counter-bias responses than did subjects who did less well on the same memory test. So far as I know, this experiment is the most detailed and conceptually incisive effort to examine the response-bias hypothesis. It provided strong, consistent, convergent evidence in support of its operation in a study of nonintentional or preconscious receptive psi, and support for the response-bias prediction has been reported by numerous other investigators, albeit generally in correlational contexts. I know of a single unsuccessful effort (Sheargold, 1972) to conceptually replicate some of my (1970) findings, but the results of his study should be considered in light of the study's methodology, which diverged from my own in some very fundamental ways (discussed in detail in Stanford, 1990).

Nonintentional, preconscious tests of receptive psi may, in theory, be among the best opportunities to observe this response-bias effect. In such tests, participants do not tend to become ruminative about their responses, whereas in deliberate (or explicit) tests of receptive psi, this rumination tends more strongly to occur and perhaps the more so in forced-choice tests. Participants in forced-choice receptive-psi tests often become very analytical and reflective (see next section on constraints upon spontaneity). As a consequence, what may appear to have been a low-probability response, based on earlier trials, could suddenly become a high-probability response if the subject decides "I have not

called enough of that target." This kind of thing could well dilute the response-bias effects that may be observed in tests that do not cause participants to become so strongly ruminative about the responses they are making.

As a consequence of these considerations, a relatively high success rate with a given target category in a receptive-psi task should not, by itself, be taken to imply that this target category is particularly communicable by psi (i.e., that someone has high psi-sensitivity to it). It may be, instead, that the psi influence on producing this response category is particularly detectable simply because responses of that kind are not diluted by false alarms, thanks to this response being relatively rarely produced.

It also is very important to note that because a response category has a relatively low probability in a given receptive-psi task situation does not mean that it is not a "ready response" in the sense of its being readily activated (e.g., easy to prime). Being readily activated and even having been primed does not mean that a given construct necessarily will control responding in a given situation at a given moment. Many other factors will influence response selection in a conscious, explicit receptive-psi task, such as presuppositions and pseudo-logical constraints (discussed in the next section). Additionally, competition from an even slightly more ready response category could reduce the probability of actual production of a given response, however easily activated that response (or response category) might be.

*Spontaneity is important to the expression of receptive psi*

Sometimes subjects in forced-choice receptive-psi tasks think about their earlier responses and constrain subsequent responses on the basis of earlier ones. For example, if they are guessing a series of twenty-five geometrical symbols randomly selected from five alternatives, they may tend to monitor how many of each they have called, not allowing themselves to call too many or too few of each. They may, thereby, tend effectively to balance their responses across the alternatives. This is, of course, imposing a seemingly logical constraint on their spontaneity on a given trial. There is substantial evidence in the receptive psi literature that imposing these kinds of pseudo-logical constraints on one's guessing behavior can prevent the subject's test scores from deviating far from chance average, whether in the positive or negative direction. Constraining the present response on the basis of one's memory of ear-

lier responses can therefore have adverse consequences for performance on receptive-psi tasks. I (1975) reviewed this evidence and found five studies that experimentally contrasted instructions to make calls independently and instructions that encouraged call balancing. Four of the five studies exhibited this effect. Additionally, there has been some correlational work that supports the conclusion that call balancing can interfere with the expression of receptive psi.

Freedom from call balancing may also allow the use of receptive psi in the case of outstanding receptive-psi subjects. For example, Morris (1972) reported that in an outstanding subject's very high-scoring series he generally made no attempt to balance his calls. The cognitive constraints produced by a tendency to balance calls should be most evident later in the test run where the subject tends to run out of options to select a given response. It is therefore interesting that in additional work with this same outstanding subject, Stump, W. G. Roll, and M. Roll (1970) noted that he in some series showed strong within-run declines in his receptive-psi performance and that in these same series he also showed a strong tendency to balance his calls. Conversely, in a series where the within-run decline was absent, the subject showed a decreased inclination to balance calls.

Another kind of rationalistic constraint that may, based on purely correlational (and, hence, causally inconclusive) evidence, interfere with receptive psi is that subjects often call a given target category far fewer times in a row than would be expected by chance. For example, they tend to call fewer "doubles," "triples," etc. (e.g., two or three stars in a row) than would occur by chance in the actual target sequence. This tendency appears to result from subjects' misconceptions about random sequences, for they would seem to act as though the recurrence of a given target category is unlikely on adjacent trials. They are engaging in a form of what is known as "the gambler's fallacy." Given that targets are randomly selected, this kind of pseudo-rationality could impair performance, potentially interfering with the expression of receptive psi. Morris (1972) noted that on high-scoring, as contrasted with chance-performance runs, an outstanding subject tended to call the same symbol twice in a row much more frequently. He performed more successfully when he was able to get away from this form of sequential constraint. Of course, this observation is both anecdotal and correlational, so conclusions about causation cannot be warranted from it, but it does suggest the potential value of experimentally manipulating this

particular form of rationalistic sequential constraint and observing its consequences, if any, for receptive-psi performance.

Numerous individuals known to have exhibited some evidence of receptive-psi ability have expressed to me the opinion that inclinations to constrain responding on the basis of things one knows (or believes one knows) tend to inhibit the free expression of receptive psi. It now seems clear on the basis of laboratory evidence that placing rationalistic constraints on how one responds in a forced-choice test, based on memory of earlier responses, can reduce the likelihood of producing statistical evidence of receptive psi. Although researchers have tended to focus on the adverse consequences of imposing rational constraints in forced-choice receptive-psi tasks, it seems reasonable that rational and other memory-related response constraints could limit the opportunities for receptive psi to act in a variety of other situations, including in free-response studies (e.g., trying to perceive pictures by psi), in experimental studies of non-intentional psi-mediated adaptive response, and in the operation of preconscious psi influences in everyday life (in the latter regard, see Stanford, 1990).

## Methodological considerations in modeling receptive psi in everyday life

*Relevance of target materials to the respondent*

As was suggested earlier, effective modeling of preconscious receptive psi influences might profitably involve using psi target materials (or circumstances) that have personal relevance, ideally, of a known kind, to the individual respondent (subject). Also, it may be very useful to employ dependent measure(s) that plausibly can be assumed to reflect response dispositions cognitively linked, in the mind of the individual respondent, to the target material (i.e., to "the preconscious psi prime"). Responses of that kind should therefore be easily activated by that target material. Selection of idiosyncratic materials as stimuli and expected idiosyncratic responses to them as dependent measures is presently being used in some non-psi research by psychologists who wish to study the role of preconscious influences in social life. One very important example of this is in the work of Susan M. Andersen. She has intensively studied unconsciously mediated cognitive and affective consequences, in social perceivers, of verbally-described characterological resemblances between significant others in their lives and new individ-

uals who share characteristics with those significant others. She terms this *transference* and regards it as a very important element in social cognition. This research is complex and of large scale, so no serious effort will be made to describe it here. Andersen assumes that transference requires neither conscious recollection of the significant other (in response to information about the new individual) nor even an attentional focus specifically on the cues shared by the new individual and the significant other. The veracity of her hypotheses about transference has been supported through her unique, but now standardized, methodologies, as well as through the use of subliminally exposed verbal descriptors (Glassman & Anderson, 1999). Anderson describes her general methodological approach as the "use of idiographic methods. . . in a nomothetic, experimental design" (p. 629). Her methodology is, as a consequence, a clever meld of two very different research traditions. It is one that might profit both psychologists and parapsychologists who study it. I find it particularly interesting in its strategies for controlling potential extraneous variables that may arise. A good overview of her program and of the social-cognitive theory that it has guided and been shaped by it may be had in a paper by Andersen and Chen (2002).

Applying idiosyncratic information in experimental paradigms obviously requires the acquisition of advance information about the psychology of the individual participant, which can inform the kinds of materials and measures one would use in one's study. It also requires thoughtful gathering and use of such information in ways that do not tip off respondents to one's expectations in the research.

One way to gain information that might be useful for modeling preconscious psi would be to get to know how the individual participant responds to preconscious sensory information of the kinds to be used in the extrasensory phase of the work. Perhaps one could start by selecting even the predictive preconscious sensory stimuli on the basis of interviews, perhaps structured ones, of the individual in regard to things, people, and issues of central importance in his or her life. Then one could, having used the pre-selected stimuli in a preconscious sensory paradigm, attempt to predict from its outcomes, the individual's extrasensory response to the same (or comparable) stimuli. This is just one off-the-cuff idea, and it is mentioned here perhaps more as an incentive to invention than as a strategy to guide immediate research.

In regard to this general approach, learning how an individual responds to preconscious (e.g., subliminal) information of the selected

kind(s) might be especially useful in predicting receptive-psi-task outcomes, given that preconsciously and consciously presented materials may elicit different outcomes (e.g., Cheesman & Merikle, 1986). This is not the place to elaborate at length on the intriguing possibilities in this approach, but some of them are discussed elsewhere (Stanford, 1990).

*Traditional research methodologies will require remodeling*

Research studies intended to illuminate relationships of memory functions and receptive psi have, in my view, been rather uneven in terms of the construct validity of their methods (Stanford, 1990). Indeed, in some cases the conceptual articulation of the problem to be addressed left considerable to be desired. If research methods are to be effective in illuminating how receptive psi acts upon information stored in memory, activates related associational links, and fosters adaptive (or, in some cases, maladaptive) response through such activation, the adopted methodologies may need, far more directly than in most of the existing studies, to model the situations in which adaptive psi influence might emerge in the real world. In that world, persons are not, with very few exceptions, trying to gain conscious extrasensory access to specific kinds of information. Historically, though, the vast majority of laboratory receptive-psi studies have involved efforts at explicit psi testing in a perceptual/cognitive paradigm. These are "use your psi" paradigms.

The result may be an overlaying of the ordinary milieu of receptive-psi function with the manifold cognitive and attentional consequences of, (a) trying to make psi do something at our deliberate bidding and, (b) directing it to access specific information, regardless of its relevance to the individual's needs. Perhaps investigation could profit by considering the possibility that cognitive/perceptual manifestations of psi may be atypical manifestations and should not be taken to indicate the intrinsic direction that receptive-psi events tend to take. In recent years a number of psi researchers appear to have shifted toward less purely cognitive/perceptual assumptions, and this has tended, in some degree, to change the methods employed. The adaptive significance of real-world receptive psi may be more a matter of acquiring valued outcomes or of avoiding undesirable ones than of fostering perception-like cognitions, unless the latter are necessary for such ends. I am reminded, in this regard, of Robert Morris' (1967) study of goldfish in a tank, one of which, on a given trial, would shortly be picked up and held momentarily in a net. The fish were scored for swimming activity for a certain

period and then a random selection was made of the one to be picked up. It tended to swim more than the others. Psi may function in support of action relative to real or potential adaptation- or survival-relevant circumstances.

One paradigm that would be useful for the investigation of possible preconscious adaptive psi is a situation in which an individual is not instructed to try to use psi but may be able, entirely unconsciously, to profit from it either to encounter an affectively positive situation or to avoid an affectively negative situation. This models real-world opportunities for adaptive-psi influences on behavior. This is the kind of paradigm that generally has been used in a number of studies of so-called psi-mediated instrumental response (PMIR; reviewed by Stanford, 1990).

Short of such an effort to study wholly unconscious psi-mediated adaptive response, one might use, at least, a paradigm in which someone is not deliberately trying to perceive information. An example would be work that I have done using word-association paradigms as vehicles for the expression of the possible role of receptive-psi in activating memory structures (e.g., constructs and schemata). In such a paradigm the participant may know that an effort may be made to transmit psi information to him or to her, but he or she is assured that it can happen as an automatic influence, one that will be expressed in the responses in the word-association task (Stanford, 1973). Other potentially useful paradigms include efforts to monitor physiological responses to information not sensorially exposed but that has relevance to the subject's needs, goals, or interests. A number of such studies by a variety of investigators have taken this approach with at least some degree of success.

Of course, there never is a single answer to what is a good methodology. The method adopted must be one suitable to address the question being asked. From this perspective, all the methods that have historically been used to study receptive psi have potential value. Even the often-despised forced-choice methods of old-fashioned receptive-psi research have some utility, but the potential usefulness of any method depends on the characteristics of the method itself. The forced-choice paradigm has serious limitations for addressing many questions of interest to psi researchers, but that is not to say it is without its uses. I have, for example, used it to demonstrate the consequences of rationalistic constraints on receptive-psi performance (as cited earlier). The

problem comes in getting stuck in a paradigm and never asking whether there may be better methods to address a specific research question.

The effort to study preconscious receptive psi might profit by using paradigms much closer to those used by cognitive psychologists to study effects of preconsciously presented information, and this trend already has begun. For such research to be maximally effective, investigators must acknowledge that methodologies, however technologically or statistically sophisticated they may be, are genuinely useful only when the work to be done with them is guided by serious conceptualization and careful planning. Conceptual development must accompany methodological sophistication and must inform the adoption and development of new methods.

Historically, the tendency to think of receptive psi as unquestionably — obviously and intrinsically — a perceptual function that struggles for conscious manifestation may have held back discovery of important facts about the events being studied. There are other ways to regard receptive-psi events, and it can be very useful deliberately to create alternative explanations or models for what we study, at least if those explanations are empirically testable by virtue of their making clear, distinctive predictions about what one should observe and under what circumstances. We have seen a number of such efforts in psi research. The failure to examine assumptions can hold investigators captive to wrong ideas and can seriously undermine scientific progress. Fundamentally important is to recognize that we do work from assumptions, however deeply embedded and implicit they may be, not from knowledge of reality. Sometimes implicit assumptions are not easy to recognize, but once they are recognized it becomes possible to create new visions of reality and to envision ways to test them.

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## References

Andersen, S. M., & Chen, S. (2002). The relational self: An interpersonal social-cognitive theory. *Psychological Review*, *109*, 619-645.

Bargh, J. A. (1997). The automaticity of everyday life. In R. S. Wyer, Jr. (Ed.), *The automaticity of everyday life: Advances in social cognition* (Vol.10, pp. 1-61). Mahwah, NJ: Erlbaum.

Bargh, J. A., & Chartrand, T. L. (1999). The unbearable automaticity of being. *American Psychologist*, *54*, 462-479.

Bargh, J. A., Raymond, P., Pryor, J. B., & Strack, F. (1995). Attractiveness of the underling: An automatic power → sex association and its consequences for sexual harassment and aggression. *Journal of Personality and Social Psychology*, *68*, 768-781.

Chartrand, T. L., & Bargh, J. A. (1996). Automatic activation of impression formation and memorization goals: Nonconscious goal priming reproduces effects of explicit task instructions. *Journal of Personality and Social Psychology*, *71*, 464-478.

Cheesman, J., & Merikle, P. M. (1986). Distinguishing conscious from unconscious perceptual processes. *Canadian Journal of Psychology*, *40*, 343-367.

Fein, S. & Spencer, S. J. (1997). Prejudice as self-image maintenance: Affirming the self through derogating others. *Journal of Personality and Social Psychology*, *73*, 31-44.

Ferguson, M. A., Bargh, J. A., & Nayak, D. A. (2005). After-affects: How automatic evaluations influence the interpretation of subsequent, unrelated stimuli. *Journal of Experimental Social Psychology*, *41*, 182-191.

Glassman, N. S., & Andersen, S. M. (1999). Activating transference without consciousness: Using significant-other representations to go beyond what is subliminally given. *Journal of Personality and Social Psychology*, *77*, 1146-1162.

Kihlstrom, J. F. (1987). The cognitive unconscious. *Science*, *237*, 1445-1452.

Mikulincer, M., Gillath, O., & Shaver, P. R. (2002). Activation of the attachment system in adulthood: Threat-related primes increase the accessibility of mental representations of attachment figures. *Journal of Personality and Social Psychology*, *83*, 881-895.

