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Differential responses to psi versus non-psi stimuli: An event-related fMRI study

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Abstract

In its most basic instantiation, the psi hypothesis asserts the anomalous presence of knowledge. A direct and promising strategy to quantify potential psi effects lies in powerful neuroimaging approaches that make few assumptions about the nature of such knowledge. By circumventing behavior and directly examining brain activity we can increase the likelihood of uncovering a psi effect, better disambiguate normal from paranormal interpretations, and decrease the ambiguity of a null result. In this study, we relied upon the robust finding within cognitive neuroscience that prior knowledge of a stimulus biases the brain's response to that stimulus, even when the prior knowledge is unconscious. We hypothesized that knowledge acquired through psi would similarly prime neuronal activity, and that evidence of priming associated with psi stimuli would offer compelling evidence for the existence of psi. Sixteen participants completed a simple binary guessing task while their brain activity was monitored using functional magnetic resonance imaging (fMRI). In this task, participants viewed two sequential photographs and were asked to decide which image would shortly be chosen randomly by the computer; after participants indicated their choice on each trial, they were shown the target stimulus from the pair a second time. Outside the scanner, an emotionally-close or biologically-related partner viewed only the target stimuli. Our experimental protocol had three potential sources of psi stimuli, corresponding to the three hypothesized mechanisms of anomalous information acquisition: precognition, telepathy, and clairvoyance. We analyzed Blood Oxygen Level-Dependent (BOLD) activity associated with target versus decoy stimuli in search for evidence of psi. No such evidence was discovered; against the psi hypothesis, psi and non-psi stimuli evoked indistinguishable hemodynamic responses.

Introduction

"But it is a miserable thing for a question of truth to be confined to mere presumption and counter-presumption, with no decisive thunderbolt of fact to clear the baffling darkness."

- William James (1896)

Parapsychology is the scientific investigation of apparently paranormal mental phenomena (e.g. telepathy, clairvoyance, precognition), or psi. Despite over 75 years of experimental parapsychology, we still do not have a clear answer to its fundamental question: Does psi exist? To answer this question, researchers must actively explore new methods for testing the psi hypothesis, methods that incorporate the intellectual and technological gains of modern psychology. In this paper, we propose one such method that we believe will ultimately resolve the psi debate, and present results from one implementation of this new method.

The Importance of Parapsychology

The cogent study of anomalous phenomena fundamentally renovates science (Kuhn, 1962), and psychology is no exception. Indeed, some of the most informative advances in our understanding of cognition and perception stem from the explication of seemingly aberrant events: instances when people remember things that they shouldn't (false memory), instances when people feel things that they shouldn't (phantom limbs), instances when people see things that aren't there (visual illusions), and instances when people don't see things that are there (change blindness). Perhaps instances where people know things that they shouldn't (psi) belongs in this category. Strong, objective evidence of psi would force a revision of fundamental assumptions within psychology, as well as philosophy, physics, and many other academic domains. A convincing lack of evidence for psi under ideal methodological conditions (e.g. high statistical power, "conservative" psi hypothesis), would bolster psychological and sociological explanations for paranormal phenomena that exclude the possibility of their existence.

In addition, the study of parapsychology is of indisputable public interest. A recent survey revealed that 60% of the American population believes in the existence of "psychic powers or ESP" (NSF, 2001). Widespread belief in psi does not offer any evidence of its existence, but it presents a stark data point that demands explication. Of course in the same survey, the public also expressed widespread belief in the existence of many paranormal or supernatural phenomena, such as angels, fairies, or God. Psi differs from these other phenomena, however, in that it is clearly amenable to scientific inquiry. The methods of science, specifically cognitive neuroscience, provide us with the ability to satisfactorily answer the question "Does psi exist?" in a way that they don't for the questions "Do angels exist?" or "Does God exist?" Whatever the outcome, resolution of the psi debate would productively shape the public's interpretation of their claims – as genuine psychic experiences, as cognitive illusions, or, in some cases, as psychopathology symptoms.

The Current Stalemate

Past research has proved insufficient for three reasons. First, negative evidence is ultimately uninformative. Because one cannot prove the null hypothesis, negative results necessarily carry with them a certain degree of ambiguity. In any given experiment, the failure to obtain evidence for psi may reflect many things other than its nonexistence. For example, the researcher may have measured the wrong variable, or measured the right variable at the wrong time or with the wrong participants. Despite an abundance of null results, we cannot with any certainty conclude that psi does not exist.

The second reason why parapsychology has reached a stalemate is that the accumulated positive evidence, like the accumulated negative evidence, is inherently ambiguous. Even when an anomaly has been unambiguously uncovered, its nature remains undetermined. Interpretation is left to presumption: some presume a yet uncovered normal explanation (e.g. undetermined methodological flaw), whereas others presume a yet uncovered paranormal explanation (e.g., extrasensory perception). Because the unexplained is not necessarily the inexplicable, psi researchers must not simply accumulate evidence for an anomaly, but they must also conduct experiments that offer insight into the nature of that anomaly. The remote-viewing debate illustrates this point most clearly. After twenty years of research, even skeptics accepted the presence of an anomalous effect; they did not, however, accept this as evidence of psi. Consider the following quotes from skeptic Ray Hyman (1995, p. 3-56):

> The case for psychic functioning seems better than it ever has been. The contemporary findings along with the output of the [government's] program do seem to indicate that something beyond odd statistical hiccups is taking place. I also have to admit that I do not have a ready explanation for these observed effects. Inexplicable statistical departures from chance, however, are a far cry from compelling evidence for anomalous cognition.

Thus, evidence for psi that fails to provide concurrent insight into its underlying process cannot resolve the psi debate. Just as negative evidence has not altered the beliefs of those who presume psi does exist, positive evidence has not altered the beliefs of those who presume psi does not exist.

Finally, parapsychology has reached an impasse because there is no single experiment with replicable results. Although the remote-viewing and psi-Ganzfeld databases arguably contain statistically replicable effects, no parapsychological paradigm has achieved replicability on an experiment-by-experiment basis. Parapsychologists often deny this problem of replicability by claiming that a) psi has a very small effect size and b) single experiments do not have the statistical power reliably to uncover an effect this small. But this explanation is unsatisfactory for two reasons. First, it ignores the experiments that show large effects. If the psi effect is so small that the best we can hope for is statistical replicability, then this subset of experiments with large effects should not exist. Indeed, one feature that defines the ganzfeld databases is their statistical heterogeneity: there is more variability in effect sizes across studies than one would expect by chance (Storm, 2000). Second, parapsychologists have traditionally used measures that are insensitive to unconscious knowledge and, in doing so, have limited themselves to small effect sizes. In the ganzfeld paradigm, for example, participants must explicitly identify the psi stimulus from a set of decoys. If a participant's knowledge of the psi stimulus is unconscious, then the forced-choice measure is an insensitive way to quantify a psi effect. Until more sensitive measures of unconscious knowledge have been thoroughly investigated, we think it is premature to conclude that psi effects cannot be replicated on an experiment-by-experiment basis.

Revising the Psi Hypothesis

These three problems of parapsychology can be traced to the actual hypotheses tested by past research. Past research has tested the following multi-part hypothesis:

1) psi exists
2) AND knowledge underlies psi

3) AND this knowledge has behavioral correlates
4) AND we know what to measure

The first claim is the raw psi hypothesis, and the second claim asserts that a mental mechanism underlies psi phenomena (in contrast to, for example, synchronicity; Jung, 1952). Both of these claims are uncontroversial in this context; both are necessary components of any parapsychological experiment. The third claim – that psi is manifested behaviorally – is justifiably sound: psychologists have extensively documented behavioral correlates for even the weakest forms of knowledge (for review, see Merikle & Daneman, 1998), although only recently and not without considerable difficulty. The final claim, however, is quite brazen. Had past experiments uncovered a reliable effect, one should applaud such boldness. Yet in the current situation, the claim to know what critical variables to select from the boundless set of possibilities appears more presumptuous than bold. Moreover, selected variables often demand the explicit identification of psi stimuli, making a further assumption about the nature of psi knowledge. Whether they know it or not, psi researchers make great assumptions about the source of psi stimuli, the nature of psi knowledge, and the nature of psi information processing when they select a behavior to measure. In our view, these assumptions may well be responsible for the stagnated state of parapsychology today.

We propose that researchers minimize the assumptions implicit in their methodology by testing the following hypothesis:

1) psi exists
2) AND knowledge underlies psi

Aside from the assumption that psi involves cognition (which is arguably part of its definition) we make no claims about the source of knowledge, the kind of processing, or the nature of mental content responsible for psi. To do this, we go to the source of knowledge and cognition: the brain. The brain mediates all behavior or, put differently, no behavior exists that does not stem from brain activity. By measuring such activity instead of overt behavior, we can test the psi hypothesis in a novel, powerful way. In fact, paring down the assumption embedded in the normal psi hypothesis offers us potential escape from the three insufficiencies of past research.

Operationalized, the psi hypothesis on the level of the brain is very simple: the brain responds differentially to psi stimuli versus non-psi stimuli. By psi-stimuli, we mean stimuli that have telepathic, clairvoyant, and precognitive sources; conversely, by non-psi stimuli, we mean stimuli that have no such sources. The two classes of stimuli are identical in all other respects, and thus the only explanation for a differential response is the existence of psi. Again, the power in this hypothesis is its relative lack of assumptions.

Revisiting the Stalemate

This new paradigm circumvents the three problems that plague parapsychology. With this method, one cannot obtain evidence for an anomaly without simultaneously obtaining evidence about its nature. If the brain responds selectively to psi stimuli, the direction and neuroanatomical location of this response will inform its interpretation. Even if such information does not completely disambiguate normal explanations (i.e., undiscovered methodological flaws) from paranormal ones (i.e., extrasensory perception), it will progress the debate beyond mere presumption and counter-presumption. No longer an amorphous, uninterpretable departure from chance, the anomaly of psi will have a size, shape, and location in the brain.

The current approach will also decrease the ambiguity of negative evidence. Although one can never prove the null hypothesis, not all null results are equal; some negative evidence is considerably more informative than others. For example, consider the possibility of water on Mars. If a set of close-up images of its surface failed to capture frozen lakes, few would accept the nonexistence of Martian water. Yet if a planet-wide analysis of its subsurface soil content failed to show telltale signs of water, most would accept the null hypothesis of a Martian desert. Past null results from parapsychology are comparable to scattered snapshots of the surface in that they measure a small sample of outwardly observable variables. The current neuroimaging approach, however, seeks anomalous knowledge at its source, inside the brain. In doing so, it advances the psi debate even with a null result.

Finally, the current approach improves the odds of achieving replication on an experimental rather than statistical level. The key here is the power of this approach, in a methodological rather than statistical sense. For the same reasons that this design decreases the ambiguity of a null result, it increases the power to detect a real effect. By making so few assumptions about psi, we can detect telepathy, clairvoyance, or precognition that is perceptual or conceptual, conscious or unconscious. A design unfettered by such assumptions is surely more powerful than one committed to a narrow, presumptuous model an unknown phenomenon.

fMRI

Our experiment capitalizes on the flagship technology of modem psychology: functional magnetic resonance imaging (fMRI). Despite its relatively recent entrance into psychological experimentation, fMRI has revolutionized both the questions that can be asked by psychologists and the answers that can be found. fMRI is a noninvasive method to track hemodynamic changes caused by localized neural activity changes. The most common form of fMRI is the Blood Oxygen Level Dependent (BOLD) technique (e.g., Kwong et al., 1992). A local increase in neural activity results in an increase in regional cerebral blood flow, which in turn causes a decrease in the concentration of deoxyhemoglobin. The lower concentration of deoxyhemoglobin decreases the rate of depolarization of hydrogen nuclei, and it is this effect that is used to create the MRI image. Hence, fMRI signal intensity is correlated with spatially localized changes in neural activity. The fMRI signal changes are usually small (often less than 2% for a 1.5 Tesla magnet scanner) and care must be taken to differentiate them from potential artifacts.

Whereas earlier fMRI experimentation was constrained to block designs (where many trials of the same type are presented in immediate succession), advances in technology and statistical analyses have allowed researchers to design more flexible event-related fMRI (ERfMRI) paradigms. In event-related designs, different types of sequential trials can be intermixed, allowing researchers to document hemodynamic responses to aperiodic or unexpected stimuli, as well as to conduct analyses based upon participants' performance on a trial-by-trial basis. In classic event-related fMRI experiments, both Wagner et al. (1998) and Brewer et al. (1998) demonstrated that neural activation recorded while participants viewed individual words could be used to predict subsequent performance on those words during a recognition task. Event-related fMRI paradigms have been employed successfully to study many cognitive domains, from mental imagery (Klein et al., 2000) to attention (Clark et al., 2001). Because the current experiment requires the intermixing of psi and non-psi stimuli, it would not be possible without the development of ERfMRI.

Neuroimaging of Knowledge

One of the most widely studied phenomena in neuroimaging is repetition suppression (for review, see Grill-Spector, Henson, & Martin, 2006), also known as adaptation (Sobotka & Ringo, 1994) or neural priming (Maccotta & Buckner, 2004). Just as prior stimulus knowledge biases judgments or response times in behavioral priming, prior stimulus knowledge biases brain activation in repetition suppression. For example, in one of the first studies of repetition suppression, Desimone (1996) showed macaque monkeys a series of novel pictures and measured the activity of single neurons in an area of the brain critical for object recognition and memory. When the monkey saw stimuli that were repeated, activity in approximately half of the measured neurons was significantly reduced. This suppression effect has been shown in all measures of neuronal activity, from single-cell recording to neuroimaging, and across a wide array of tasks. Brain activation is reduced even when the source of the prior stimulus knowledge is subliminal (Naccache & Dehaene, 2001), days earlier (van Turennout, 2000), a different size, position, orientation (Grill-Spector et al, 1999), or conceptual (Wheatley, Weisberg, Beauchamp, & Martin, 2005). Repetition suppression is an extremely robust, well-established phenomenon that reflects the brain's differential response to novel versus repeated stimuli.

If psi exists, the brain should respond differently, as measured by fMRI, to psi stimuli than non-psi stumuli. Based upon the repetition suppression literature, we expect that foreknowledge of psi stimuli will suppress neuronal activity associated with those stimuli. But to maintain our goal of minimizing assumptions, and because of evidence that attention enhances brain activity (Corbetta, Miezin, Shulman, & Petersen, 1995), we will not make a hypothesis concerning the direction of the differential activation.

The Current Experiment

We adapted our design from a series of event-related potential (ERP) experiments that first investigated the possibility of differential psychophysiological responses to psi versus non-psi stimuli (McDonough, Don, & Warren, 2002; Don, McDonough, & Warren, 1998; Warren, McDonough, & Don, 1992a, 1992b). Our proposal relies on similar logic as that used in this group of experiments, while at the same time takes advantage of a dependent measure (BOLD hemodynamic response) that is substantially more informative than ERPs. In addition, we altered several key aspects of the methodology in an attempt to better document the hypothesized anomalous processes.

In the current experiment, the brains of sixteen participants were scanned while engaged in a simple guessing task. Each participant brought with them an acquaintance, friend, significant other, or relative to act as the telepathic "sender". During the guessing task, participants saw pairs of photographs and tried to identify the randomly determined "target" image from a "decoy" image. We collected functional data during five sets of trials. The target image in each pair was associated with three possible psi sources, corresponding to the three hypothesized mechanisms of psi; the decoy image was not associated with any psi sources. The sender, the telepathic source, viewed target images in a remote room. A computer monitor, the clairvoyant source, displayed target images in a remote room. And as a precognitive source of information, target images were presented again to receivers in the future. To test the psi hypothesis, we compared brain activation for target images with brain activation for decoy images on a series (set of trials), participant, and group level.