Mikulincer, M., & Shaver, P. R. (2001). Attachment theory and intergroup bias: Evidence that priming the secure base schema attenuates negative reactions to outgroups. *Journal of Personality and Social Psychology*, *81*, 97-115.

Mikulincer, M., Shaver, P. R., Gillath, O., & Nitzberg, R. A. (2005). Attachment, caregiving, and altruism: Boosting attachment security increases compassion and helping. *Journal of Personality and Social Psychology*, *89*, 817-839.

Morris, R. L. (1967). Some new techniques in animal psi research. *Journal of Parapsychology*, *31*, 316-317.

Morris, R. L. (1972). Guessing habits and ESP. In W. G. Roll, R. L. Morris, and J. D. Morris (Eds.), *Proceedings of the Parapsychological Association, Number 8*, (pp. 72-74). Durham, NC: The Parapsychological Association.

Rhine, L. E. (1967). *ESP in life and lab: Tracing hidden channels.* New York: The Macmillan Company.

Roll, W. G. (1966). ESP and memory. *International Journal of Neuropsychiatry*, *2*, 505-521.

Sheargold, R. K. (1972). Experiment in precognition. *Journal of the Society for Psychical Research*, *46*, 201-208.

Stanford, R. G. (1970). Extrasensory effects upon "memory." *Journal of the American Society for Psychical Research, 64*, 161-186.

Stanford, R. G. (1973). Extrasensory effects upon associative processes in a directed free-response task. *Journal of the American Society for Psychical Research, 67*, 147-

190.
Stanford, R. G. (1974). An experimentally testable model for spontaneous psi events: I. Extrasensory events. *Journal of the American Society for Psychical Research, 68*, 34-57.
Stanford, R. G. (1975). Response factors in extrasensory performance. *Journal of Communication, 25*, 153-161.
Stanford, R. G. (1982). An experimentally testable model for spontaneous extrasensory events. In I. Grattan-Guiness (Ed.), *Psychical research: A guide to its history, principles and practices* (pp. 195-205). Wellingborough, Northamptonshire, UK: Aquarian Press (for The Society for Psychical Research).
Stanford, R. G. (1990). An experimentally testable model for spontaneous psi events: A review of related evidence and concepts from parapsychology and other sciences. In S. Krippner (Ed.), *Advances in parapsychological research: Vol. 5* (pp. 54-167). Jefferson, NC: McFarland.
Stump, J. P., Roll, W. G., & Roll, M. (1970). Some exploratory forced-choice ESP experiments with Lalsingh Harribance. *Journal of the American Society for Psychical Research*, *74*, 421-431.
Tyrrell, G. N. M. (1946). The 'modus operandi' of paranormal cognition. *Proceedings of the Society for Psychical Research*, *48*, 65-120.
Winkielman, P., & Berridge, K. C. (2004). Unconscious emotion. *Current Directions in Psychological Science*, *13*, 120-123.
Winkielman, P., Berridge, K. C., & Wilbarger, J. L. (2005). Unconscious affective reactions to masked happy versus angry faces influence consumption behavior and judgments of value. *Personality and Social Psychology Bulletin*, *31*, 121-135.
Winkielman, P., Zajonc, R. B., Schwarz, N. (1997). Subliminal affective priming resists attributional interventions. *Cognition and Emotion*, *11*, 433-465.
Zajonc, R. B. (1980). Feeling and thinking: Preferences need no inferences. *American Psychologist*, *35*, 151-175.
Zajonc, R. B. (2001). Mere exposure: A gateway to the subliminal. *Current Directions in Psychological Science*, *10*, 224-228.

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# Why Do Ghosts Wear Clothes? — Examining the Role of Memory and Emotion in Anomalous Experiences

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**Abstract**

*The likely involvement of memory in mediating the anomalous information of psychic experiences into consciousness was first recognized by early psychical researchers. Later models by Roll (1966) and Irwin (1979) strengthened the case that the experiential component of a psychic experience was based on visual memory images. Recent research on the emotional system has revealed the close links between that and memory, especially the role of feelings in determining the memory images that occupy conscious attention, particularly when making decisions or planning action. The integration of the fight-or-flight capabilities of the emotional system with cognition and the large data storage capabilities of memory has conveyed enormous evolutionary advantage and it is argued that if evolution has developed a means of capitalizing on anomalous information, it is likely to have incorporated this within the emotional system. Examples from the three main classes of spontaneous cases (dreams, intuitive experiences, and hallucinations) are used to illustrate the joint role of emotion and memory in "naturally occurring" psychic experiences*

## Introduction

If I were to say that I had seen a ghost, I think most people could imagine what my experience was like. I would have encountered an entity that was not a living human being, but might have been at one time.

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But what did that entity look like? Does the pale, shapeless, shroud-draped figure of illustrations and cartoons spring to mind? If it does, that would be far from what most people who claim to have seen a ghost typically report.

Consider this example from a Canon Phillips in 1963. Shortly after the death of C.S. Lewis, he had this experience:

> "But the late C.S. Lewis, whom I did not know very well, and had only seen in the flesh once, but with whom I had corresponded a fair amount, gave me an unusual experience. A few days after his death, while I was watching television, he 'appeared' sitting in a chair within a few feet of me, and spoke a few words which were particularly relevant to the difficult circumstances through which I was passing. He was ruddier in complexion than ever, grinning all over his face and, as the old-fashioned saying has it, positively glowing with health."

Canon Phillips had a second similar encounter with the ghost of C. S. Lewis, and later he was asked by two investigators from the Society for Psychical Research to describe the scene in more detail. He reported:

> "On both occasions C.S. Lewis was dressed in rather rough, well-worn tweeds, brown in colour. This did not strike me as remarkable at the time until I realized some weeks later that I had never seen him in ordinary clothes."
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> (MacKenzie & Goldney, 1970)

The fact is that in most cases the "ghost" is clearly recognizable as a person, and in many cases it is a person who is known to the one who sees the ghost. Often the ghost is a loved one, but sometimes they represent historical figures, or complete strangers.

The popular interpretation of ghosts has traditionally been that the ghost is the spirit of a dead person who has returned for a visit, and many still believe that today. Almost as soon as the study of these experiences became systematized with the formation of the Society for Psychical Research in the late 19th century investigators began to see problems with this interpretation. The possibility of an immaterial soul or spirit continuing beyond bodily death was not a problem and, in fact, was in keeping with prevailing religious belief. The problem was that the ghosts were wearing clothes, and sometimes bearing objects. The

idea that one's very material clothing also passed on to the afterlife did not give way to any convincing explanation within the traditional religious concept of survival. Often the clothing that the ghost appeared in was what the deceased customarily wore, not necessarily those in which the person died. Are there "valet angels" in heaven who will fetch one's favourite clothes?

A more serious problem arose as the early investigators began to amass a large collection of reports of ghostly apparitions that were, for all practical purposes, like other ghost experiences, except that the person in the apparition was not dead. This led early SPR researchers such as Edmund Gurney to conclude that these experiences were hallucinations mediated by telepathy (Gurney, Myers, & Podmore, 1886). F.W.H. Myers largely agreed with the idea that the experience was hallucinatory, but he proposed that the departed spirit interacted with the mind of the person experiencing the apparition in order to bring it about (Myers, 1903).

Thus from the earliest days of psychical research there was an awareness, if not a consensus, that classic ghost experiences were essentially a product of the mind of the percipient — an hallucination composed of images taken or constructed from the experiencer's memory. How the hallucination was triggered remained a mystery, and, of course, a subject of much discussion to this day.

It is not primarily my intention to discuss ghosts and apparitions beyond using them to illustrate that very quickly the psychic experience was being deconstructed into its constituent parts, and one of those parts was our memory store of images. What makes psychic experiences interesting for science is that they involve an apparently anomalous transfer of information. Hallucinations are of psychological interest of themselves, but when an hallucination carries information that alerts a person of a tragedy or provides other information that we are confident was neither sensed or deduced, then it presents a different challenge to science.

A simple way of looking at psychic experiences involves three main components:

1. A trigger event. Usually this is of personal significance to the experiencer, but not always.

2. A method of "transmission". I put the word transmission in quotes because I do not want to suggest a radio model. It is precisely this

point that makes the paranormal just that. What could possibly carry information from great distances with no apparent medium, or, worse yet, bring information from the future? My opinion is that this is a problem to be solved by physics, but at this point we can say little more than that this is an unknown linkage between the event and the experience.[1]

3. The recipient. This, of course, is the person who receives the information about the event. I use the term recipient rather than percipient to avoid pre-judging the issue on whether something is being perceived in a manner analogous to sense perception. In the recipient, the process probably involves two stages: (1) How the anomalous information enters the system, and (2) How the human system turns the input into useful information that can change behaviour or the content of conscious awareness.[2]

It is with this third component — the recipient — that some glimmer of understanding is beginning to emerge. Irwin (Irwin, 1999) refers to this as the experiential phase of psi, and he draws a distinction between two different approaches to understanding this phase. One approach is a sensory-like approach (or "pseudo-sensory" in Irwin's terminology) which is embodied in the popular notion of a "sixth sense". In contrast to this are models of psychic experience based on memory. Although some investigators have proposed models based on sensory processing (e.g. Schmeidler, 1991) Irwin's thorough analysis of spontaneous psychic experiences and laboratory research (Irwin, 1979) lead him to the conclusion that "...the available evidence does not encourage a view that extrasensory information receives the same sorts of processing as sensory input" (Irwin, 1999, p. 166). The typical characteristics of sensory stimuli, e.g., its discriminability, have no systematic effect on performance in ESP tests. In the cases of spontaneous ESP, apart from waking hallucinations, which are the least common and have only an apparent sensory input, the anomalous information enters consciousness without any clues to how it got there. In all cases of normal sensory experience one can reflect upon the process by which the information arrived through the senses, but one cannot for psychic information.

[1]This is not to suggest that there are no theories of what might be happening. Indeed there are several (see Stokes, 1987) but all remain speculative.

[2]Parapsychological researchers commonly speak of a generally accepted "two-stage" model for receptive psychic experiences (extrasensory perception or ESP) which is simply the second and third items in this list.

While sensory models are not ruled out, Irwin believes that the weight of the evidence points toward memory-based models that involve what he calls "ideational" processing. In these models, the bulk of the information processed in a psychic experience comes *not* from a source external to the receiver, but from within the receiver.

I am in general agreement with Irwin's assessment, but I take a slightly different perspective on the input issue. While I agree that the pseudo-sensory model has not been helpful, we still have to recognize that there is an input of some sort. I think a better way of characterizing the issue is to consider whether the anomalous input is a high bandwidth channel that can convey a great quantity of new information, or a low bandwidth channel that carries only a very little information, but sufficient information to activate the memory systems needed to "get the job done". Although we have no way of measuring the channel capacity, the evidence suggests that component images and ideas of a psychic experience arise from within the system.

**Memory models of Extra Sensory Perception.**

The role of memory in the psychic process was recognized to a greater or lesser degree by the earliest investigators, first in the case of hallucinations, and then more broadly across the entire range of psychic experiences. In the 1930s René Warcollier observed, "We must admit... that the images which appear to the mind of the percipient under the form of hallucinations, dreams, or more or less well-formed images, spring exclusively from *his own mind,* from his own conscious or subconscious memory. *There is no carrying of visual impressions from the agent to the percipient...*" (Warcollier, 1939, p. 133, italics in original).

William Roll (1966) was the first to develop a comprehensive memory theory of extrasensory perception. Drawing on observations of spontaneous case researchers Roll argued that psychic experiences of extrasensory perception (ESP) consist of *revived* memories. In Roll's model there is an anomalous input or trigger, but after that the psychic experience is based on the contents of existing memory. Roll draws a contrast between ordinary sensory input, in which new input is processed by comparing it against existing memory data, and the extrasensory experience, which arises solely from memory. Once evoked, the ESP-triggered memories are subject to the same mental processing that ordinary memories are (Roll, 1966).

The most fully elaborated memory model of ESP has been developed by Harvey Irwin (1979). Building upon Roll's work, Irwin acknowledges that while neither the spontaneous cases nor the experimental research establishes it conclusively, the case for ESP being wholly based on memory is very strong.

One of the obvious characteristics of ESP information is that it is new to the recipient. Irwin notes, however, that this does not mean that memory not is involved. A psychic experience that informs you that a good friend has just died obviously could not activate a memory of your friend dead, but would likely activate networks of memories involved with your friend, and death in general, and perhaps the manner in which he died. ESP activates and links the discrete components that give rise to new information by their conjunction. This sort of experience is often accompanied by the distinct awareness that the ESP-triggered memory images are suddenly intruding in mental activity that was directed elsewhere.

If this conjecture is correct, then it can account for the fact that ESP information is often frustratingly incomplete. The recipient simply may not have the memories needed to complete the picture, or perhaps for whatever reason, the ESP trigger is unable to cause some links to be made.

Investigators who have used the experimental technique known as the ganzfeld have often seen this memory process in action. In a ganzfeld experiment the subject is placed in a state of mild sensory isolation and asked to describe the images that parade across consciousness while someone in another location attempts to "send" an image. Afterwards the subject judges a set of pictures to identify the one that was most like his or her imagery. Often the technique produces very striking hits, but the actual images that the participant reports are drawn from his or her past experience. When those memory images approximate the target picture reasonably well the subject can correctly identify it as the target. It is probably no coincidence that two of the most successful targets in one well-known series of ganzfeld experiments conducted by Charles Honorton and his colleagues were a short segment of a Bugs Bunny cartoon and a static picture of Santa Claus in a Coca-Cola advertisement. These would be well-established memory images for his American participants.

Another issue for Irwin was what type of memory is involved in ESP. It seems clear that the type of memory involved in the ESP process

is long-term declarative memory. The spontaneous cases suggest that the memories are primarily visual and generally concrete rather than abstract. Irwin observes that in a number of experimental and quasi-experimental studies it is the *structure* of the target that seems to come through. Semantic memories do not seem to be activated. If any semantic information emerges it is usually at a later stage when the subject attempts to interpret the images he or she has experienced. Warcollier's extensive naturalistic telepathy experiments led him to conclude, "It is not the *meaning* which is transmitted, but the *image*" (Warcollier, 1939, p. 131). Upton Sinclair's experiments with his wife as subject (Sinclair, 1962) repeatedly produced examples of strikingly close reproductions of the target material, but which Mrs. Sinclair could not identify at all. More contemporary examples come from the research on remote viewing that supported America's psychic spying programme. Early in that project the scientists realized that the sketches and drawings the remote viewers made were likely to be more useful and accurate than the viewer's effort to interpret his or her impressions verbally. This led to the standard operating procedure to remind the remote viewers to just draw their images, not to try to interpret them.

While I agree with Irwin that semantic memory seems to play little or no part in ESP, I think more than mere structural elements are involved. I think the evidence, particularly from the dream research and the ganzfeld experiments, indicates that complete images of objects or individuals are often activated. I am inclined to think that the basic memories that underlie the ESP process when it arises into consciousness are primarily *visual* images, but I don't think this is far from Irwin's notion.

Having focused on the structural memories as the vehicle for ESP, Irwin goes on to develop a model of how the known features of human information processing will determine how the ESP information eventually emerges into consciousness. Although I think we are dealing with more than just structural memories, I think the idea that once activated, ESP-triggered memories will be subject to the same sort of cognitive processing that ordinary memories are makes sense. One simple example is that issue of cognitive processing capacity. Spontaneous case investigations reveal that a large proportion of the waking psychic impressions come when the recipient is relaxing or engaged in a more-or-less "mindless" activity such as washing dishes. Spontaneous experiences are rarely reported during periods of intense cognitive activity.

In the most successful laboratory ESP experiments an effort is made to make the subjects relaxed and undistracted by sensory or cognitive activities.