We believe that the current design offers the broadest test possible of the psi hypothesis, for it tests the entire range of hypothesized mechanisms and, in essence, the entire range of hypothesized dependent variables. Although situational variables may still moderate the presence or degree of observed psi effects, our study has the novel potential to identify anomalous cognition in its various forms.

Methods

Design

Although blocked designs with a 21-40 second periodicity produce optimal power for uncovering differential responses, such a design is not possible in this context due to the need to present unpredictable target sequences. Rapid event-related designs – which involve the deconvolution of overlapping hemodynamic responses (HRs) evoked by different event types – offer the next most powerful fMRI design, over slow event-related protocols with non-overlapping HRs (Wager & Nichols, 2003).

Participants

We recruited nineteen pairs of individuals from the local community to participate in this experiment. These pairs were comprised of identical twins (four pairs), non-twin biological relatives (two pairs), romantic partners (seven pairs) emotionally close friends (five pairs), and acquaintances (one pair). Based upon the participants' preference, right-handedness, as well as MR safety guidelines, we selected one member from each pair (the "receiver") for fMRI scanning. The remaining member (the "sender") was not scanned, but participated by passively viewing visual stimuli from outside the scanning room. Data from three pairs (one twin pair, one friend pair, and one couple) were eliminated due to scanner spiking, equipment malfunction, or excessive head motion. Of the remaining participants, 14 were males (mean age: 22.8, range: 19-47) and 18 female (mean age: 23.4, range: 19-58). Participation lasted approximately 2.5 hours, with 1.5 hours devoted to MR scanning and the rest reserved for paperwork, instructions, training, and debriefing. In addition to travel expenses, we compensated receivers and senders $120 and $50, respectively, for their participation.

Materials

Stimuli. We chose emotional stimuli for this experiment due to parapsychological literature suggesting that such stimuli are psi-conducive (Cornell, 1999; Myers, 1903; Rao, 1986; Moss & Gengerelli, 1967, 1970; Sherwood et al., 2000). Using Lang, Bradley, and Cuthbert's (1995) International Affective Picture System (IAPS) stimulus set, we selected 240 pairs of photographs as test stimuli based upon content uniqueness and normative emotionality ratings. Negative, highly arousing pictures (e.g., a snake) were paired with either neutral or positive images of low arousal (e.g., a tissue box, giraffe); positive, highly arousing pictures (e.g., an erotic couple) were paired with either neutral or negative images of low arousal (e.g., a neutral face, cemetery). By maximally separating pictures within each stimulus pair on dimensions of valence and arousal, we sought to increase the likelihood of a psi effect. Figure 1 presents four example stimulus pairs.

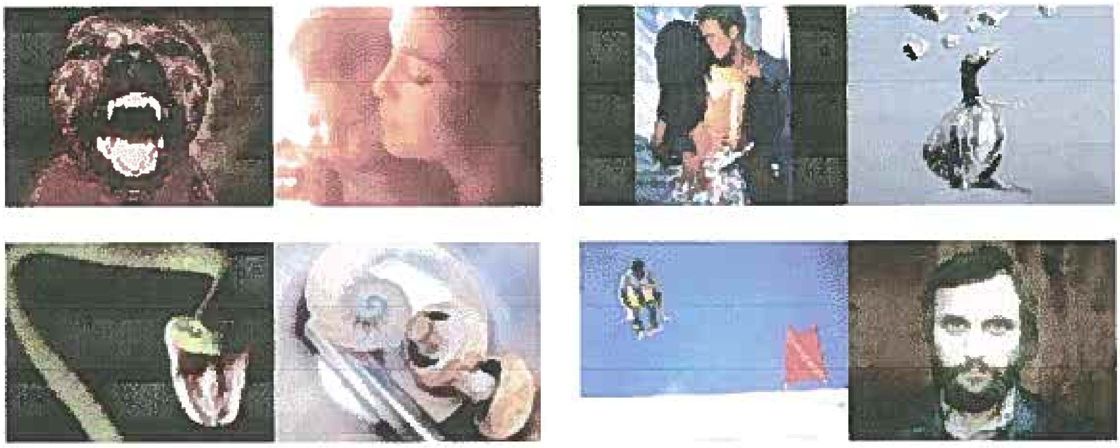


FIGURE 1: *Example stimuli for the four types of pairing based on normative valence and arousal ratings. Clockwise, from top left: negative, high arousal + positive, low arousal; positive, high arousal + negative, low arousal; negative, high arousal + neutral, low arousal; positive, high arousal + neutral, low arousal.*

Stimulus lists. For each picture pair, we first randomly assigned Stimulus Category (decoy, target). Next, we randomly assigned half the target stimuli to each Position (first, second) to ensure that target assignment was not confounded with stimulus presentation. We ensured the adequacy of this randomization by testing for differences in arousal and valence across Stimulus Category x Position conditions (paired t-tests, all $p > .1$, two-tailed). We then created four stimulus lists to counterbalance completely each pair on these variables. Therefore, across lists, any given picture was assigned once to each of the following conditions: decoy-first, decoy-second, target-first, target-second. We also randomly assigned pairs to trial positions within each list, and created an additional counterbalancing variable of Trial Order (forwards, backwards). We crossed Trial Order with Stimulus Category and Position to create eight total between-participants stimulus lists.

Apparatus

All stimuli presented during MR scanning were generated using a Macintosh G4 computer using PsyScope software (MacWhinney, Cohen, & Provost, 1997). Visual stimuli were backprojected via an LCD projector and a mirror attached to the head coil. We collected press responses by using an MR-compatible button box connected to the Macintosh via a custom USB interface.

Tasks and Procedure

Receiver. After providing informed consent and still outside the scanner, participants completed a practice version of the experimental task for approximately 10 minutes to familiarize themselves with the stimuli, procedure, and minimum response window (2s). None of the experimental stimuli were used in the practice task.

Once positioned in the scanner, we first obtained anatomical scans from the receiver participants; this required approximately 25 minutes, during which time they passively viewed a dynamic visual display designed to entertain and relax them. Following this, we administered 5 functional scans while the participants completed 5 sets of the experimental task. Each set consisted of 48 trials, and each trial contained three sequential exposures to two unique IAPS stimuli. The first and second exposures, lasting 1 second each, presented participants with the assigned decoy and target from each stimulus pair (with the presentation order varying across trials), and were separated by a randomly jittered period of fixation (2, 4, or 6s). From the participants' perspective, of course, each of these first two stimuli was a potential target. After the second exposure, participants saw "press key now" on the screen and indicated their target selection by pressing one of two buttons (button "1" for the first item, button "2" for the second item). Participants had between 2-6s to respond depending upon the trial, and their responses did not advance the script. Following this forced-choice response, participants were presented with the target stimulus (1s duration) and feedback ("correct! + $1" or "wrong!", duration 2-6s). Figure 2 offers a visual representation of the receiver's task.

The physical construction of the fMRI scanner precludes visual contact between the participant and experimenter; auditory leakage is controlled for by the loud noise of the scanner as well as the use of earplugs.

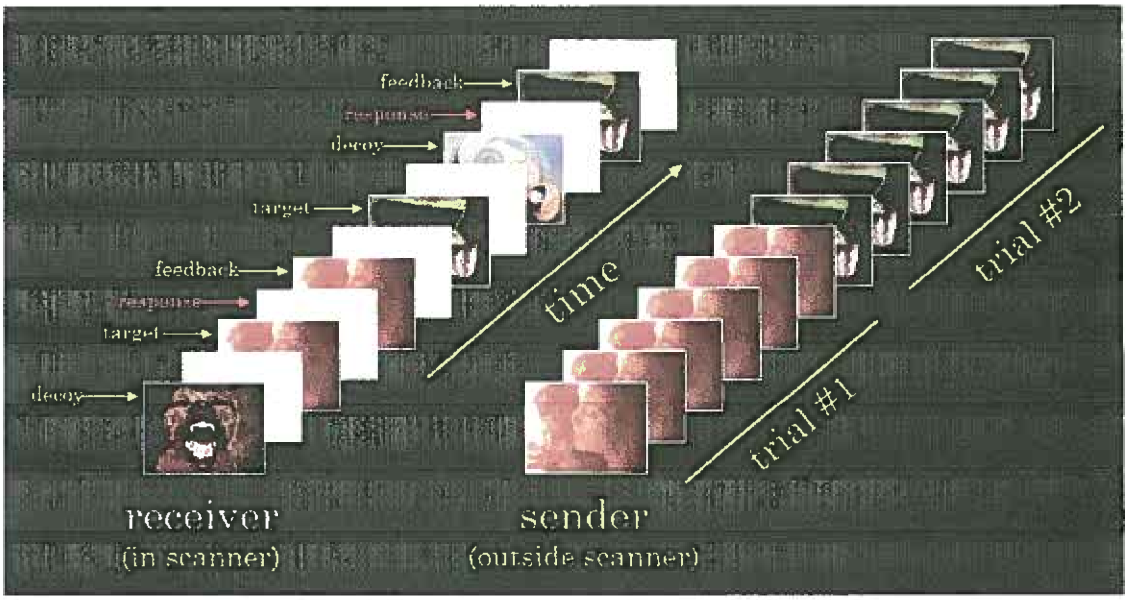


FIGURE 2: *A schematic of two trials. In the first trial for the receiver, the decoy stimulus appears first; in the second trial, the target stimulus first. The third stimulus presentation in each trial is always a repeat of the target stimulus. All three presentations are separated by a variable delay of 2-6 seconds. The sender sees only the target stimuli for each trial.*

Sender. After providing informed consent, sender participants observed their partner practice the receiver's task, and were concurrently given instructions about the details of the their task. We then took the senders to another room, where they could not see or hear the scanner, and they waited for the experimenter's cue to begin their task. At the start of each functional scan, the experimenter signaled the sender to begin, at which point the sender started viewing the series of target images associated with that scan. The sender and receiver scripts were temporally synchronized such that during each entire trial of the receiver's task, the sender viewed that trial's target stimulus. Senders viewed each stimulus for between 10-22s, depending upon timing of the receivers' stimulus presentations. We asked senders to "influence the receiver" with the target picture, adopt a "playful" attitude, maintain an active interest in the stimuli, and use whatever "sending" tactics they deemed appropriate. We did not provide senders with real-time, trial-by-trial feedback; both receivers and senders were informed of the overall hit rate at the conclusion of the experiment. Figure 2 offers a visual representation of the sender's task.

fMRI Acquisition

MR scanning was performed using a 3T Siemens Allegra head-only MR scanner with high-speed imaging gradients and a quadrature head coil. Structural images included a high-resolution MP-RAGE scan (128 sagittal slices, 1.33 mm thickness, T_R = 2530 ms, T_E = 3.25ms). Whole-brain functional imaging was performed using a gradient-echo EPI pulse sequence (33 interleaved slices oriented along the AC-PC line, 3mm thick with 1mm interslice gap, T_R=2s, T_E = 30ms, flip angle = 90°). Each functional series lasted 13min 8s and consisted of 394 sequential whole-brain acquisitions. Four additional volumes were acquired at the start of each functional series to account for T1 saturation effects.

Imaging Analysis

We performed a series of preprocessing steps to prepare the imaging data for statistical analyses. First, brain and non-brain image data were segmented using FSL's Brain Extraction Tool (BET; Smith, 2002). Each extraction was manually inspected and conducted again with new parameters (center of gravity, fractional intensity threshold) if necessary to ensure maximal segmentation. We applied slice timing correction to account for the sampling offsets inherent in slice-wise EPI acquisition sequences, and rigid-body transformation of 3D functional volumes to correct for spatial misalignment due to head motion. Mean changes in MR signal between functional series were eliminated with 4D global intensity normalization. Data were convolved in three-dimensional space with a 5mm (FWHM) Gaussian kernel to increase signal-to-noise ratio and allow later GRF-based statistical thresholding. To inform decisions about temporal filtering, we conducted a Fourier analysis of our unfiltered design matrix. This revealed significant spectral power in high-frequency domains and little power in frequencies slower than 25 Hz. Based upon this spectral analysis, we decided against low pass filtering and set a high pass filter cutoff of 25s to eliminate low-frequency noise (e.g., scanner drift). Using the tool FLIRT (Jenkinson, Bannister, Brady, & Smith, 2002; Jenkinson & Smith, 2001), functional data were registered to the brain-extracted structural images using a 6DOF rigid-body transformation and to standard MNI space using a 12DOF affine transformation. We visually inspected the accuracy of each registration, and also manually checked each brain volume for evidence of scanner spiking or other image anomalies.

We analyzed 4D functional series separately using FMRIB's Improved Linear Model (FILM; Woolrich, Ripley, Brady, & Smith, 2001), which removes nonparametrically-estimated temporal autocorrelation in each voxel's time series before applying the general linear model (GLM). Three variables – decoy, target, feedback – were linearly modeled by convolving their basic waveforms with a double-gamma canonical hemodynamic response function. The temporal derivative of each variable – based upon a first order Taylor series expansion – was also entered into the model to allow for minor temporal shifts in stimulus presentations or hemodynamic responses across the brain. The same temporal filtering that was applied to the data was also applied to the model. For every functional volume, linear contrasts were employed to create statistical parametric maps (SPMs) for the effects of interest.

Data were analyzed at the level of individual series, individual participants, and the group. All analyses were fixed-effects rather than mixed-effects, meaning that only within-series variance was modeled (and not between-series or between participants variance). We chose this analysis because we have no interest in drawing inferences beyond our tested population; rather, we are exclusively interested in evaluating the evidence for psi in our dataset.

At the group level, we tested two contrasts: *decoy > target* and *target > decoy*. Using parametric statistics based on Gaussian random-feld theory (Friston, Worsley, Frakowiak, Mazziotta, & Evans, 1994; Forman et al., 1995; Worsley, Evans, Marrett, &

Neelin, 1992), the corresponding SPMs were thresholded on the voxel level at $z = 3.1$ (uncorrected $p = .001$), and on the cluster level at $p = .025$ (the alpha level was halved to correct for the two independent contrasts of interest). Although researchers commonly apply a voxel z-cutoff of 2.3 in the analysis of group data, this choice is ultimately arbitrary without any prior hypothesis about the size of expected activation. To ensure that the results from the primary analysis did not critically depend on an arbitrary chosen thresholding parameter, we completed a set of supplemental analyses. Specifically, we tested whether the number, location, or significance of clusters qualitatively changed as a function of the voxel height threshold (tested range: $2.3 < z < 3.2$).

At the participant level, we tested the same two contrasts described above but with two appropriate revisions. First, due to a decrease in the amount of data we changed the primary voxel z-threshold to 2.3 and the exploratory range to $1.9 < z < 2.7$. Second, because we now had 32 independent tests (16 participants x 2 contrasts per participant), we lowered the alpha value to 0.0016.