## Revisiting spontaneous psychic experiences

Irwin's model, and Roll's which preceded it, offer an important framework within which to understand how psi works in the brain. Surprisingly, there has been very little research to explore Irwin's model, and no research at all studying possible connections between memory function and ESP using experimental techniques that produce better psi combined with appropriate sample sizes based on power analyses. In the years since Irwin's work, neuroscience has moved on, and I shall argue that today there are even more compelling reasons for suspecting the involvement of memory systems in psychic experiences, and that other systems of the brain and body are likely to be involved as well.

First a brief review of the salient characteristics of extrasensory psychic experiences as they occur in life is needed. However much we may learn in the laboratory, the natural occurrence of psi in daily life remains the touchstone against which our understanding must be tested.

Spontaneous cases naturally fall within three main categories. Because the various case collections have used different criteria and have had different emphases, it is not wise to rely on them for exact proportions, but we draw some general conclusions. By far, the most common vehicle for psychic experiences is the dream. Dreams form between one third and nearly two-thirds (in the massive L.E. Rhine collection) of all spontaneous cases reported (Stokes, 1997).

It has long been recognized that the images that play across our dreams are from memory sources. The dreaming state seems particularly designed to facilitate the re-arranging and combining of memory images in new patterns and episodes. Of course, this is fundamental to psychoanalytic theory and is a well known feature of creativity and problem-solving. It is not a big step to expect that dreams might form an ideal workspace in which the yet unknown psychic influence can assemble the various memory images to present new information to consciousness.

Louisa Rhine, who specialized in the analysis of spontaneous cases, divided the dream experiences into two categories: realistic and symbolic. Two examples from the thousands of dream cases will serve to

illustrate how memory underlies the ESP process.

The first case comes from a streetcar (tram) driver in Los Angeles. He reported an extremely realistic and detailed dream in which he was driving a streetcar on the W line loaded with passengers. "All the things in the dream were as they actually were; I mean the streets, stores, traffic conditions. Everything was in the dream just as they were in real life." Crossing an intersection he saw a northbound number 5 streetcar and waved to the motorman as he passed. "Suddenly, without warning a big truck, painted a solid bright red, cut in front of me ... and the truck making the illegal turn could not see my car because of the other streetcar. There was a terrific crash. People were thrown from their seats and the truck overturned." Two men from the truck lay dead on the pavement and the driver walked up to a woman from the truck who was screaming in pain. She looked at the driver with what he described as the "largest, bluest eyes I had ever seen" and repeatedly shouted, "You could have avoided this."

The driver awoke from his dream soaked in sweat and very shaken. Later when he arrived at work he was assigned to the W line and had forgotten about the dream. On his second trip of the day he was at the intersection from his dream with a full load of passengers.

> "I was waiting for the signal to change, still not thinking of the dream, when suddenly I became sick to my stomach. I was actually nauseated. I felt provoked at myself and hoped it would go away. As I left the intersection on the signal change, I saw, just as in my dream, a Number 5 car, northbound. Now I was definitely sick. Everything seemed to have happened before, and my mind seemed to be shouting at me about something. When I waved to the motorman on the '5' car, the dream came to me. I immediately shut off the power and applied the brakes, stopping the car. A truck, not a big truck completely red as in my dream, but a panel delivery truck, with the space for the advertising on the side painted over with bright red, shot directly in my path. Had I been moving at all, I would have hit it as surely as I did in the dream."
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> "There were three people on the truck, two men and a woman. As the truck passed in front of me, the woman leaned out of the window and looked up at me with the same startled, large blue eyes I had seen in my dream, and ... waved her arm

and hand, thumb and forefinger circled in the familiar 'okay' gesture."[3]

This is an example of a realistic case and it is easy to see that the dream consisted of the driver's well-established memories of the streetcar route and the situations he normally encountered, plus apparently new information assembled from his memories of other vehicles, people and possible scenarios. Although he may not have had the memory of a streetcar collision as such, it would be a simple matter to construct a plausible scenario from existing memory images just as anyone could imagine an auto accident happening. It is interesting to note that the dream images of what had not yet happened were not entirely accurate (e.g., the truck), but they were enough to get the message across.

The second illustration is a symbolic dream from a woman whose son was in the Navy in the South Pacific Theatre during the Second World War. "I dreamed that my young son, an only child ... came to me while I was busy in the kitchen and handed me his uniform which was sodden, soaking, and dripping wet. He had a most distressed expression on his young face..." The woman's dream continued with her wringing the water out of her son's uniform but her son took it from her and dropped into the laundry tub and took her into his arms and said, "Isn't this terrible! Oh, Mom — it's all so terrible!" In the dream the woman reminded her son that nothing was so terrible that they could sit down and talk it through, and the two of them went into the living room and the woman sat down and her son sat in her lap, and, as these things happen in dreams, the son turned into an infant as she rocked him and the dream soon ended. In due course the woman learned that her son's ship had been torpedoed on the very night that she had the dream and that her son and 250 others had been killed in the massive explosion of ordnance that resulted (Rhine, 1961, p. 49).

Again, the dream images are all drawn from memory images or the plausible manipulation of image components. It is classified as a symbolic dream because it is far from a representation of the related event. One can only speculate as to why some psychic dreams are symbolic, but it could be that for this woman the images needed to construct a scenario of the carnage of a torpedoed munitions ship were simply not available and the "message" was carried through other images. This was before television, of course, and the images that the public saw in

[3]From the LE Rhine collection, quoted in Broughton (1991, pp. 20-21).

theatres and elsewhere were carefully controlled during the war.

The second largest type of spontaneous ESP in most of the collections is the intuitive impression, representing a little more than a quarter of the cases. The intuitive impressions often amount to a sudden hunch or an unexpected awareness that something of consequence has happened (usually to someone connected to the person who has the experience). The cases are often described as "just knowing" and are unaccompanied by any imagery or reasoning process. A typical example of an impression case would be when a mother suddenly "knows" that something has happened to her child and drives to the school where she learns that her child had just had an accident in the playground. Many of these impressions are accompanied by strong emotional feelings, often anxiety or dread. A significant number of cases involve *only* the feelings, with no cognitive content to explain them. Consider this case from a young man in California.

> "One night in July of 1951 we had just finished supper, and my brother-in-law was getting ready to go to a meeting in San Jose, which is twenty-five miles from our house. For no reason I started crying, me, crying, twenty-five years old! I *begged* him not to go. Well there was quite a fuss and I got everyone upset. Mom kept saying, "He will be all right." You know, the usual soft soap you give an upset person. This went on for about fifteen minutes. Then the feeling left me, and I said, "It's all right for Bob to go now."
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> "By this time the fellow he was to ride with had waited at their meeting place, but left before Bob got there, so Bob had to drive his own car down. He got as far as Bayshore and Charter Streets, when the traffic began to back up. A wreck, which is nothing unusual around here, but when Bob got to the corner, he said he almost passed out. There spread out on the highway was the man he was to have ridden with; his head was half gone. The car was a total loss. They found later that his brakes had locked on one side, and he flipped up in the air and came down on the other side of the road to be hit head-on by another car."
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> (Rhine, 1961, p. 127)

This is a striking example of a case where the unexpected onset of

strong feelings — with no images or other information — managed to prevent a family tragedy.

The final type of spontaneous case is that of the waking hallucination or quasi-sensory image. This is where we find our clothed ghosts, but most of the hallucinations involve living individuals. Sometimes the hallucinations can be relatively trivial, but frequently they convey important information. In another Second World War example, an American soldier had been driving a car with several officers on an inspection tour of the front lines. Just before starting the return journey, another soldier told him of a short cut back to the base. He found the short-cut and began driving down the road when suddenly he saw a friend waving him to stop, telling him to go back the way he came. Without thinking much of it, he reversed the car, taking care to avoid a truck full of marines waiting to go down the same road. Only when he was nearly back at the base did he realize that the friend who had just directed him back to the main road had, in fact, been killed a couple of weeks earlier. The next day when the casualty reports came in, the driver learned that the truckload of marines that he had taken care to avoid had hit a mine on the road he almost went down, and all were killed.

Those are the main types of psychic experiences when we recognize them to be sufficiently different from our ordinary experience that we take note of them. But if psi can affect behaviour rather dramatically, as we seen in spontaneous cases, might it not operate at more subtle and less noticeable ways. This brings us to a fundamental question that we often loose sight of, "What is psi for?" Surely it is more than a curious freak show on the margins of consciousness. At an operational level, it seems that the chief function of psi is to provide information that we use, consciously or unconsciously, to make better decisions about future courses of action or to otherwise guide our behaviour. At a more general level, at least for me, the answer is simple: psi is meant to be useful, and it is meant to be useful in the same way as all our other abilities — to contribute to our survival and our ability to pass on our genetic heritage (Broughton, 1988).

From this perspective we would expect ESP (or receptive psi) to have certain characteristics. As something that is likely to have co-evolved with consciousness we should expect ESP to be tightly integrated with all the other components of human behaviour. The "normal" operation of ESP (in contrast with the more exceptional instances that attract our attention) might be to bias our decision-making process

and influence our behaviour in such a way that serves our needs. The most thoroughly developed model of receptive psi as a need serving component of human behaviour has been developed by Stanford (Stanford, 1990). In Stanford's model, ESP information can unconsciously initiate a wide range need-relevant "responses", including behaviours, feelings, images and associations or even desires.

One of evolution's distinguishing characteristics is that makes remarkably economical use of an organism's resources. Evolution tends not to devise entirely new systems but rather adapts and extends existing systems to serve new needs and confer new advantages. Our own brains are build on the substrate of a reptilian brain which remains a crucial part of what it means to be human, and to survive. New functions are typically "piggybacked" onto existing systems that they might enhance.

What system or systems of the human brain might we speculate that psi is piggybacked upon? If I were to place a bet, it would be on the emotional system, working in conjunction with memory. The emotional system is, of course, the underpinning of that most fundamental survival adaptation, the fight or flight response, and, through the operations of the amygdala, is designed the detect threats and danger and to initiate responses automatically. This is a system that we share with virtually all vertebrates. LeDoux (1996) argues that it is precisely the merging of the emotional and cognitive systems that conveys our immense evolutionary advantage because it allows us to shift from simple automatic reactions to planned actions. Damasio (1994) has further shown that the emotional system plays a major role in the action phase by constraining the available options by biasing the selection of memory images that present to us our options for action.

Elsewhere I have argued that Damasio's somatic marker hypothesis offers a very promising framework in which to see the possible operation of anomalous information in consciousness (Broughton, 2002). In that paper I drew attention to the emotional components of psychic experience, but Damasio's somatic marker hypothesis involves both the emotional system *and* memory working in close harmony.

In this context, let us turn again to the spontaneous cases. The special relationship between sleep and dreaming, and memory is an area of much research and considerable debate today (Hobson, Pace-Schott, & Stickgold, 2000; Stickgold, Hobson, Fosse, & Fosse, 2001). Sleeping and dreaming seems to enjoy a two-way relationship with memory. Mem-

ory is obviously the source of the content of dreams, but dreams, and more generally, sleep, plays a role in the consolidation and strengthening of long-term memory.

Precisely what the sources of the memories for dreaming are and how they are selected remains uncertain. Some researchers now see dreams not simply the result of more-or-less random brain activity, but a form of meaningful memory processing (Paller & Voss, 2004; Revonsuo, 2000). There is growing evidence that the emotional system plays an important role. Recent research shows that during REM sleep the central nucleus of the amygdala and the limbic forebrain structures are activated, contributing substantial input from the emotional system into the dreaming process. Braun and colleagues have shown that the limbic system and visual association areas are active in REM sleep, but not the primary visual area of the brain, suggesting that dreaming may represent a closed system that operates without the brain systems that mediate sensory information (Braun et al., 1998). Interestingly, Stickgold and colleagues note, "... although emotions appear to play an important role in the selection of memories for incorporation into dreams, the dreams themselves often show little or no emotional content." (Stickgold, Hobson, Fosse, & Fosse, 2001, p. 1056). The particular combination of brain activity that occurs during dreams may be suited to the creative and problem solving activity associated with sleep and dreams (Wagner, Gais, Haider, Verleger, & Born, 2004), and it may provide a suitable canvas upon which the memory images needed to convey anomalous information can be painted.

Intuitions, which form the second most common type of psychic experience, might, on first inspection, present problems since they seem to involve neither memory nor images, but that would be misleading. The momentary contents of consciousness are images, some sensory, others drawn from memory. During periods of low sensory input, memory images will dominate. These images are held in working memory for periods from a fraction of a second to several seconds. The mechanism of basic attention holds a particular image in working memory more or less to the exclusion of other images. As part of his somatic marker hypothesis Damasio (1994) argues that the emotional system, working in concert with the prefrontal structures of the brain, plays a major role in the generation of the particular images that play across consciousness and in determining which images receive the focus of our attention. When this unconscious process leads to sudden conclusions or de-

cisions, it will feel like *intuition* because the solution or the decision will seem to have just "popped" into mind. In fact, a very subtle interplay of learned emotional experience and memory will have been behind the process, but that will all be hidden from conscious awareness. My speculation is that this would be a convenient process on to which a system for using anomalous information might be grafted.

A particularly interesting feature of many of the intuitive cases is that they involve strong emotional feelings (Stevenson, 1970). LeDoux (1996) and Damasio (1994) and others have shown that in addition to declarative memory a person has emotional memory. These are memories that can set our bodies in the physical states — the feelings — associated with past experiences. These memories of feelings are the somatic markers that underpin decision making and planning in Damasio's somatic marker theory. Cases such as the young man whose teary outburst delayed his brother-in-law's departure provide a most important clue to the process in that they seem to represent a direct activation of feelings with no cognitive content at all.

The third and least common type of psychic experience — hallucinations — also involve memory images, but now masquerading as sensory input. The images are most commonly visual, but also can be auditory or involve other senses. The hallucinations of spontaneous cases are also quite unlike the recurrent hallucinations associated with clinical and neurological conditions. They usually are isolated events in normal individuals.

The aetiology of hallucinations in clinical and non-clinical populations is also the subject of much research and debate (see Collerton, Perry, & McKeith, 2005, and accompanying commentaries). The prevailing models for hallucination generally involve deficiencies or malfunctions in attributing the source of images in short term memory (Bentall, 2000; Collerton, Perry, & McKeith, 2005). In these models, images from internal sources are incorrectly attributed to external sensory input. More recently, speculation has grown that waking hallucinations may have their origins in the same mechanisms that underlie dreaming (Gottesmann, 2005; Ingle, 2005; Mahowald, Woods, & Schenck, 1998; Pace-Schott, 2005).

However hallucinations are triggered, there is little doubt that the images are drawn from, or constructed from schema held in, long-term memory. In psychic hallucinations, what is it that selects the particular memory images that convey the message? At this stage the an-

swer to that question can only be speculative, but, again, Damasio's somatic marker hypothesis provides some intriguing clues. A fundamental component of his model is the role of the emotional system in selecting the images to which we attend, and the evolutionary roots of this system are in that part of the brain concerned with threat detection and survival reactions. That system has evolved mechanisms to bias and influence the attentional resources we deploy to the images that represent our options for action (Damasio, 1994, 1996). Again, if we have a system designed for fast automatic processing of survival-related information, would it not make sense for it to incorporate *psychic* information if and when it is available? Evolution may simply have found that the way to present psi information during ordinary waking consciousness may to "superimpose" suitable memory information on ongoing sensory processing.

A final intriguing suggestion linking our use of psychic information and the emotional system lies in what we often think of as one of the great problems with research in this area — our inability to control consciously the use of psychic information. Spontaneous cases are just that — spontaneous. They come of their own accord — one does not make them happen. In the laboratory psi is described as "elusive" and that its operation is unconscious. In my evolutionary view of psi I argued that one of the characteristics we could expect of evolved psychic abilities might be that they are not subject to conscious control, because if they were they would be too easy to misuse in a way that was not conducive to survival in evolutionary terms (Broughton, 1988). At the time I could not think of a mechanism, but recently Dolan has noted that emotions "…are less susceptible to our intentions than other psychological states insofar as they are often triggered, in the words of James, 'in advance of, and often in direct opposition of our deliberate reason concerning them"' (Dolan, 2002, p. 1191). Indeed, it seems the very nature of the emotional system's operation as a survival response system is that it is unconscious and independent of our intentions.