We analyzed individual series in the same way as we analyzed participant data, but changed the alpha value to 3.25×10^{-4} to correct for the 154 independent tests (77 series x 2 contrasts per series). In addition, we applied additional corrections for motion artifacts due to the decreased amount of data contained in individual series (compared to the amount of data contained in the participant or group analysis). We reduced the six motion parameters (3 translations, 3 rotational) into two factors through principal components analysis, and added these two factors as covariates in the GLM analysis of the functional data. We disregarded any activation whose anatomical location or shape matched well-established patterns of spurious, motion-related activation (e.g., curvilinear activation near tissue/non-tissue intersections). For series with significant clusters that did not appear motion-related, we also applied a cluster-based nonparametric thresholding technique (FSL's *randomise* tool, 10000 iterations, 5mm FWHM smoothing, z voxel-threshold = 2.3).

Results

Behavioral Results

Hit rates. Participants failed to respond within the time window on 76 trials, and equipment malfunction resulted in the loss of data from 77 trials. The remaining data consisted of 3687 trials from 79 series, with the target occurring in the first stimulus position 1842 times (50.0%). Participants selected the second stimulus more frequently than this first (53.1% versus 46.9%; two-tailed $p = 7.9 \times 10^{-5}$). Overall, participants selected the decoy 1842 times and the target 1845 times. This overall success rate (50.0%) did not significantly deviate from chance expectation (50.0%). Because participants guessed the second stimuli more frequently than the first, their hit rate was higher when the target was the second stimulus (53.1%) than when it was the first (46.9%). We also analyzed results on a participant level to determine whether, despite the overall chance results, any individual participants exhibited nonrandom guessing behavior. No participant's mean performance deviated from chance (Bonferroni-corrected $\alpha = .0031$). Furthermore, the distribution of participant's scores did not significantly differ from chance distributions in standard deviation, kurtosis, or skew

(Monte Carlo analyses, p's > .1). Finally we also checked for nonrandomness in each individual series. No series had a hit rate that deviated from chance (Bonferroni-corrected $\alpha = 6.33 \times 10^{-4}$). And although the distribution of scores fit the expected distribution on indices of skew and kurtosis (Monte Carlo analyses, p's > .1), the standard deviation of the obtained data was abnormally high (Monte Carlo analysis, simulation mean σ = 0.073, data σ = 0.093, two-tailed p = 5.86×10^{-4}). Attempts to explain hit rates or variability in series hit rates by participant variables (e.g. age, sender/receiver relationship), situational variables (e.g. date), or task variables (e.g. series number, list properties) were unsuccessful.

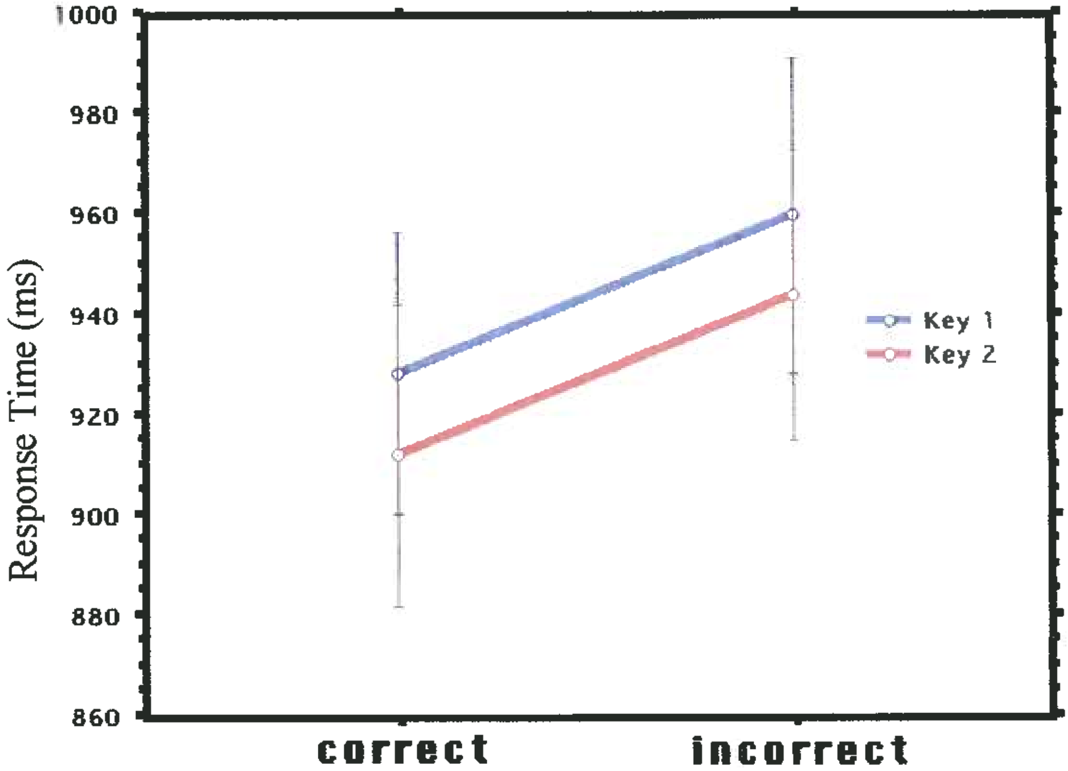


FIGURE 3: *Series RT means for correct and incorrect, plotted separately for each response key.*

Response times. We also tested whether participants' responded faster (or slower) when they correctly selected the target compared to when they incorrectly selected the decoy. We conducted a 2 [Response Category: correct, incorrect] x 2 [Response Key: key 1, key 2] Analysis of Variance (ANOVA) on participants' overall mean RTs and, separately, on their mean series RTs. We defined outliers as responses that were more than 2.5 times greater than the remainder of that series' mean RT. With participants as the random effect, there was no significant main effect of Response Key [key 1 mean = 938ms, key 2 mean = 936ms, $F(1,15) = .006$, $p = .94$] or interaction effect [$F(1,15) = .32$, $p = .58$]; there was, however, a marginally significant main effect of Response Category [decoy mean = 948ms, target mean = 927ms, $F(1,15) = 4.2$, $p = .058$]. With series as the random effect, there was no significant main effect of Response Key [key 1 mean =

935ms, key 2 mean = 937ms, $F(1,78) = .015$, $p = .90$] or interaction effect [$F(1,78) = .36$, $p = .55$]; there was, however, a significant main effect of Response Category [decoy mean = 952ms, target mean = 920ms, $F(1,78) = 5.5$, $p = .022$]. Our exclusion of outliers and inclusion of the factor Response Key did not qualitatively change any of the results. Figure 3 presents the results from the analysis of series mean RTs.

fMRI Results

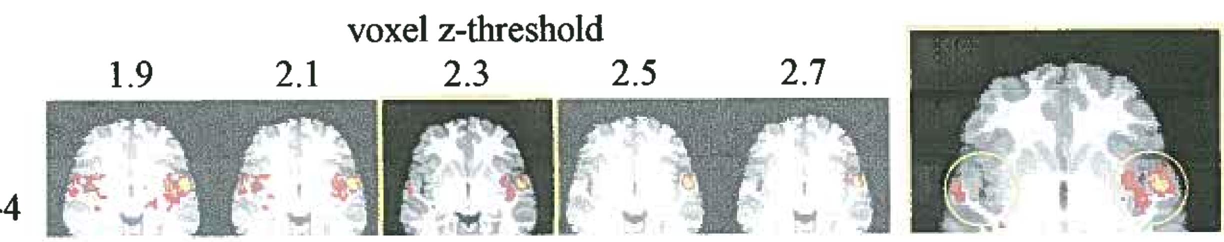
Group. On a group level, our analysis revealed no significant clusters for either psi contrast (decoy > target, target > decoy).