In conclusion, it seems clear, perhaps even obvious, that memory mediates the appearance of psychic information into consciousness. How those memories are triggered remains a mystery, but I am convinced that the evidence from our growing understanding of the emotional systems suggests that we should look there for the origins of the psychic influence on behaviour. The substantial number of spontaneous cases that seem to consist solely of emotional feelings further suggests

that we may be dealing with a system that affords multiple paths for psi to achieve its goals. One might involve the interaction of the emotional system and memory, as we have been examining, and another may be a direct triggering of the feeling component of emotions.

Ultimately there is a need for new and innovative experimental approaches if there is any hope of translating my speculations into hypotheses. Recent years have seen the emergence of exciting new experiments exploring more directly the link between psi and the emotional system in the work of Dean Radin (Radin, 1997, 2004) and Dick Bierman (Bierman, 2004; Bierman & Radin, 1997). But this is just the beginning, and more than ever there is a need for parapsychologists to join with neuroscientists in the quest for understanding psychic experiences.

## References


Bentall, R. P. (2000). Hallucinatory experiences. In E. Cardeña, S. J. Lynn & S. Krippner (Eds.), *Varieties of anomalous experience: Examining the scientific evidence* (pp. 85–120). Washington, DC: American Psychological Association.

Bierman, D. J. (2004). *Non conscious processes preceding intuitive decisions.* Paper presented at the Bial Foundation 5th Annual Symposium: Aquém e Além do Cérebro (Behind and Beyond the Brain), Porto, Portugal.

Bierman, D. J., & Radin, D. I. (1997). Anomalous anticipatory response on randomized future conditions. *Perceptual and Motor Skills, 84*, 689–690.

Braun, A. R., Balkin, T. J., Wesensten, N. J., Gwadry, F., Carson, R. E., Varga, M., et al. (1998). Dissociated pattern of activity in visual cortices and their projections during human rapid eye movement sleep. *Science, 279*(5347), 91–95.

Broughton, R. S. (1988). If you want to know how it works, first find out what it's for. In D. H. Weiner & R. L. Morris (Eds.), *Research in Parapsychology 1987* (pp. 187–202). Metuchen, N.J.: Scarecrow Press.

Broughton, R. S. (1991). *Parapsychology: The controversial science*. New York: Ballantine.

Broughton, R. S. (2002). Telepathy: Revisiting its Roots. In *4º Simpósio da Fundação Bial: Aquém e Além do Cérebro: Relações Interpessoais Excepcionais.* (pp. 131–146). Porto: Fundação Bial.

Collerton, D., Perry, E., & McKeith, I. (2005). Why people see things that are not there: A novel Perception and Attention Deficit model for recurrent complex visual hallucinations. *Behavioral and Brain Sciences, 28*(06), 737.

Damasio, A. R. (1994). *Descartes' error: Emotion, reason, and the human brain*. New York: G. P. Putnam's Sons.

Damasio, A. R. (1996). The somatic marker hypothesis and the possible functions of the prefrontal cortex. *Philosophical Transactions of the Royal Society of London, B, 351*, 1413–1420.

Dolan, R. J. (2002). Emotion, Cognition, and Behavior. *Science, 298*(5596), 1191–1194.

Gottesmann, C. (2005). Waking hallucinations could correspond to a mild form of dreaming sleep stage hallucinatory activity. *Behavioral and Brain Sciences, 28*(06), 766.

Gurney, E., Myers, F. W. H., & Podmore, F. (1886). *Phantasms of the living* (Vol. 1). London: Trubner.

Hobson, J. A., Pace-Schott, E. F., & Stickgold, R. (2000). Dreaming and the brain: Toward a cognitive neuroscience of conscious states. *Behavioral and Brain Sciences, 23*(06), 793.

Ingle, D. (2005). Two kinds of "memory images": Experimental models for hallucinations? *Behavioral and Brain Sciences, 28*(06), 768.

Irwin, H. J. (1979). *Psi and the mind*. Metchen, N.J.: Scarecrow Press.

Irwin, H. J. (1999). *An introduction to parapsychology* (Third ed.). Jefferson, NC: McFarland & Company, Inc.

LeDoux, J. (1996). *The emotional brain: The mysterious underpinnings of emotional life*. New York: Touchstone.

MacKenzie, A., & Goldney, K. M. (1970). Two Experiences of an Apparition. *Journal of the Society for Psychical Research, 45*(December), 381–391.

Mahowald, M. W., Woods, S. R., & Schenck, C. H. (1998). Sleeping dreams, waking hallucinations, and the central nervous system. *Dreaming, 8*(2), 89–102.

Myers, F. W. H. (1903). *Human personality and its survival of bodily death* (Vol. 1). London: Longmans, Green, and Co.

Pace-Schott, E. F. (2005). Complex hallucinations in waking suggest mechanisms of dream construction. *Behavioral and Brain Sciences, 28*(06), 771.

Paller, K. A., & Voss, J. L. (2004). Memory reactivation and consolidation during sleep. *Learning & Memory, 11*(6), 664–670.

Radin, D. I. (1997). Unconscious perception of future emotions: An experiment in presentiment. *Journal of Scientific Exploration, 11*(2), 163–180.

Radin, D. I. (2004). Electrodermal Presentiments of Future Emotions. *Journal of Scientific Exploration, 18*(2), 253–273.

Revonsuo, A. (2000). The reinterpretation of dreams: An evolutionary hypothesis of the function of dreaming. *Behavioral and Brain Sciences, 23*(06), 877.

Rhine, L. E. (1961). *Hidden Channels of the Mind*. New York: William Morrow.

Roll, W. G. (1966). ESP and memory. *International Journal of Neuropsychiatry, 2*, 505–521.

Schmeidler, G. R. (1991). Perceptual processing of psi: A model. *Journal of the American Society for Psychical Research, 85*, 217–236.

Sinclair, U. (1962). *Mental radio* (2nd ed.). Springfield, Il: Charles C. Thomas.

Stanford, R. G. (1990). An experimentally testable model for spontaneous psi events: A review of related evidence and concepts from parapsychology and other sciences. In S. Krippner (Ed.), *Advances in Parapsychological Research 6* (pp. 54–167). Jefferson, N.C.: McFarland.

Stevenson, I. (1970). *Telepathic impressions: A review and report of thirty-five new cases*. Charlottesville, VA: University Press of Virginia.

Stickgold, R., Hobson, J. A., Fosse, R., & Fosse, M. (2001). Sleep, Learning, and Dreams: Off-line Memory Reprocessing. *Science, 294*(5544), 1052–1057.

Stokes, D. M. (1987). Theoretical parapsychology. In S. Krippner (Ed.), *Advances in parapsychological research, Vol. 5* (pp. 77–189). Jefferson, NC: McFarland.

Stokes, D. M. (1997). Spontaneous psi phenomena. In S. Krippner (Ed.), *Advances in parapsychological research 8* (pp. 6–87). Jefferson, N.C.: McFarland.

Wagner, U., Gais, S., Haider, H., Verleger, R., & Born, J. (2004). Sleep inspires insight. *Nature, 427*(6972), 352–355.

Warcollier, R. (1939). *Experiments in telepathy*. London: George Allen & Unwin.

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# Incredible Memories — How Accurate are Reports of Anomalous Events?

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**Abstract**

*Psychologists have studied the accuracy of eyewitness testimony for many decades and, more recently, there has been a great deal of research carried out on the topic of false memories. An overview of research in these two areas is presented with a particular focus upon the accuracy of reported memories for anomalous experiences. It has been shown that eyewitness accounts of faked séances and other pseudo-psychic demonstrations are often highly inaccurate. Recent research has also considered memory conformity effects whereby the account of an ostensibly paranormal event provided by one witness can be shown to have an effect upon the accuracy of the report of a co-witness. It is often the case that the degree of memory distortion in such studies is related to the level of paranormal belief. Paranormal belief and the tendency to report ostensibly paranormal experiences have been shown to be correlated with a number of psychological variables which themselves correlate with susceptibility to false memories, including dissociativity, absorption, fantasy proneness, hypnotic susceptibility, and reports of childhood trauma. This suggests that at least some reports of ostensibly paranormal experiences may be based upon false memories. The results of recent studies supporting such a claim will be presented.*

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## Introduction

Ever since records began and in all known societies, people have reported unusual experiences which, taken at face value, would suggest that the current conventional Western scientific world view is at best incomplete and at worst seriously in error. Many of these experiences would today be labelled as 'paranormal' and it is recognised that they are a major factor in explaining the high levels of paranormal belief found even in modern societies (e.g., Blackmore, 1984). The ubiquitous nature of such claims might be taken as evidence that paranormal forces really do exist, but it must always be borne in mind when dealing with such reports that they are almost always mainly dependent upon the memory of the claimant. The issue of the actual degree of accuracy of anecdotal accounts is therefore central in assessing such evidence. French (2003) presented a comprehensive review of the relevance of research into eyewitness testimony and false memories for reports of anomalous experiences. It is the purpose of this paper to summarise and update that review, including the presentation of recent findings from the Anomalistic Psychology Research Unit at Goldsmiths College. The first part of this paper will describe findings from recent studies of eyewitness testimony relating to ostensibly paranormal events, including studies of the effects of verbal suggestion, and misinformation effects, including so-called memory conformity effects. The second part will discuss the relevance of false memory research in assessing reports of anomalous experiences, and discuss various possible interpretations of the empirical link between paranormal belief and experience, childhood trauma, dissociativity and fantasy proneness.

## Verbal suggestion and eyewitness testimony for anomalous events

A vast body of experimental literature demonstrates that eyewitness testimony for crimes and other events can often be extremely unreliable even when no deliberate attempt is made to distort the memories of witnesses (e.g., Loftus, 1979). The same is true of witnesses to ostensibly paranormal events. French (2003, p. 157) highlights several factors often associated with such events that would serve to undermine the reliability of honest witnesses including "poor viewing conditions (e.g., darkness or semi-darkness), altered states of consciousness (e.g., due to tiredness, biological trauma, engaging in particular rituals or drug

abuse), emotional arousal, and either the ambiguous and unexpected nature of the event on the one hand (in spontaneous cases) or a high level of expectation and will to believe on the other (e.g., in a séance)".

More recent research by Wiseman and colleagues has explored the effects of verbal suggestion on the reliability of eyewitness accounts of séances and other ostensibly paranormal events, taking their cue from the fact that fraudulent mediums and mentalist conjurors have often described how powerful simple verbal suggestion can be in influencing witnesses' accounts of an event. Wiseman, Greening, and Smith (2003), for example, carried out a fake séance in which an actor suggested that a stationary table was moving. In response to a memory questionnaire, around one third of the observers incorrectly reported that the table had indeed moved, with this tendency being stronger for believers in the paranormal than for disbelievers. In a second experiment, Wiseman and colleagues systematically varied whether the verbal suggestions provided were consistent or inconsistent with the observers' stated attitude towards the paranormal. Believers were found once again to be more susceptible to the effects of suggestion than disbelievers but only when the suggestion was congruent with their stated belief. Overall, around 20% of those taking part in these fake séances reported believing that genuine paranormal phenomena had taken place in the darkened séance room.

Wiseman and Greening (2005) explored the effects of verbal suggestion in another ostensibly paranormal context. Many eyewitnesses to alleged psychokinetic metal-bending attest that not only did they see a metallic object (typically a key or a piece of cutlery) bend while in the hands of the alleged psychic, but that it continued to bend right before their very eyes even when it had been placed on the table in front of them. The claim that the metal continues to bend even when not in contact with the psychic is offered as compelling evidence that a genuinely paranormal effect had taken place, not simply some form of sleight of hand. Wiseman and Greening provide convincing evidence that such a conclusion would be inadvisable given the unreliability of eyewitnesses in this context. They presented participants with a video clip showing a skilled conjuror playing the part of an alleged psychic claiming to use psychokinetic ability to bend a key, although in fact using sleight of hand to achieve the effect. Having bent the key, the psychic then placed it back on the table and the video clip ended with a long close-up of the bent key. Half of the participants then heard the psychic say

that the key was continuing to bend while the other half saw the same footage but without the verbal suggestion. Although the key did not in fact continue to bend, around 40% of the participants in the suggestion condition reported that it did. Only one participant out of 23 reported that the key continued to bend in the no-suggestion condition. A second study replicated this general pattern of results as well as showing that those who reported that the key continued to bend were more confident in their testimony and also less likely to recall the actual verbal suggestion from the fake psychic. Surprisingly, no differences were found between believers in the paranormal and non-believers in terms of their susceptibility to verbal suggestion in this context.

These studies provide an interesting insight into the effects of the hitherto neglected factor of verbal suggestion on eyewitness reliability in an ostensibly paranormal context. Despite the attractiveness of obtaining such results in relatively naturalistic settings, questions remain regarding the mechanisms that underlie the reported effects. It is possible that the verbal suggestion affected either the perception of the event or the observer's memory of the event or both. It is even possible that neither of these explanations is correct and that instead the participants were influenced by the demand characteristics of the situation, i.e., they were simply giving the responses that they believed the investigators wanted to receive. Future investigations should be directed at attempting to determine which of these explanations is correct. However, the visible surprise upon the faces of many witnesses in response to such simple verbal suggestions would suggest that at least some of them do perceive the events in line with the verbal suggestion.

## Post-event misinformation and eyewitness testimony for anomalous events

If no attempt is made to influence an eyewitness's memory for an event until after the event has taken place, we can be sure that any effects reported cannot be due to any direct effect upon the perception of the event itself. A number of different techniques have been developed that show the distorting influence of misinformation presented after an event has been witnessed. Memory researchers have been studying such misinformation effects for over 30 years (e.g., Loftus, 1979). What these techniques have in common is that witnesses first observe a complex event such as a staged crime or accident. Half of the participants

are then exposed to misleading information about the event, while the other half are not so exposed. Finally, all participants are tested upon their recall for the original event. Typically, a higher degree of memory distortion is observed amongst the participants exposed to the misinformation.

This approach was employed in a recent study of memory for psychic readings in which we demonstrated that believers in the paranormal are more likely to misremember a psychic reading in such a way that they recall the information provided by the psychic as being more specific than it actually is (K. Wilson & French, submitted, a). Participants were presented with a video clip of an alleged psychic giving a reading to a sitter, followed by another clip in which the sitter comments upon the accuracy of the reading. In fact, both the reading and the post-reading interview were entirely scripted. Two different versions of the video were prepared. Both versions showed identical readings and almost identical post-reading interviews with the sitter, apart from one crucial statement. In one version of the interview the sitter correctly asserts that the psychic "mentioned the name Sheila, and that is my mother's name". In the other version, she incorrectly asserts that the psychic "said my mother's name was Sheila", making it appear that the psychic was more specific in his utterance than he actually was. We had hypothesised that this post-event misinformation would lead to greater memory distortion on the part of believers than non-believers, given that greater accuracy on the part of the psychic would be congruent with their general belief in psychic ability. In fact, somewhat surprisingly, we found that believers showed a strong tendency to misremember this part of the reading less accurately *whether or not* they received the post-reading misinformation. Non-believers tended to remember the reading more accurately than believers if no misinformation was supplied but, interestingly, their memories were as distorted as the believers' in the misinformation condition.