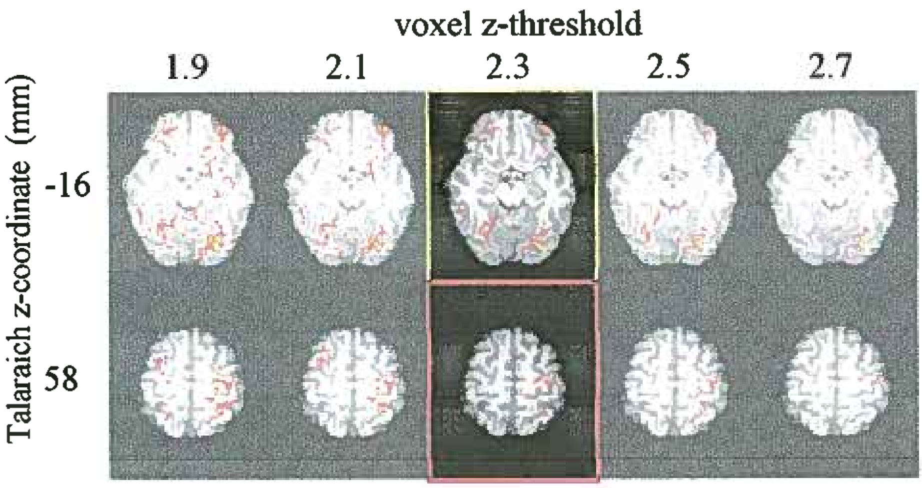
Individual participants. On an individual level, our analysis revealed no significant clusters (after correction) for either psi contrast in fifteen participants. In one participant (#14), however, the decoy > target contrast revealed multiple clusters of significant activation, as seen in Figure 4. The first series from this participant was excluded from analysis due to excessive head motion, leaving only series 2-5 subject to analysis. The clusters were centered bilaterally in the superior temporal gyrus (BA 21, 22, 41), with the largest, most significant, and most robust activity in the left hemisphere. Using our *a priori* voxel correction ($z > 2.3$, $p < .01$), activation in both hemispheres reached significance (after correcting for multiple analyses). In the left hemisphere, the cluster contained 592 voxels (corrected $p = 2.48 \times 10^{-5}$), and remained significantly large across all explored voxel thresholds (z: 1.9 – 2.7; p: .03 – .003). In the right hemisphere, the cluster contained 342 voxels (corrected $p = .0107$), and remained significantly large across most of the explored voxel thresholds (z: 1.9 – 2.3; p: .03 – .01). Table 1 presents cluster size, location, and significance value as a function of the voxel z-threshold.

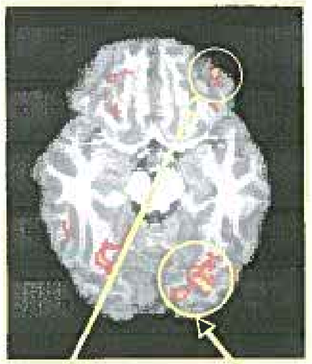
Individual series. On an individual series level, our analysis revealed significant clusters of activation in 3 out of 77 series. For one of these series (Participant 16, third series), the pattern of activation was unambiguously motion-related: activity was limited exclusively to tissue borders. The remaining two series were from the same participant (#11), consecutive (series 3 and 4), and showed multiple clusters of significant activation for the decoy > target contrast.

In Participant 11's third series, the most robust activity occurred in the left inferior occipital gyrus (BA 19), left middle frontal gyrus (BA 11), and left postcentral gyrus (BA 3), as seen in Figure 5. Activity in all three regions was significant after correction for multiple analyses (2 contrasts per series x 77 series = 144 possible contrasts), significant at primary ($z > 2.3$) and exploratory voxel correction thresholds ($z > 1.9 – 2.7$), and significant using both parametric and nonparametric thresholding calculations (Monte Carlo permutation test using FSL's *randomise* tool). Table 2 presents cluster statistics for these three locations.

In Participant 11's fourth series, the most robust activity occurred bilaterally in the inferior frontal gyrus (BA 47) and in left occipitotemporal regions (BA 39, 19), as seen in Figure 6. Again, activity in these areas reaches significance after Bonferroni correction using both primary and exploratory voxel threshold, and both parametric and nonparametric tests. Table 3 presents cluster statistics for these regions.







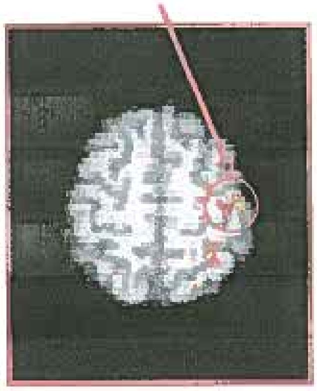
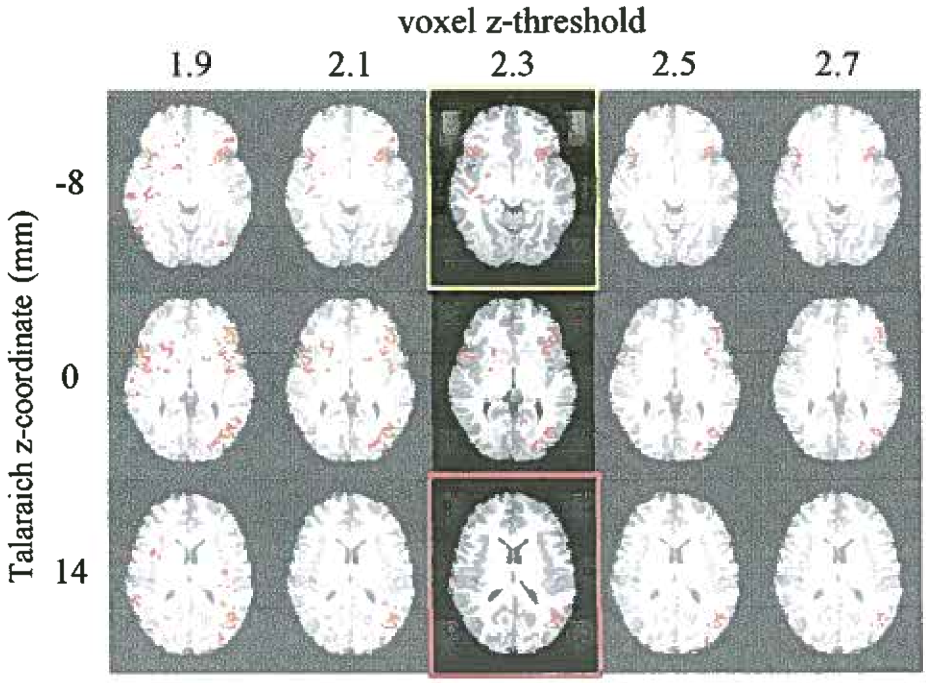
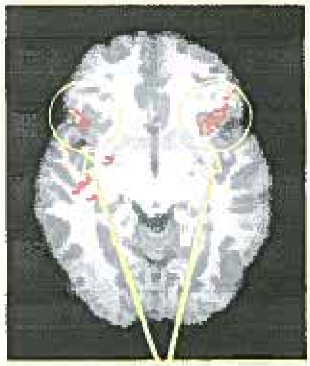


FIGURE 5: *Significant activation for decoy > target contrast in Participant 11Õ third run as a function of voxel z-threshold. Clusters were centered in the left middle frontal gyrus (BA 11), left inferior occipital gyrus (BA 19), and left postcentral gyrus (BA 3). Functional activation is overlaid on the participantÕ T1 structural image.*

	Left Middle Frontal Gyrus		Left Inferior Occipital Gyrus		Left Postcentral Gyrus	
voxel z-threshold	# voxels in cluster	corrected p-value	# voxels in cluster	corrected p-value	# voxels in cluster	corrected p-value
1.9	185	8.28×10^{-5}	2593	~0	2593	~0
2.1	257	6.40×10^{-10}	821	2.40×10^{-26}	434	8.57×10^{-16}
2.3	**79**	$\mathbf{8.90 \times 10^{-4}}$	**235**	$\mathbf{3.98 \times 10^{-12}}$	**241**	$\mathbf{2.11 \times 10^{-12}}$
2.5	41	.0403	94	1.41×10^{-6}	102	8.57×10^{-8}
2.7	–	n.s.	42	.00191	43	.00147

TABLE 2: *Left middle frontal gyrus, inferior occipital gyrus, and postcentral gyrus cluster statistics for Subject 11Õ third run as a function of voxel z-threshold. All p-values are corrected for multiple comparisons across the brain as well as multiple analyses (2 contrasts x 77 runs) across the experiment. These clusters also survived a nonparametric analysis (Monte-Carlo simulation).*





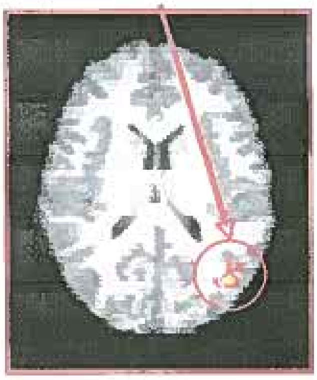


FIGURE 6: *Significant activation for decoy > target contrast in Participant 11Õ fourth run as a function of voxel z-threshold. Clusters were centered in left and right inferior frontal gyrus (BA 47), and left middle temporal/occipital grus (BA 39/19). Functional activation is overlaid on the participanÕ T1 structural image.*

	Left Inferior Frontal Gyrus		Right Inferior Frontal Gyrus		Left Middle Temporal/Occipital Gyrus	
voxel z-threshold	# voxels in cluster	corrected p-value	# voxels in cluster	corrected p-value	# voxels in cluster	corrected p-value
1.9	380	3.05×10^{-10}	632	4.57×10^{-16}	327	7.22×10^{-9}
2.1	185	4.34×10^{-7}	217	2.40×10^{-8}	187	3.62×10^{-7}
2.3	**94**	$\mathbf{1.10 \times 10^{-4}}$	**65**	**.00871**	**101**	$\mathbf{3.67 \times 10^{-5}}$
2.5	65	3.03×10^{-4}	50	.00621	75	4.59×10^{-5}
2.7	43	.00162	35	.0139	56	6.42×10^{-5}

TABLE 3: *Left middle frontal gyrus, inferior occipital gyrus, and postcentral gyrus cluster statistics for Participant 11Õ fourth run as a function of voxel z-threshold. All p-values are corrected for multiple comparisons across the brain as well as multiple analyses (2 contrasts x 77 runs) across the experiment. These clusters also survived a nonparametric analysis (Monte-Carlo simulation).*

Head motion. Although we accounted for head motion artifacts by a) eliminating series with excessive head motion, b) spatially aligning all three dimensional functional images in the preprocessing stage, c) adding motion covariates to our model, d) scrutinizing the anatomy of any significant activation, and e) using nonparametric tests that do not make assumptions about the distribution of noise, stimuli-correlated motion might still produce specious activation. Because motion induces not just spatial misalignment but complex, unmeasured changes in the MR signal (e.g., interpolation and magnetic susceptibility effects), the motion "correction" methods listed above do not, in fact, completely eliminate the effect of head motion. We can, however, correlate motion parameters with the design matrix to evaluate directly the possibility that stimulus-correlated motion produced spurious activation. For each series or set of series of with significant decoy > target activity, we correlated measures of absolute and relative head motion with the convolved decoy and target variables (as well as a the difference between them). We found no significant correlations, and thus no evidence that stimulus-correlated motion explains our results.

"Gleam in the eye". We wanted to ensure that the observed differences between decoy and target BOLD hemodynamic responses did not reflect participants' future behavioral responses rather than their implicit knowledge of the target items. Because the relevant hit rates differed numerically from chance (47.9, 56.3, 39.6% hit rate for Participant 15, Participant 11's third series, and Participant 11's fourth series, respectively), the observed activity might reflect a "gleam in the eye" effect instead of a psi effect. To test for this possibility, we divided the first two stimulus presentations into selected and unselected stimuli (based upon whether or not the participant later selected those as target) instead of target and decoy stimuli, and tested the following two contrasts: selected > unselected, unselected > selected. These contrasts revealed either no significant clusters of activation, or clusters that did not overlap with the observed decoy > target clusters described above. Thus, the possibility of a "gleam in the eye" effect appears unlikely.

Content differences. A third possibility is that the observed differences stem from content differences between the target and decoy stimuli. On a group level, no such content differences exist because each stimulus is presented across participants as both decoy and target; on an individual participant and series level, however, the target and decoy stimuli are not identical. Although we randomly assigned stimuli to psi conditions and checked for mean differences in emotional arousal or valence between targets and decoys, we cannot rule out the possibility that participants responded differently to targets and decoys for normal reasons (i.e., differences in perceptual, conceptual, or affective information). To test the hypothesis that content differences explain the significant activation described above, we randomly permutated target assignment within each stimulus pair and re-analyzed the series that originally showed significant clusters of differential activation. In each simulation, the target and decoys reassignments were not significantly correlated ($r < .1$) with the real target and decoy assignments. We applied the same constraints to the permutations that were applied to the original stimulus lists: targets and decoys were balanced for onset times and stimulus position. Because of the

Bonferroni-corrected significance thresholds ($\alpha = 0.0016$ and 3.25×10^{-4} for participant and series results, respectively) and the computational demands of fMRI analyses, we cannot in practice conduct enough simulations to rule out the content differences hypothesis. With a limited number of simulations we could in theory, however, rule in such an explanation. We therefore conducted 75 simulations for Participant 11's third and fourth series, and 30 simulations for Participant 15's entire session (excluding the noisy first series).

The simulations of Participant 15's third series supported the content differences hypothesis. Out of 77 simulations, 2 revealed clusters in either the decoy > target or target > decoy contrasts that were equal to or larger than the largest cluster (241 voxels) found in the real series. The first significant simulation produced four clusters of activation (using the same voxel and cluster thresholds as used in the real series), ranging in size from 59-281 voxels. The second simulation also produced four clusters, ranging in size from 55-550 voxels. Moreover, some of these clusters were in the same or nearby areas (left middle frontal gyrus, left precentral gyrus, left middle occipital gyrus) as those of the original clusters (left middle frontal gyrus, postcentral gyrus, left inferior occipital gyrus). For neither of these simulations was the target assignment significantly correlated with the actual target assignment. The probability that the results from the real series would pass significance ($p < 3.25 \times 10^{-4}$) after an exhaustive permutation analysis and that the present simulation results (with 75 permutations) were due to chance is: 75 x (3.25 x 10-4)^2 = 7.92×10^{-6}.

The simulations of Participant 15's fourth series also supported the content differences hypothesis. Out of 77 simulations, 22 revealed clusters in either the decoy > target or target > decoy contrasts that were equal to or larger than the largest cluster (101 voxels) found in the real series. These clusters were located in all of the same regions (inferior frontal gyrus, left middle temporal/occipital gyrus) as those of the original clusters. The probability of obtaining these simulation results by chance is impossibly small.

Finally, the simulations of Participant 11's set of series also revealed similar results as those obtained with the real target assignments. Out of 30 simulations, one revealed clusters that were equal to or larger than the largest cluster (592 voxels) found in the real data. Moreover, this activation also occurred in the superior temporal gyrus (though only in the left hemisphere). In addition, the simulation produced significant activity in the inferior parietal lobule, middle frontal gyrus, and precentral gyrus. The probability that the results from the real series would pass significance ($p < 0.0016$) after an exhaustive permutation analysis and that the present simulation results (with 30 permutations) were due to chance is: 30 x 0.0016 = .048.

Discussion

Although we did not hypothesize a behavioral effect, participants responded more quickly to psi stimuli than non-psi stimuli in the guessing task. This effect is consistent in direction and magnitude to visual priming effects found in psychology (e.g. Marshall & Walker, 1987), and accords with our conception of psi as implicit knowledge. The significance of this effect, however, is not high enough to convince that the response time bias reflects anomalous cognition (as opposed to chance deviation). Nevertheless, this result is curious and we encourage parapsychologists to collect more response time data using tasks sensitive to priming.