We are as interested in investigating possible memory biases on the part of non-believers as we are on the part of believers. We therefore intend to carry out a follow-up experiment using a similar methodology, i.e., scripted reading and post-reading interview. However, in addition to the conditions described above, we would also include a condition in which the sitter makes an incorrect assertion that makes the psychic appear to be *less* accurate and specific than he actually was. The main foci of interest would be (a) to replicate effects found in the first study

using this methodology and (b) to investigate the possibility that non-believers would show greater memory distortion than believers when the post-reading misinformation is congruent with their beliefs.

When attempting to ascertain the reliability of eyewitness accounts in either a forensic context or in an anomalistic context, highly similar accounts from multiple witnesses are understandably taken as being more reliable than either a single uncorroborated account or an account which differs from that of another witness. While such an assumption is probably justifiable, it should always be borne in mind that such accounts may well be influenced by a particularly insidious form of misinformation effect known as memory conformity. When multiple witnesses observe an unusual event such as a crime or a possible sighting of a UFO, a ghost or the Loch Ness Monster, they will be very likely to discuss the event between themselves prior to any formal questioning by investigators. Memory conformity refers to the phenomenon whereby the testimony of one eyewitness directly influences the testimony provided by a second eyewitness (Gabbert, Memon, & Allan, 2003). For example, if pairs of participants are asked to view a video recording of a staged crime and are led to believe that they have both viewed the same video clip when in fact the video clips are subtly different, it can be shown that, following discussion, one witness's account can have a direct influence on that of the co-witness. Thus the first witness may report directly observing actions (such as someone stealing a purse) that were in fact only directly observable on the co-witness's video clip.

As already described, Wiseman and Greening (2005) showed participants a video clip of a key being bent by an alleged psychic who was in fact using sleight of hand. They reported that around 40% of the participants reported that the key continued to bend after it was placed on the table if the psychic simply said, "Look. It's still bending." In the absence of such a suggestion from the psychic, virtually no one reported that the key continued to bend. K. Wilson and French (submitted, b) replicated this basic effect, but also found, in contrast to Wiseman and Greening, that believers were more likely to report that the key carried on bending than non-believers. However, we also went one stage further by adding a memory conformity component to the original experimental design. In addition to the conditions used by Wiseman and Greening, we also included conditions with a stooge present. The stooge either indicated that he did see the key continue to bend or that he saw that the key did not continue to bend. We found that the stooge's

expressed belief about whether the key continued to bend also had an effect on the reports of the genuine participants. Clearly, although the original verbal suggestion may have a direct effect on the witnesses' perception, the subsequent influence of the stooge's expressed belief must be explained in terms of either an effect on memory or demand characteristics. Further experiments investigating memory conformity are planned, using a wider range of ostensibly paranormal events.

## False memories and reports of anomalous events

Studies of the unreliability of eyewitness accounts of observed events have been carried out since the early days of scientific psychology and it has long been recognised that details of witnessed events may be lost or distorted in memory. A great deal of research over the last couple of decades, however, has focussed upon the fact that some apparent memories appear to be entirely false; that is to say, they are not based upon any actual event directly witnessed by the claimant at all. French (2003) also considered the relevance of this body of research with respect to reports of anomalous events, especially reports of past-life memories and alien abduction claims (see also, French, 2001; Holden & French, 2002).

A wide range of experimental paradigms have been developed to investigate the factors that lead to the development of false memories although a comprehensive review of these techniques is beyond the scope of the current presentation (see, e.g., Garry & Gerrie, 2005; Loftus, 1997, 2001, 2003; McNally, 2003; Ost, 2005; Smeets, Jelicic, Peters, Candel, Horselenberg, & Merckelbach, 2006). One of the issues that has been the focus of a great deal of attention is the identification of psychological variables that correlate with susceptibility to false memories. A number of such variables have been identified, although it should be noted that there is considerable variability in the findings across studies, possibly reflecting the variation in experimental paradigms employed and other factors. Amongst the variables that appear to correlate with susceptibility to false memories (at least in certain contexts) are fantasy proneness (Spanos, Burgess, & Burgess, 1994), hypnotic suggestibility (e.g., Barnier & McConkey, 1992), dissociativity (e.g., Hyman & Billings, 1998), absorption (e.g., Eisen & Carlson, 1998), and vividness of visual imagery (e.g., Winograd, Peluso, & Glover, 1998). As French (2003) points out, these variables have also been shown to correlate with para-

normal belief and/or tendency to report paranormal experiences (fantasy proneness: e.g., Irwin, 1990, 1991; hypnotic suggestibility: e.g., Kumar & Pekala, 2001; dissociativity: e.g., Wolfradt, 1997; absorption: e.g., Irwin, 1985; vividness of visual imagery: e.g., Diamond & Taft, 1975). This raises the possibility that at least some reports of anomalous experiences may be based upon false memories and that those who report such experiences may be more susceptible to false memories.

Until recently, direct attempts to test the hypothesis that there is a link between susceptibility to false memories and the tendency to report anomalous experiences had met with only limited success (French, 2003), possibly reflecting the use of inappropriate techniques to measure susceptibility to the type of false memories that one might expect to correlate with the tendency to report anomalous experiences. Intuitively, one might expect that techniques that attempt to produce detailed false memories for entire episodes (e.g., Loftus & Pickrell, 1995) might be more relevant than techniques which attempt to produce false memories for, say, individual words in word lists (e.g., Roediger & McDermott, 1995).

It is somewhat surprising then that up until recently one of the few studies to produce results supporting this hypothesis had used the latter type of technique. Clancy, McNally, Schacter, Lenzenweger, and Pitman (2002) used a technique which involves presenting lists of word to participants. Within each list, all words are strongly semantically related to a critical non-presented word. For example, the words *bed, pillow, snore, dream, snooze,* and so on might be presented, but not the critical word *sleep*. Subsequently, many participants would incorrectly recall or recognise the word *sleep* as having been on the original list. Clancy et al. used this technique to demonstrate that people with conscious memories of having being abducted by aliens were more susceptible to false memories than either people who believed that they had been abducted by aliens but had no conscious memory of the event or people who did not believe that they had ever been so abducted.

A strong case can be made that false memories are indeed likely to be the explanation for reports of alien abduction and contact, although a number of other factors may also be involved (Clancy, 2005; French, 2001; Holden & French, 2002). Results of a recent study by French, Santomauro, Fox, Hamilton, and Thalbourne (2005) generally support this claim insofar as a group of participants reporting memories of alien contact were found to score more highly than a control group on a number

of variables known to correlate with susceptibility to false memories, including dissociativity, fantasy proneness, tendency to hallucinate, and absorption. However, no differences were found between the experiencers and the control group in this study in terms of susceptibility to false memories as assessed by the same measure as used by Clancy et al. (2002).

Many theoretical models of how false memories are formed would predict that believers in the paranormal would be more susceptible to false memories for ostensibly paranormal events simply because such events would be more plausible for believers than for non-believers. For example, Mazzoni, Loftus, and Kirsch (2001) presented a three-stage model in which, for a false memory to develop: (a) the event in question must be deemed to be plausible, (b) the individual must have good reason to believe that the event is likely to have happened to them personally, and (c) they must interpret their thoughts and fantasies about the event as actual memories (see also Mazzoni & Kirsch, 2002; Scoboria, Mazzoni, Kirsch, & Relyea, 2004). They presented evidence in support of this model by showing that individuals who initially reported that they had not witnessed another individual being possessed subsequently increased their estimate of how likely it was that they had witnessed such an event following interventions designed to increase the subjective plausibility of the event for the participants.

However, given the fact that so many psychological variables that correlate with susceptibility to false memories also correlate with paranormal belief and the tendency to report anomalous experiences, it is possible that believers in the paranormal may show a more general susceptibility to false memories that also encompasses susceptibility to false memories for non-paranormal events. Recent findings from our research unit support such a possibility. K. Wilson and French (2006) had one hundred participants complete a "News Coverage Questionnaire" concerning personal memories of where they were, what they were doing and who they were with when news footage of dramatic news events was first shown on television, as well as asking them to recall details of the footage itself. These news items included four events that are known to have been captured on film and one item concerning non-existent footage of the explosion of a bomb in a nightclub in Bali. Overall, 36% of respondents reported false memories of the alleged footage of the Bali bombing. Participants reporting false memories were found to score significantly higher than those who did not

report such memories on various measures of paranormal belief and experience, supporting the hypothesis that believers in the paranormal may be more susceptible to even non-paranormal false memories than non-believers. We have subsequently replicated this finding with a new sample of participants.

## A link with childhood trauma?

The final topic we wish to cover is that of the various possible interpretations of the empirically established link between paranormal belief and experience, dissociativity and fantasy proneness, and reports of childhood trauma. Ever since the early 1990s, psychologists and parapsychologists have known that fantasy proneness correlates with both paranormal belief and tendency to report paranormal experiences (e.g., Irwin, 1990, 1991). Fantasy proneness was first identified by S. C. Wilson and Barber (1983) as being a characteristic of highly hypnotically susceptible individuals. Fantasy-prone individuals spend much of their time engaged in fantasy, have particularly vivid imaginations, sometimes confuse imagination with reality, and report a very high incidence of paranormal experiences. They also are much more likely to report a history of childhood trauma. It has been postulated that fantasy proneness sometimes develops as a defence mechanism to help a child to cope with an on-going aversive situation (e.g., Lynn & Rhue, 1988; Rhue & Lynn, 1987). Fantasy provides an escape from an intolerable situation over which the child has no control into a world of imagination where the child at least has the illusion of control.

Similar arguments have been put forward to explain the association between reports of childhood abuse and tendency to dissociate. Dissociation is defined by the DSM-IV as "A disruption in the usually integrated functions of consciousness, memory, identity, or perception of the environment. The disturbance may be sudden or gradual, transient or chronic" (American Psychiatric Association, 1994, p. 766). There are problems with this definition and in practice the term *dissociation* is applied to a wide range of altered states of consciousness. Many therapists believe, however, that dissociative tendencies develop in childhood, again as a defence mechanism to help the child cope with trauma. It is believed that the dissociated state somehow attenuates awareness of the child's stressful circumstances. Indeed, many therapists would view dissociation as being the mechanism responsible for repression of

memories of such experiences, although the concept of repression itself has been questioned by experimental psychologists (see, e.g., McNally, 2003). The issue of whether or not repression ever occurs is beyond the scope of the current presentation. We do know, however, that dissociativity has been found to correlate with retrospective reports of childhood abuse (e.g., Mulder, Beautrais, Joyce, & Fergusson, 1998) and also with reports of ostensibly paranormal experiences and with paranormal belief (e.g., Irwin, 1994; Pekala et al., 1995; see French, 2003, for review). A number of investigators have provided evidence for a direct link between reports of childhood trauma and paranormal belief/experience (e.g., Irwin, 1992, 1993; Lawrence, Edwards, Barraclough, Church, & Hetherington, 1995).

What is the best explanation for the positive relationship between fantasy-proneness, tendency to report paranormal experiences and belief in the paranormal? Sceptics would argue that many claims of paranormal experiences reflect the overactive imaginations of the claimants. People with fantasy-prone personalities have very good imaginations and their claims to paranormal experiences may well reflect such imagination rather than any events that actually occurred. It is, furthermore, widely accepted that one of the most important factors in determining belief in the paranormal is personal experience of ostensibly paranormal events. According to this chain of reasoning, fantasy proneness leads to the experience of ostensibly (but not actually) paranormal events, which in turn leads to belief in the paranormal.

Proponents of the paranormal, on the other hand, have often offered a different explanation of the relationship. They have argued that "fantasy proneness may engender paranormal belief, which in turn may be conducive to parapsychological experience" (Irwin, 1991, p. 321). It is widely believed by proponents of the paranormal that believers are much more likely to experience genuine paranormal events than disbelievers. According to this chain of reasoning then, fantasy proneness leads to belief in the paranormal, which in turn makes that person more likely to experience genuine paranormal phenomena.

A third possibility is that both the reports of ostensibly paranormal experiences and the reports of childhood abuse are based upon false memories, as the measures of childhood abuse used in these studies were retrospective in nature. French and Kerman (1996) presented data comparing fantasy proneness scores and levels of paranormal belief in 23 institutionalised adolescents with reported histories of abuse with

those of 23 well-matched control participants with no known history of abuse. The former group did indeed score higher than the controls, suggesting that the results of studies using retrospective questionnaire measures can probably be accepted at face value. However, the limitations of this study must also be recognised. Although the adolescents had been institutionalised on the basis of alleged abuse, the investigators did not have direct access to documentary proof of such abuse. While it seems reasonable to assume that the institutionalised group would indeed have endured a higher level of actual abuse than the control sample, much more research is needed in this area before definitive conclusions can be drawn.

Of course, the above possibilities are not mutually exclusive. It is conceivable that childhood abuse does indeed lead to increased levels of fantasy proneness and dissociativity and consequently that retrospective reports of abuse have a basis in fact. It may also be the case, however, that this leads to heightened susceptibility to false memories and that *some* of the reported memories of abuse from any particular individual are false, even though others are true. This leaves open the question of whether or not the reported memories of paranormal and related anomalous experiences are based in fact. It is possible that someone may have an increased susceptibility to false memories as a consequence of actual, always-remembered abuse. It would be ironic and tragic, however, that the testimony of such a victim might be severely undermined in the eyes of most psychologists and psychiatrists if it were to be contaminated with false memories for more extreme forms of abuse and/or paranormal experiences. Such considerations underline the extreme caution needed to approach the truth in such cases.

The link between childhood trauma and reports of paranormal experiences merits much more research. Within anomalistic psychology, correlations between variables are often exactly those that one would expect. For example, it is not too surprising that reports of having personally experienced the paranormal are correlated with paranormal belief. But the link with reports of childhood trauma is not one of those intuitively obvious relationships — and yet it seems to be a reliable and robust finding in need of explanation.

## Conclusion

The evidence reviewed above and by French (2003) confirms the notion that much is to be gained by considering the implications of research into eyewitness testimony and into false memories when assessing the accuracy of reports of anomalous events. Research into the accuracy of eyewitness testimony, especially for staged pseudo-psychic demonstrations, strongly suggests that such reports should be treated with caution. In addition to the basic unreliability of human memory, factors such as verbal suggestion and post-event misinformation also have a distorting effect upon memory and possibly even the actual perception of such events. In many situations, believers in the paranormal appear to be more susceptible to such memory distortion but more research should be directed at establishing whether belief-congruent biases might also be found in non-believers in appropriate contexts.

With respect to false memory research, recent findings suggest that believers in the paranormal may show greater general susceptibility to false memories, including false memories for non-paranormal events. Future research should be directed at confirming such findings with a wider range of experimental techniques, as well as attempting to distinguish between the various possible explanations for the link between paranormal belief and experience, psychological variables such as fantasy proneness, dissociativity and hypnotic suggestibility, and reports of traumatic childhoods.

## References

American Psychiatric Association (1994). *Diagnostic and Statistical Manual of Mental Disorders.* 4th ed. [DSM-IV]. Washington: American Psychiatric Association.

Barnier, A. J., & McConkey, K. M. (1992). Reports of real and false memories: The relevance of hypnosis, hypnotizability, and test control. *Journal of Abnormal Psychology, 101,* 521-527.

Blackmore, S. J. (1984). A postal survey of OBEs and other experiences. *Journal of the Society for Psychical Research, 52,* 225-244.

Clancy, S. A. (2005). *Abducted: How People Come to Believe they were Kidnapped by Aliens.* Cambridge, MA: Harvard University Press.

Clancy, S. A., McNally, R. J., Schacter, D. L., Lenzenweger, M. F., & Pitman, R. K. (2002). Memory distortion in people reporting abduction by aliens. *Journal of Abnormal Psychology, 111,* 455-461.

Diamond, M. J., & Taft, R. (1975). The role played by ego-permissiveness and imagery in hypnotic responsivity. *International Journal of Clinical and Experimental Hypnosis,* 23, 130-138.

Eisen, M. L., & Carlson, E. B. (1998). Individual differences in suggestibility: Examining the influence of dissociation, absorption, and a history of childhood abuse. *Applied Cognitive Psychology, 12,* S47-S61.

French, C. C. (2001). Alien abductions. In Roberts, R., & Groome, D. (eds.). *Parapsychology: The Science of Unusual Experience.* London: Arnold. Pp. 102-116.

French, C. C. (2003). Fantastic memories: The relevance of research into eyewitness testimony and false memories for reports of anomalous experiences. *Journal of Consciousness Studies, 10*, 153-174.

French, C. C., and Kerman, M. K. (1996). Childhood trauma, fantasy proneness and belief in the paranormal. British Psychological Society London Conference at the Institute of Education, London, 17-18 December.

French, C. C., Santomauro, J., Fox, R., Hamilton, V., & Thalbourne, M. (2005). Psychological and parapsychological aspects of the alien contact experience. Invited presentation, Conference on Developing Perspectives on Anomalous Experience, Liverpool, 4 June 2005.

Gabbert, F., Memon, A., & Allan, K. (2003). Memory conformity: Can eyewitnesses influence each other's memories for an event? *Applied Cognitive Psychology, 17,* 533-543.

Garry, M., & Gerrie, M. P. (2005). When photographs create false memories. *Current Directions in Psychological Science, 14,* 321-325.

Holden, K. J., & French, C. C. (2002). Alien abduction experiences: Clues from neuropsychology and neuropsychiatry. Cognitive Neuropsychiatry, 7, 163-178.

Hyman, I. E., & Billings, F. J. (1998). Individual differences and the creation of false childhood memories. *Memory, 6,* 1-20.

Irwin, H. J. (1985). Parapsychological phenomena and the absorption domain. *Journal of the American Society for Psychical Research, 79,* 1-11.

Irwin, H. J. (1990). Fantasy proneness and paranormal beliefs. *Psychological Reports, 66,* 655-658.

Irwin, H. J. (1991). A study of paranormal belief, psychological adjustment and fantasy proneness. *Journal of the American Society for Psychical Research, 85,* 317-331.

Irwin, H. J. (1992). Origins and functions of paranormal belief: The role of childhood trauma and interpersonal control. *Journal of the American Society for Psychical Research, 86,* 199-208.

Irwin, H. J. (1993). Belief in the paranormal: A review of the empirical literature. *Journal of the American Society for Psychical Research*, *87*, 1-39.

Irwin, H. J. (1994). Paranormal belief and proneness to dissociation. *Psychological Reports, 75,* 1344-1346.

Kumar, V. K., & Pekala, R. J. (2001). Relation of hypnosis-specific attitudes and behaviors to paranormal beliefs and experiences: A technical review. In Houran, J. & Lange, R. (eds.), *Hauntings and Poltergeists: Multidisciplinary Perspectives.* Jefferson, NC: McFarland & Co. Pp. 260-79.

Lawrence, T., Edwards, C., Barraclough, N., Church, S., & Hetherington, F. (1995). Modelling childhood causes of paranormal belief and experience: Childhood trauma and childhood fantasy. *Personality and Individual Differences*, *19*, 209-215.

Loftus, E. F. (1975). Leading questions and the eyewitness report. *Cognitive Psychology, 7,* 56-572.

Loftus, E. F. (1979). *Eyewitness testimony.* Cambridge, MA: Harvard University Press.

Loftus, E. F. (1997). Creating false memories. *Scientific American, 277*, 3, 70-75.

Loftus, E. F. (2001). Imagining the past. *The Psychologist, 14,* 584-587.

Loftus, E. F. (2003). Make-believe memories. *American Psychologist, 58,* 867-873.

Loftus, E. F., & Pickrell, J. E. (1995). The formation of false memories. *Psychiatric Annals, 25,* 720-725.

Lynn, S. J., & Rhue, J. W. (1988). Fantasy proneness: Hypnosis, developmental antecedents, and psychopathology. *American Psychologist, 43*, 35-44.

Mazzoni, G., & Kirsch, I. (2002). Autobiographical memories and beliefs: A preliminary metacognitive model. In T. Perfect & B. Schwartz, (eds.) *Applied Metacognition*. Cambridge, UK: Cambridge University Press. Pp. 121-145.

Mazzoni, G. A. L., Loftus, E. F., & Kirsch, I. (2001). Changing beliefs about implausible autobiographical events: A little plausibility goes a long way. *Journal of Experimental Psychology: Applied, 7,* 51-59.

McNally, R. J. (2003). *Remembering Trauma.* Cambridge, MA: Harvard University Press.

Mulder, R. T., Beautrais, A. L., Joyce, P. R., & Fergusson, D. M. (1998). Relationship between dissociation, childhood sexual abuse, childhood physical abuse, and mental illness in a general population sample. *American Journal of Psychiatry, 155,* 806-811.

Ost, J. (2005). Recovered memories. In T. Williamson (ed.) *Investigative Interviewing*. Cullompton, UK: Willan. Pp. 259-291.

Pekala, R. J., Kumar, V. K., & Marcano, G. (1995). Anomalous/paranormal experiences, hypnotic susceptibility, and dissociation. *Journal of the American Society for Psychical Research, 89,* 313-332.

Rhue, J. W., & Lynn, S. J. (1987). Fantasy proneness: Developmental antecedents. *Journal of Personality, 55,* 121-137.

Roediger, H. L, & McDermott, K. B. (1995). Creating false memories: Remembering words not presented on lists. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 21,* 803-814.

Scoboria, A., Mazzoni, G., Kirsch, I., & Relyea, M. (2004). Plausibility and belief in autobiographical memory. *Applied Cognitive Psychology, 18,* 791-807.

Smeets, T., Jelicic, M., Peters, M. J. V., Candel, I., Horselenberg, R., & Merckelbach, H. (2006). "Of course I remember seeing *that* film!": How ambiguous questions generate crashing memories. *Applied Cognitive Psychology, 6*, 779-789.

Spanos, N. P., Burgess, C. A., & Burgess, M. F. (1994). Past-life identities, UFO abductions, and Satanic ritual abuse: The social construction of memories. *International Journal of Clinical and Experimental Hypnosis, 42,* 433-446.

Wilson, K., & French, C. C. (submitted, a). The effects of paranormal belief and post-event misinformation on the accuracy of memory for a psychic reading.

Wilson, K., & French, C. C. (submitted, b). The effects of suggestion, social influence and paranormal belief on eyewitness testimony for an ostensibly paranormal event.

Wilson, K., & French, C. C. (2006). The relationship between susceptibility to false memories, dissociativity, and paranormal belief and experience. *Personality and Individual Differences, 41,* 1493-1502.

Wilson, S. C., & Barber, T. X. (1983). The fantasy prone personality: Implications for understanding imagery, hypnosis, and parapsychological phenomena. In A. A. Sheikh (ed.). *Imagery: Current theory, research, and application*. New York: Wiley. Pp. 340-387.

Winograd, E., Peluso, J. P., & Glover, T. A. (1998). Individual differences in suscepti-

bility to memory illusions. *Applied Cognitive Psychology, 12,* S5-S27.
Wiseman, R., & Greening, E. (2005). 'It's still bending': Verbal suggestion and alleged psychokinetic ability. *British Journal of Psychology, 96,* 115-127.
Wiseman, R., Greening, E., & Smith, M. (2003). Belief in the paranormal and suggestion in the seance room. *British Journal of Psychology, 94,* 285-297.
Wolfradt, U. (1997). Dissociative experiences, trait anxiety and paranormal beliefs. *Personality and Individual Differences, 23,* 15-19.

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# Memory Without a Trace

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**Abstract**

*Ever since Plato proposed that memories are analogous to impressions in wax, many have suggested that memories are formed through the creation of traces, representations of the things remembered. That is still the received view among most cognitive scientists, who believe the remaining challenge is simply to determine the precise physical nature of memory traces. However, there are compelling reasons for thinking that this standard view of memory is profoundly wrongheaded — in fact, disguised nonsense. This paper considers, firstly, what those reasons are in detail. Secondly and more briefly, it considers how trace-like constructs have undermined various areas of parapsychological theorizing, especially in connection with the evidence for postmortem survival-for example, speculations about cellular memory in transplant cases and genetic memory in reincarnation cases. Similar problems also emerge in areas often related to parapsychology — for example, Sheldrake's (1981) account of morphic resonance.*

## Introduction

One of the most persistent conceptual errors in philosophy, psychology, and neurophysiology is the attempt to explain memory by means of memory traces (sometimes called "engrams"). The underlying problems are very deep and difficult to dispel, and as a result, trace theories are quite seductive. In fact, in the cognitive sciences this approach

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to memory is ubiquitous and is almost never seriously questioned (for representative samples of the view, see, e.g., Damasio, 1996; Gazzaniga et al., 1998; Moscovitch, 2000; and Tulving and Craik, 2000). If doubts are raised at all, they typically concern how trace mechanisms are implemented or what the physical substrate of traces might be, not whether something is profoundly wrongheaded about the very idea of a memory trace. Moreover, positing memory traces is one aspect of a larger explanatory agenda that prevails in the behavioral sciences — namely, the tempting but ultimately fruitless strategy of explaining human behavior as if it is emitted by, and wholly analyzable in terms of, processes occurring within an agent. And one reason that agenda is so difficult to overturn is that in order to present a viable alternative, one must outline a very different approach to the analysis and understanding of human behavior.

But that last task goes well beyond the scope of this paper. My more modest goals here are (1) to summarize the main reasons for thinking that the concept of a memory trace is, not simply useless, but actually incoherent, and (2) to show, only briefly, how analogous concepts have crept insidiously into various areas of parapsychological theorizing, especially in connection with the evidence for postmortem survival — for example, speculations about cellular memory in transplant cases and genetic memory in reincarnation cases. Similar problems also undermine theorizing in areas often related to parapsychology — for example, Sheldrake's (1981) account of morphic resonance.

## Why traces?

Suppose I meet my old friend Jones, whom I haven't seen in twenty-five years. How is it, we wonder, that I'm able to remember him? Many believe that I couldn't possibly remember Jones without there being something *in* me, a trace (presumably a modification in my brain), produced in me by my former association with Jones. Without that trace, that persisting structural modification in my brain, we'd apparently have causation over a temporal gap. We'd have to suppose that I remember Jones now simply because I used to know him. And to many, that looks like magic. How could something twenty-five years ago produce a memory now, unless that twenty-five-year gap is somehow bridged? So when I remember Jones after twenty-five years, we're tempted to think it's because something in me now closes that gap, link-

ing my present memory to my past acquaintance with Jones.

Now parenthetically, I have to say that it's at least controversial (and in many instances rather naive) to suppose there's something wrong with the idea of causation over a temporal gap. Gappy causation is a problem only on the assumption that the only real causes are proximate causes (i.e., that cause and effect must be spatiotemporally contiguous). But that's a thread I can't pursue here. Positing memory traces is problematic enough quite apart from its underlying questionable picture of causation.

So, let's return to the motivation for asserting the existence of memory traces. Notice that traces aren't posited simply to explain how I happen to be in the particular states we identify as instances of remembering — for example, my experiencing a certain mental image of Jones. They're supposed to explain how memory is *possible* in the first place. The idea is that without a persisting structural modification in me, caused by something in my past — in this case, presumably, a physiological representation of Jones, no state in me *could* be a memory of Jones. So if after twenty-five years I have a mental image of Jones, the only way that image could count as a memory of Jones would be if it had the right sort of causal history. And the right sort of causal history, allegedly, is one that spatially and temporally links my present experience with my past acquaintance with Jones. So my image of Jones counts as a memory of Jones only if (1) there's a trace in me, caused by my previous acquaintance with Jones, and (2) the activation of that trace is involved in producing my present image of Jones. So mental images of Jones might be possible without that sort of causal history, but they wouldn't then be instances of remembering.

History has proven that this general picture of remembering is initially very attractive. But it gets very ugly very quickly, as soon as one asks the right sorts of questions. (In my view, this is where philosophy is most useful, and often the most fun: showing how claims which seem superficially plausible crumble as soon as their implications or presuppositions are exposed). What eventually becomes clear is that the idea of memory as involving *storage* is deeply mistaken, and that the mechanism of storage, memory traces conceived as representations of some kind, can't possibly do the job for which they're intended. This is actually an enormous topic and one of the most interesting subjects in the philosophy of mind. But since this issue is both vast and only part of what I want to discuss, I can't do more here than outline a few of

the problems with the concept of a memory trace and indicate where one might look for additional details. (For extended critiques, see Bennett and Hacker, 2003; Braude, 2002; Bursen, 1978; Heil, 1978; Malcolm, 1977.)

## More preliminaries

The first thing to note is that the problems with the concept of a memory trace are *hardware-independent*. It doesn't matter whether traces are conceived as mental or physical, or more specifically as static, dynamic, neurological, biochemical, atomic, subatomic, holographic (á la Pribram), nonspatial mental images, or (as Plato suggested) impressions in wax. No matter *what* memory traces are allegedly composed of or how they're purportedly configured, they turn out to be impossible objects. Memory trace theory requires them to perform functions that nothing can fulfill. So my objections to trace theory have nothing to do specifically with the fact that those theories are typically physiological or physical. Rather, it's because they're *mechanistic* and (in particular) because the mechanisms they posit can't possibly do what's required of them.

Before getting into details, I must deflect a certain standard reaction among scientists to the sort of criticisms I'm making here. Many have complained to me that, as scientists, they're merely doing empirical research, and so it's simply beside the point to argue, *a priori*, that their theories are unintelligible or otherwise conceptually flawed. However, I'm afraid that this response betrays a crucial naivete about scientific inquiry. There is no such thing as a purely empirical investigation. Every branch of science rests on numerous, often unrecognized, abstract (i.e., philosophical) presuppositions, both metaphysical and methodological. These concern, for example, the nature of observation, properties, or causation, the interpretation, viability, and scope of certain rules of inference, and the appropriate procedures for investigating a given domain of phenomena. But that means that the integrity of the discipline as a whole hinges on the acceptability of its root philosophical assumptions. If those assumptions are indefensible or incoherent, that particular scientific field has nothing to stand on, no matter how attractive it might be on the surface. And I would say that several areas of science, as a result, turn out simply to be bad philosophy dressed up in obscurantist technical jargon, so that the elementary nature of their mistakes

remains well-hidden. Memory trace theory is just one example of this. And I'd argue that today's trace theories of memory, for all their surface sophistication, are at bottom as wrongheaded and simplistic as Plato's proposal that memories are analogous to impressions in wax. In short, I'd say they are disguised nonsense.

Two more disclaimers, before outlining my objects to trace theory. First, when I say that the concept of a memory trace is incoherent or that trace theory is conceptually naive in certain respects, I'm not saying that trace theories — or the scientists who hold them — are stupid. To say that a proposal or concept is nonsensical or incoherent is simply to say it makes no sense. Now although the world isn't suffering a shortage of stupidity, not all nonsense is stupid. In fact, the most interesting nonsense is *deep* nonsense, and it's something which can all too easily deceive even very smart people. That's because the problematic assumptions are buried well below the surface and require major excavation.

Second, I've learned over the years that when I outline my objections to trace theory, many hear me as suggesting that the brain has nothing to do with memory. I'll say a bit more about this later, but for now I'll just note that I'm saying nothing of the kind — although evidence for postmortem survival *would* seriously challenge this. In fact, let's overlook for now complications to all physiological cognitive theories posed by the evidence for postmortem survival and restrict our attention to embodied humans. In those cases, clearly, the capacity to remember is causally dependent, not simply on having a functioning brain, but probably also on changes to specific areas of the brain. However, it's one thing to say that the brain *mediates* the capacity to remember, and another to say it *stores* memories. The former view (more likely the correct one) takes the brain to be an instrument involved in the expression of memory; the latter view turns out to be deeply unintelligible. For a very limited analogy, we can say that while a functionally intact instrument may be causally necessary for performing a musical improvisation, the music is not stored in the instrument (or anywhere else).

## The horns of a dilemma

So why is the concept of a memory trace incoherent? Let's begin with an analogy (drawn from John Heil's outstanding critique of trace

theory — Heil, 1978). Suppose I invite many guests to a party, and suppose I want to remember all the people who attended. Accordingly, I ask each guest to leave behind something (a trace) by which I can remember them. Let's suppose each guest leaves behind a tennis ball. Now clearly I can't use the balls to accomplish the task of remembering my party guests. For my strategy to work, the guests must deposit something reliably and specifically linked to them, and the balls obviously aren't differentiated and unambiguous enough to establish a link only with the person who left it.

So perhaps it would help if each guest signed his/her own tennis ball, or perhaps left a photo of him/herself stuck to the ball. Unfortunately, this threatens an endless regress of strategies for remembering who attended my party. Nothing reliably (much less uniquely and unambiguously) links the signature or photo with the guest who attended. A guest could mischievously have signed someone else's name, or left behind a photo of another person. Or maybe the signature was illegible (most are), or perhaps the only photo available was of the person 25 years earlier (e.g., when he still had hair, or when he had a beard, wore eyeglasses, and was photographed outdoors, out of focus and in a thick fog), or when he was dressed in a Halloween costume or some other disguise.

But now it looks like I need to remember in order to remember. A tennis ball isn't specific enough to establish the required link to the person who left it. It's not the sort of unambiguous representational calling card the situation requires. So we supposed that something else might make the tennis ball a more specific link — a signature or a photo. That is, we tried to employ a secondary memory mechanism (trace) so that I could remember what the original trace (the tennis ball) was a trace of. But the signature and photo are equally inadequate. They too can't be linked unambiguously to a specific individual. Of course, if I could simply *remember* who wrote the signature or left behind the photo, then it's not clear why I even needed the original tennis balls. If no memory mechanism is needed to make the connection from tennis ball to party guest, or illegible signature to its author, then we've conceded that remembering can occur without corresponding traces, and then no trace was needed in the first place to explain how I remember who attended my party. So in order to avoid that fatal concession, it looks like yet another memory mechanism will be required for me to remember who left behind (say) the illegible or phony signature, or the fuzzy photo. And

off we go on a regress of memory processes. It seems that no matter what my party guests leave behind, nothing can be linked only to the guest who left it. We'll always need something else, some other mechanism, for making the connection between the thing left behind and the individual who left it.

In fact, it seems that the only way to stop the regress is for a guest to leave behind something that is *intrinsically* and exclusively linked only to one individual. That is why Wolfgang Köhler, for example, proposed that traces must be *isomorphic* with the things of which they're traces — that is, the things they represent (e.g., Köhler, 1947, 1969). But what Köhler and others have failed to grasp is that this kind of intrinsic connection is impossible, because nothing can function in one and only one way. As I'll argue shortly, this is especially clear when the function in question is one of representation or meaning. Nothing can represent unambiguously (or represent one and only one thing); representing is not something objects can do all by themselves; and representation can't be an intrinsic or inherent relation between the thing represented and the thing that represents it.

Interestingly, although Köhler failed to see why trace theory is doomed to fail, he was remarkably clear about what trace theory requires. Köhler understood that a major hurdle for trace theory is to explain trace *activation* — that is, how something present triggers my trace of Jones, rather than the trace of someone else. And that's a serious problem, because what triggers a memory (or activates a trace) can be quite different from what established it in the first place. So Köhler wrote,

> "...recognition... means that a present fact, usually a perceptual one, makes contact with a corresponding one in memory, a trace, a contact which gives the present perception the character of being known or familiar. But memory contains a tremendous number of traces, all of them representations of previous experiences which must have been established by the processes accompanying such earlier experiences. Now, why does the present perceptual experience make contact with the *right* earlier experience? This is an astonishing achievement. Nobody seems to doubt that the *selection* is brought about by the similarity of the present experience and the experience of the corresponding earlier fact. But since this earlier experience

> is not present at the time, *we have to assume that the trace of the earlier experience resembles the present experience, and that it is the similarity of our present experience (or the corresponding cortical process) and that trace which makes the selection possible*."
>
> (Köhler, 1969, p. 122, emphasis added)

By the way, this passage reveals another serious limitation of trace theory, one I can only mention in passing here. If trace theory has any plausibility at all, it seems appropriate only for those situations where remembering concerns past *experiences*, something which apparently could be represented and which also could resemble certain triggering objects or events later on. But we remember many things that aren't experiences at all, and some things that aren't even past — for example, the day and month of my birth, the time of a forthcoming appointment, that the whale is a mammal, the sum of a triangle's interior angles, the meaning of "anomalous monism." Apparently, then, Köhler's point about trace activation and the need for similarity between trace, earlier event, and triggering event, won't apply to these cases at all. So even if trace theory was intelligible, it wouldn't be a theory about memory generally.

In any case, trace theory is not intelligible, and Köhler's observation reveals why. To avoid the circularity (and potential regress) of positing the ability to remember in order to explain my ability to remember (e.g., by requiring further trace mechanisms to enable the previous trace do its job), we must suppose that some trace uniquely and unambiguously represents or connects to the original experience. And because unambiguous representation is an impossible process, trace theory is caught between two fatal options. I'll explain in a moment why unambiguous representation is impossible, but first, we need to observe that the tennis ball/party example hides a further complication noted in the passage from Köhler.

Traces are usually supposed to be brain processes of some sort, some physiological representation produced, in this case, by a party guest. But what *activates* this trace later can be any number of things, none of which need to resemble the experience, object, or event that produced the original trace. Suppose Jones attended my party. Trace theory requires my experience of Jones at the party to produce a representation in me of Jones (or my experience of him) so that I can later remember that he was at the party. But what will subsequently activate that trace?

It could be Jones himself, or an image of Jones, or the lingering smell of someone's cologne, or a telltale stain on the carpet, or perhaps someone asking, "Who was at the party?" Of course, some of these potential triggering objects or events might plausibly be said to resemble the thing that originally produced the trace. But how can (say) the smell of cologne, a stain, or the words "Who was at the party?" trigger the trace of Jones created by his presence at the party? These things aren't obviously similar to Jones himself. If we posit another memory *mechanism* to explain how I draw the connection between the cologne and Jones (e.g., he may have worn it, spilled it, or simply talked about it), or how the question "Who was at the party?" leads me to the right party and not some other party, or even how I remember what the word "party" means, we're starting a regress of memory mechanisms. But if we say it's because I can simply *remember* who wore (or perhaps mentioned) the cologne, stained the carpet, or who my party guests were, then we're still reasoning in a circle. We're still explaining memory by appealing to the ability to remember. Moreover, if I can remember these things without some further trace, then we didn't need a trace in the first place to explain my ability to remember that Jones was at the party. However, if we follow Köhler's lead, then we have to assert some kind of intrinsic similarity or resemblance, some kind of psychophysical *structural isomorphism*, between three things: the original experience or event, the trace produced on that occasion, and the subsequent triggering events.

If nothing else, it should make you suspicious that a representation of Jones at the party will be isomorphic both to Jones (or my experience of him) and to the innumerably many and quite different things that can later activate the trace — for example, a particular scent or a sequence of sounds. What kind of similarity could this be? The answer is that it can't be any kind of similarity and that Köhler's proposal is literally meaningless. As tempting as it is to continue for a while enumerating the problems with trace theories, I'll restrict myself now to two more points, to explain perhaps the deepest confusion underlying these theories.

The first problem is with the very idea of structural isomorphism. The term "structural isomorphism" sounds impressive and scholarly, but in trace theories the appeal to structural isomorphism is really just the appeal to an *inherent similarity* between two things, *determined solely by their respective structures*. Traces must be produced in a way that relates them structurally to the things of which they're traces, and they

must be activated only by things having the right underlying structure. Moreover, that activation must be determined solely by intrinsic relations between the structures of the trace and the things that activate them. Otherwise, we'd need another mechanism to explain how the *right* trace is activated in the presence of a trigger that could just as well have been isomorphic with (or mapped onto) something else. And that raises the circularity or regress problem noted earlier.

But the alternative, inherent similarity, makes no more sense than saying that a square is a circle. Inherent similarity is a *static* relation obtaining only between the similar things. And it must hold between those things *no matter what*. If, for example, context could alter whether two things count as similar, then those things are not similar merely in virtue of intrinsic relations holding between their respective structures. But that's why intrinsic similarity is nonsense. Similarity exists only with respect to variable and shifting criteria of relevance. It can only be a dynamic relation holding between things at a time and within a context of needs and interests.

A simple example from geometry should make the point clearly. Consider the five geometric figures in Figure 1.

Now consider the question: To which of the last four figures is the triangle (a) similar? The proper response to that question should be puzzlement; you shouldn't know how to answer it. Without further background information, without knowing what matters in our comparison of the figures, the question has no answer at all. Mathematicians recognize this, although instead of the term "similarity" they use the expression "congruence." In any case, mathematicians know that in the absence of some specified or agreed-upon rule of projection, or function for mapping geometric figures onto other things, no figure is congruent with (similar to) anything else.

Mathematicians recognize that there are different standards of congruence, appropriate for different situations. But no situation is *intrinsically basic*, and so no standard of congruence is inherently privileged or more fundamental than others. For example, engineers might sometimes want to adopt a fairly strict mapping function according to which (a) is congruent only with other figures having the same interior angles and the same horizontal orientation. But in that case, (a) would be congruent with none of the other four figures. Of course, only in very specialized contexts are we likely to compare figures with respect to their horizontal orientation. In many situations it would be appro-

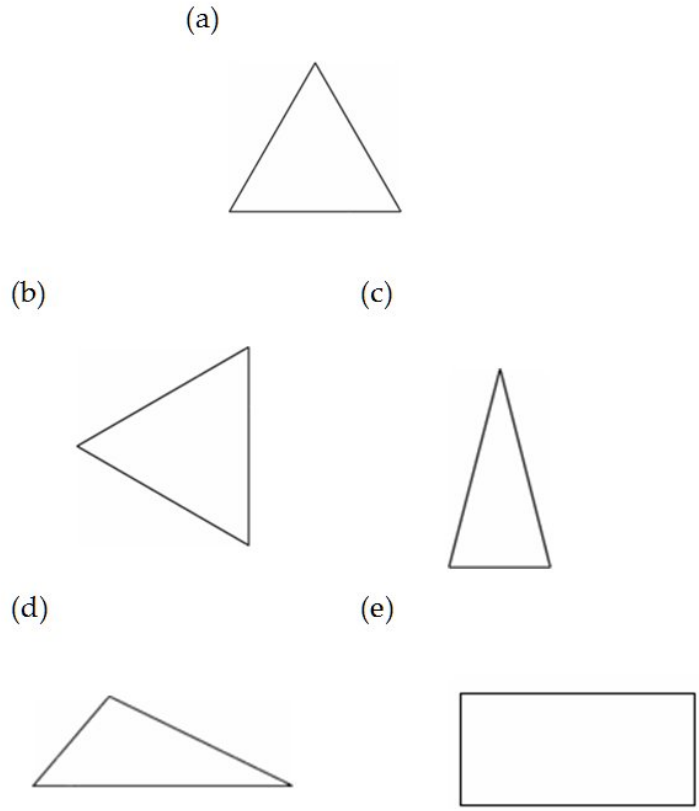


*Figure 1.* Five simple geometric figures

priate to adopt a different standard of congruence, according to which sameness of interior angles is all that matters. And in that case we'd say that figures (a) and (b) are congruent but that (a) is not congruent with the other figures. However, there's also nothing privileged about sameness of interior angles. Perhaps what matters is simply that (a) is congruent with any other three-sided enclosed figure, in which case we could say it's congruent with the three triangles (b)–(d), but not with the rectangle (e). But even that criterion of congruence can be modified or supplanted. Mathematicians have rules of projection that map triangles onto any other geometric object, but not to (say) apples or oranges. Of course, the moral here is obvious. If simple geometric figures are not intrinsically similar — that is, if they count as similar only against a background of assumptions about which of their features matter (i.e., are relevant), then we certainly won't find intrinsic similarity with much more complex objects — in particular, memory traces and the various objects or events that allegedly produce and activate them.

But maybe you're still not convinced. Perhaps you think that there *is* a fundamental principle of congruence for this geometric example. You might think that, first and foremost, (a) is similar to just those figures with sides of exactly the same length, the same horizontal orientation, and with exactly the same interior angles. And perhaps you'd want to call that something like "strict congruence (or identity)." But there are at least three serious problems with that position.

First, even if this sort of congruence counted as more fundamental than other forms of geometric similarity, that could only be in virtue of a kind of historical accident. The primacy of that standard of congruence would reveal more about us, our conventions and values — in short, what merely happens to be important to us, than it does about the figures themselves. In fact, it's a standard appropriate for only a very narrow range of contexts in which we consider whether things are similar. Second (and as an illustration of that first point), it's easy to imagine contexts in which two triangles have exactly the same interior angles, horizontal orientation, and sides, but don't count as similar. If we're interior designers, for example, it might also matter whether the triangles are of the same color, or whether they're placed against the same colored background, or whether they're made of the same material. If we're graphic artists, it might matter whether the triangles were both original artworks or whether one was a print. Or if we're librarians or archivists, it might matter whether the triangles occur on the same page of different copies of the same book. And third, even if we could decide on some very strict sense of congruence (or identity) which would count as privileged over all other forms of similarity, it would be useless in the present context. Memory traces are never strictly identical either with the things that produce them or with the things that activate them. The looser and more complex forms of similarity at issue in trace theories are classic examples of the sorts of similarities that can't possibly be inherent, static relations between things.

And as if that weren't enough, another aspect of this general confusion about similarity is the requirement that traces and other things have intrinsic or inherent *structures* — that is, some context-independent parsing into basic elements. Because isomorphism (mapping) is tied to structural elements of the isomorphic things, that's a necessary condition for intrinsic isomorphism to hold between the trace and the things it represents. After all, if what counted as structure depended on context — that is, if a trace could just as well have been parsed differently and

assigned alternative structures, then it could be mapped onto (or count as similar to) different things. And unfortunately for trace theory, objects and events can always be parsed in an indefinite number of ways, and whatever parsing we select can only be conditionally, and never categorically or intrinsically, appropriate. We always determine a thing's components relative to a background against which certain features of the things (but not others) count as relevant. But then it's only against shifting and non-privileged background criteria of relevance that we take two things to have the same structure; they are never isomorphic *simpliciter*.

So the trace theorist's inevitable appeal to privileged, inherent structures and intrinsic mappings is literally absurd. It's on a par with claiming that a pie has a basic context-independent division into slices or elements, or that there's an absolutely context-independently correct and privileged answer to the questions, "How many events were there in World War II?" and "How many things are in this room?"

## Confusions about representation

The appeal to inherent similarity or structure is merely a specific form of a more pervasive problem in the so-called cognitive sciences — namely, confusions about and equivocations on the term "representation." Traces are supposed to represent their causes, the events or experiences that produced them, and they must be internally and structurally differentiated in ways that correspond to the different things we remember. This is one version of the general view that distinct mental states are caused by (or are identical to) certain corresponding distinct internal physical states, and that what those different internal states *are* (i.e., what they represent) is explainable wholly in terms of their distinctive structural features. At this point, cognitive scientists typically do a lot of hand waving and say something like, "We may not currently know all the details, but presumably some super psychology of the future (or perhaps God) could in principle look inside our heads and know, from the way we're configured, what we're thinking."

However, this general picture rests on the utterly false assumption that a thing's representational properties can be determined solely by its structural or topological features. I've examined this error in considerable detail elsewhere (Braude, 1997, 2002). For now, a few brief remarks will have to suffice.

To see what's wrong, we need to appreciate that *anything can represent anything*. In fact, a thing's representational options are limited only by the situations into which it can be inserted. And if that's the case, then what something represents can't simply be a function of how it's configured. Things must be *made* to represent or mean something. Suppose I'm trying to teach a child the alphabet. I show him a picture of a dog and I say "D is for dog." In that case, we might say that the picture represents the class of dogs. But I could have said, "C is for collie," and in that case the picture would have represented a subset of the set of dogs. Similarly, I could have said "L is for Lassie," in which case the picture would have represented an even smaller subset of dogs. I could also have said "Z is for Ziggie," referring to the child's pet collie. And notice, these changes in what the picture represents have nothing whatever to do with corresponding changes in the arrangement of pixels, or atoms, or anything else in the picture. Those structural features of the pictures remained the same in all cases. What the picture represents depended instead on how it was used.

And in fact, the picture's representational properties could be changed even more dramatically. My disgruntled students could make the picture represent me and symbolically express their hostility toward me by using it as target for darts. Or I could jokingly point to the picture and say "This was Joan Rivers before plastic surgery." Or suppose I'm trying to give directions to someone without the aid of a map. I could place the picture on a table and say, "This is the shopping center, this [a ham sandwich] is the hospital, this [my fork] is the access road, and this [a salt shaker] is the water tower."

Of course, contexts in which (say) a sandwich represents a building, or in which a picture of a dog represents a distinguished philosopher (or over-the-hill comedienne), are atypical in some respects. But those situations are unusual *only* with respect to what the objects represent. They aren't at all unusual with respect to how representational properties are acquired. And it doesn't matter whether we're talking about images, words, or (say) synaptic connections. In every case (familiar and offbeat), what a thing represents depends ultimately on the way we place it in a situation. There are no context-independent forms of representation or meaning. So when it comes to examples like the picture of a dog or the ham sandwich, the mistake many make is to think that some representational properties — the familiar and apparently default ones — are inherently fundamental and that others are anomalous. That is,

they believe that representation in familiar cases is somehow built-into or hardwired into the representing objects, and that this inherent function simply gets *overridden* in the more unusual cases. But in fact, the familiarity of certain contexts reveals more about us, about our patterns of life and our interests, than it does about the objects themselves. If our form of life were radically different, the default or familiar representational properties of objects could change accordingly.

But then if a brain structure (say) is to represent something past and function as a memory trace, it can't do so solely in virtue of its structural features. Nothing represents or means what it does on topological grounds alone. However, the whole point of Köhler's principle of psychophysical isomorphism (or related hypotheses in the cognitive sciences) is to tie what a thing represents solely to its structure. That was the only way to avoid the equally fatal error of requiring a regress of mechanisms to explain how the original mechanism or state can do its job. So this, too, turns out to be a dead end.

## Tokens and types

But let's return more explicitly to trace theory. A related, and equally unheralded problem with such theories is that traces and their causes or activators are of radically different ontological kinds, and the sort of thing traces have to be is a kind that many think is simply a philosophical fiction. At any rate, it's nothing but a philosophical move, not even remotely a scientific move, to posit the existence of traces. Hopefully, one distinction and one more example will make this clear.[1]

Trace theorists have always been tempted to regard traces as kinds of *recordings* of the things that produced them. In fact, some previous influential writings on memory compared traces to tape recordings or grooves and bumps in a phonograph record. The justification for that idea, as we've seen, is that traces must somehow capture essential structural features of the things that produce them. However, the poverty of this view is easy to expose.

Consider: One of the things I remember is Beethoven's Fifth Symphony (hereafter abbreviated as B5). Modern versions of trace theory require that my memory is explained in terms of a representation of B5, stored in some form in my brain and produced in me by the experience of hearing B5 in the past. This trace must have certain structural

[1]For a considerably more detailed presentation of the following arguments, see Bursen, 1978.

or topological properties that link it to the thing(s) that caused it, properties which also distinguish it from traces of other pieces of music. So presumably this trace of B5 was produced by and captures features of a performance I heard of B5. But which features? Tempo, rhythm, pitch, length of notes, instrumental timbre, dynamic shadings? You'd think so if my trace of B5 was produced by and represents or records a B5 performance, and also if that trace is to differ (say) from my trace of Beethoven's Fourth (B4) or "Yankee Doodle.". But I (like many others) can remember B5 by recognizing a wide variety of musical performances as *instances* (or as philosophers would put it, *tokens*) of B5. For instance, I could recognize B5 when certain notes are held for an unusually long time, or when it's played with elaborate embellishments, or with poor pitch and many mistakes by an amateur orchestra. In fact, I could recognize truly outlandish musical events as instances of B5 — for example, when it's played extremely slowly or rapidly, or with tempo changing every bar, or with arbitrary notes raised a major sixth, or when it's played with inverted dynamics, or played only on kazoos, banjos, or tubas. Similarly, I could recognize a series of percussive taps as a pitch-invariant version of the opening bars of B5.

But this means that the trace is not a recording. On the contrary, it must be a very unusual sort of entity. Whereas the remembered and triggering events or experiences are concrete event-tokens, the trace itself must be a relentlessly abstract object — what philosophers call a *type*. And it has to be so abstract that it can't contain *any* features found in the performances or experiences that produced it (e.g., precise rhythm, pitch, etc.). If it had those features, we'd need to posit another mechanism to explain how my trace can be activated by tokens of B5 lacking them — for example, a tuba-only performance of B5 played at quarter speed with many wrong notes. But if we try to prevent a regress by saying that I can simply recognize that the tuba-only version is an instance of B5, then we don't need to posit the trace of B5 at all. We've conceded that I can remember B5 without recourse to a B5 trace.

But now look at what has happened. We've seen that the B5 trace is an abstract type. However, that trace has to have *some* features in virtue of which it's a B5 trace and not a trace of (say) Beethoven's Fourth, the "Waldstein" Sonata, or "Yankee Doodle." But it can't have features found in any specific instances of B5, because none of those are necessary for a musical event to be an instance of B5 capable of either producing or activating the trace. So the B5 trace somehow needs to have

features necessary and sufficient for being an abstract B5 and not (say) a B4, but without having any specific features regarding pitch, tempo, dynamics, etc., all of which can be changed or absent (perhaps you can now see why many consider abstract types impossible objects).

In any case, we've arrived at the point where we see the ultimately nonscientific nature of trace theory. It's committed to the view that a memory trace and all its concrete instances have a structure that is essential to all things that are instances of B5, but none of the specific features which versions of B5, including the nightmare versions, can lack. This position is commonly called *Platonic essentialism* — the view that things are of the same kind in virtue of sharing a common underlying, but abstract, structure. And that's not a scientific view at all. It's a philosophical view, and a bad one at that.

## The abuse of memory in parapsychology

It's unfortunate enough that memory trace theory is received dogma in the cognitive sciences. Almost no one seems to doubt that memories are somehow stored and encoded in us. So it's not surprising that this picture of memory has found its way to more overtly speculative or frontier areas of science, including parapsychology. No doubt it's very tempting for parapsychologists to posit trace-like processes in their own theories, because they will at least appear to be reasoning along scientifically orthodox lines, even if the subject matter itself falls outside the scientific mainstream.

For example, Roll has proposed a "psi structure" theory of survival, modeled explicitly after memory trace theory, and according to which memory traces are left, not simply in individual brains, but in our environment as well (Roll, 1983). Of course, this escapes none of the classic problems of trace theory, because on Roll's view, what certain structures represent (or are similar to) remains unintelligibly tied to inherent features of those structures. This is especially problematical when Roll suggests that an individual mind or personality is a system of such structures. That's no more plausible than saying that we can tell whether a person is thinking about his grandmother just by examining the state of his brain, or that a picture of a dog represents something specific independent of its use in a context. It requires brain or mental structures to mean or represent something simply in virtue of how they're configured, never mind their dynamic position within an equally dynamic

life situation. Roll also proposes explaining ESP as the responding to memory traces left on objects by previous guesses. But that seems no more credible than supposing that I could remember my party guests from looking simply at the tennis balls they left behind, or the illegible signatures or photos they left along with the balls.

Trace theory also appears in other guises in connection with the evidence for postmortem survival. One is the suggestion that reincarnation cases can be explained in terms of genetic memory. However, I've found no serious researcher making that suggestion. It seems, instead, to be entertained simply as a real possibility, albeit one that can be rejected on empirical grounds (see, e.g., Almeder, 1992; Stevenson, 1974). That is, it's treated as if it's an intelligible position that happens merely to be inadequate to the data. Another application of trace theory to survival is the attempt to explain transplant cases by appealing to cellular memory (e.g., Pearsall et al., 1999). No doubt the reason it's tempting here to posit genetic or cellular memory traces is that in reincarnation and transplant cases, complex psychological regularities seem to persist in the absence of the usual presumed bodily correlates. So to those for whom it's unthinkable that memories could persist without being stored somewhere, it might seem reasonable to propose that memories and personality traits can be encoded in a kind of hardware that has nothing to do with the brain. However, since the problems noted earlier with trace theories are hardware-independent, it's an insignificant change merely to relocate the traces in different physical systems. It's still untenable to suppose that representation, meaning, or similarity, are determined solely by a thing's topological features.

To me, it's interesting that when the usual suspect — the brain — isn't available as the locus of memory storage, some find it inevitable that memories must simply be located in a different place or perhaps in a modified form. It demonstrates just how deeply mechanistic assumptions have taken root, and in a way, it shows a profound lack of scientific imagination. The situation here closely parallels what happened in response to Lashley's famous experiments in the 1920s (Beach et al., 1960; Lashley, 1929, 1950). When Lashley found that no matter how much of a rat's brain he surgically removed, trained rats continued to run their maze, some concluded that the rats' memories weren't specifically localized in their brains. Instead, they suggested that the memories were diffusely localized, much as information is diffusely distributed in holograms (Pribram, 1971; Pribram et al., 1974; Pribram, 1977). But to

someone not antecedently committed to traditional mechanistic dogma, Lashley's experiments take on a different sort of significance, perhaps similar to that of the evidence for postmortem survival. They suggest that memories are not located anywhere or in any form in the brain. And more generally, they suggest that the container metaphor (that memories and mental states in general are *in* the brain, or in something else) was wrong from the start. Of course, that's what my arguments in the preceding sections were intended to show.

Another variant of this general error emerges in Rupert Sheldrake's (1981) suggestion that morphic fields capture the essential structure of developmental forms and even behavioral kinds. Although Sheldrake thought he was escaping the evils of mechanistic theories with his view, in fact he retained the underlying errors of supposing that similarity is an intrinsic structural relation between things, and that things of the same kind are of that kind because they share a common underlying structural essence. The claim that behavioral kinds, such as feeding behavior and courtship, can be captured in strictly structural terms, is especially implausible.[2]

## Summing up

I realize that I'm pretty much a voice in the wilderness on these issues, and I find myself in the unenviable position of having to argue that many prominent and respected scientists actually don't know what they're talking about. I wish there were some other, less fundamentally upsetting, way to undercut trace theories of memory. But I believe that the problems really are that deep and that the theories really are that essentially confused.

However, as long as I'm being antagonistic, I see no compelling reason to stop where I left off. I might as well finish with brief obnoxious coda. As I see it, both memory researchers and parapsychologists are missing an opportunity to be genuine scientific pioneers. Rather than boldly searching for new explanatory strategies (for memory specifically and for human behavior generally), they cling instead to familiar mechanistic presuppositions, which they've typically never examined in any depth, but by means of which they can maintain the illusion that they're doing science according to the allegedly tough-minded methods exemplified in some physical sciences. (Sherry Turkle has appropriately

[2]For a detailed critique of Sheldrake's theory, see Braude (1983).

called this "physics envy.") They can't get past the assumption that human abilities and behavior must be analyzed in terms of lower-level processes and mechanisms. And they seem not to recognize the difference between claiming that cognitive functions are *analyzable* in terms of underlying physical processes and claiming instead that those functions are merely *mediated* by underlying physical processes. But there are novel explanatory options and strategies they never consider; there are alternative and profoundly different approaches to the understanding of human beings. However, spelling out those options is a huge project, one that must be reserved for another occasion.

## References


Almeder, R. (1992). *Death and Personal Survival*. Lanham, MD: Rowman & Littlefield.

Beach, F.A., Hebb, D.O., Morgan, C.T., and Nissen, H.W. (eds.) (1960). *The Neuropsychology of Lashley: Selected Papers of K. S. Lashley*. New York: McGraw-Hill.

Bennett, M.R. and Hacker, P.M.S. (2003). *Philosophical Foundations of Neuroscience*. Oxford: Blackwell.

Braude, S.E. (1983). "Radical Provincialism in the Life Sciences: A Review of Rupert Sheldrake's *A New Science of Life*." *Journal of the American Society for Psychical Research, 77,* 63–78.

Braude, S.E. (1997). *The Limits of Influence: Psychokinesis and the Philosophy of Science*. Lanham, MD: University Press of America.

Braude, S.E. (2002). *ESP and Psychokinesis: A Philosophical Examination (Revised Edition)*. Parkland, FL: Brown Walker Press.

Bursen, H.A. (1978). *Dismantling the Memory Machine*. Dordrecht, Boston, London: D. Reidel.

Damasio, A.R. (1996). *Descartes' Error: Emotion, Reason and the Human Brain*.

Gazzaniga, M.S., Mangun, G.R., and Ivry, R.B. (1998). *Cognitive Neuroscience: The Biology of the Mind*. New York: Norton.

Heil, J. (1978). "Traces of Things Past." *Philosophy of Science, 45,* 60–67.

Köhler, W. (1947). *Gestalt Psychology*. New York: Liveright.

Köhler, W. (1969). *The Task of Gestalt Psychology*. Princeton: Princeton University Press.

Lashley, K.S. (1929). *Brain Mechanisms and Intelligence*. Chicago: University of Chicago Press.

Lashley, K.S. (1950). "In Search of the Engram." *Symposia of the Society for Experimental Biology, 4,* 454–482.

Malcolm, N. (1977). *Memory and Mind*. Ithaca: Cornell University Press.

Moscovitch, M. (2000). "Theories of Memory and Consciousness." In E. Tulving and F.I.M. Craik (eds), *The Oxford Handbook of Memory* (p. 609-626). Oxford: Oxford University Press.

Pearsall, P., Schwartz, G.E.R., and Russek, L.G.S. (1999). "Changes in Heart Transplant Recipients That Parallel the Personalities of Their Donors." *Integrative Medicine, 2,* (2/3), 65–72.

Pribram, K.H. (1971). *Languages of the Brain* . Englewood Cliffs, N.J.: Prentice Hall.

Pribram, K.H. (1977). "Holonomy and Structure in the Organization of Perception." In U.M. Nicholas (Ed.), *Images, Perception and Knowledge*. Dordrecht: Reidel.

Pribram, K.H., Nuwer, M., and Baron, R.U. (1974). "The Holographic Hypothesis of Memory Structure in Brain Function and Perception." In D.H. Krantz, R.C. Luce, and P. Suppes (eds), *Contemporary Developments in Mathematical Psychology, Vol. 2*. San Francisco: Freeman.

Roll, W.G. (1983). "The Psi Structure Theory of Survival." In W.G. Roll, J. Beloff, and R. White (eds), *Research in Parapsychology 1982* (p. 117-120). Metuchen, NJ & London: Scarecrow Press.

Sheldrake, R. (1981). *A New Science of Life: The Hypothesis of Formative Causation.* London: Blond & Briggs Limited.

Stevenson, I. (1974). *Twenty Cases Suggestive of Reincarnation, 2nd Ed. Rev*. Charlottesville: University Press of Virginia.

Tulving, E. and Craik, F.I.M. (eds.) (2000). *The Oxford Handbook of Memory*. Oxford: Oxford University Press.