Our neuroimaging data offer strong evidence against the existence of psi. The most compelling result is the lack of a group psi effect. The psi hypothesis predicts that psi and non-psi stimuli are different yet in our experiment these stimuli evoked indistinguishable neural responses. One counter explanation for this result is that we lacked the power to uncover such an effect. Although this is possible, it is improbable. We scanned a relatively large number of participants (n=16) for fMRI studies (typical n=12), and used an analysis (fixed-effects) with considerably more power than the analysis (mixed-effects) typically employed. We had remarkable power to detect activation associated with target and decoy presentations (versus baseline) on a group level (using the same analysis used in the psi analyses). As seen in Figure 7, nearly all areas of the brain showed activation on a group level to both target and decoy stimuli. Despite this, no areas of the brain showed differential activation to targets versus decoys. Because of the methodological power acquired by measuring psi knowledge at its source (the brain), and the statistical power of a fixed-effect analysis (as observable in the target > baseline and decoy > baseline analyses), we dismiss power as a likely explanation for our lack of results on a group level.

On an individual participant and series level, the target and decoy stimuli differed in aspects other than their association with precognitive, telepathic, and clairvoyant psi sources. There were likely small but discernable low-level visual differences (e.g. visual complexity) between the targets and decoys, as well as conceptual (e.g. animate versus inanimate) and affective (e.g. emotional valence) disparities. We did not present each stimulus twice – once as a decoy and once as a target – within each series or participant for fear that doing so would obscure a psi effect with normal repetition suppression or recognition effects. Instead, we conducted a simulation analysis to assess the likelihood that content differences might explain any differential response to target versus decoy stimuli. These simulation analyses revealed that the observed differential activation – on an individual and series level – was not a specific product of target assignment. Although we have not conclusively established the source of these differential responses, we can state with confidence that they do not reflect psi.

Given the emotionally evocative nature of our stimuli, one likely possibility is that participants responded differently to targets and decoys because of differences in emotional content between the stimuli. In a recent event-related fMRI study using the same stimulus pool (IAPS pictures), Kensinger & Schacter (in press), isolated brain regions that respond selectively to affective valence and arousal. With one exception (the inferior occipital gyrus), all of the regions that responded selectively to decoys versus targets in our experiment also responded selectively to valence in Kensinger & Schacter's experiment. Even though we balanced the target and decoy stimuli on dimensions of arousal and valence, the normative IAPS ratings reflect only central tendencies, not intractable stimulus attributes. In all likelihood, certain participants – or certain participants during certain series – reacted differently to target and decoy stimuli because of affective content differences between the stimuli.

The anatomical instability of the significant participant and series results also suggest that they reflect stimulus differences and not psi. Although the neural loci of perceptual processes do vary across individual (Rademacher, Caviness, Steinmetz, &

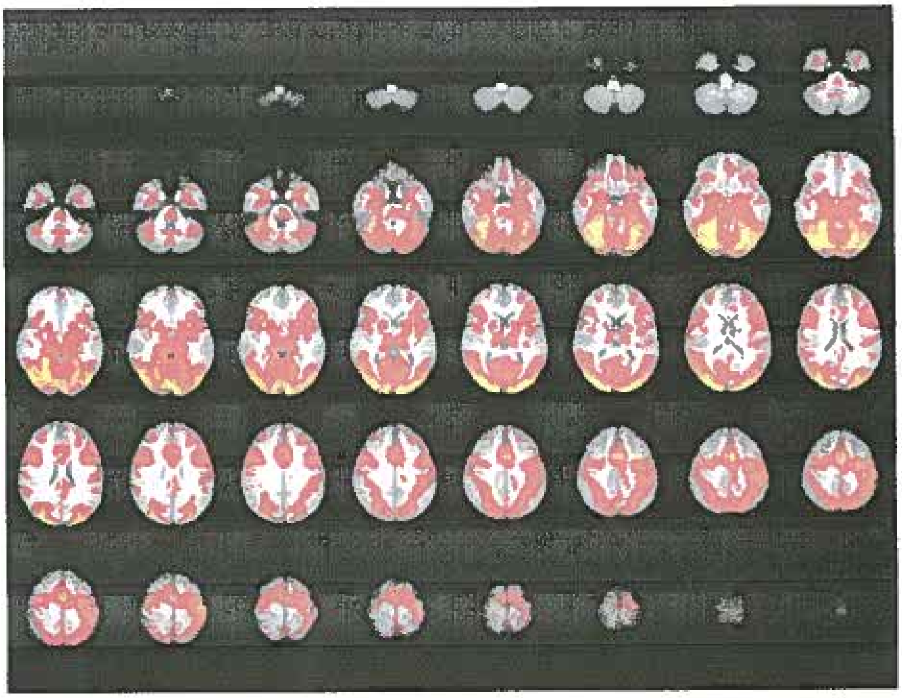
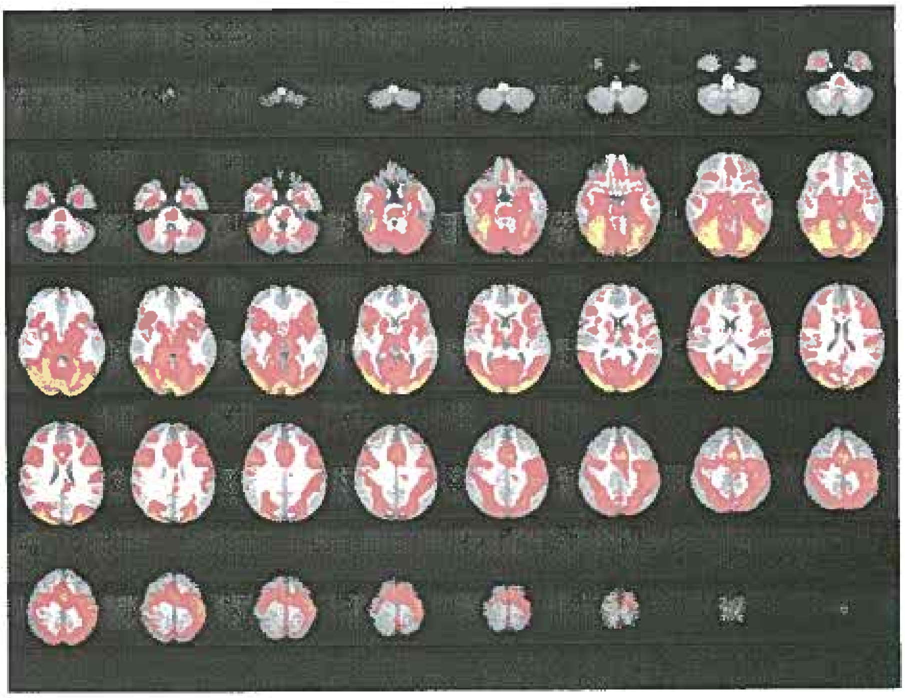


FIGURE 7: *Activation mapss for decoy >baseline (above) and target > baseline (below) contrasts on a group level using a fixed-effects analysis, voxel-thresholding at $z < 3.1$, and cluster thresholding at $p < .05$.*



Galaburda, 1993), the neuroanatomical instability in this experiment between differentially activated regions exceeds normal variation. Moreover, to explain our significant session results in terms of psi, one would have to argue that the neural correlates of psi shift substantially within an individual over a period of minutes. Within individuals, the test-retest reliability of fMRI has been well documented; in general, at least 60% of significantly-activated voxels reactivate upon retest, even when scanning periods are separated by a year (Kiehl & Liddle, 2003; Kurland et al., 2006; Machielsen, Rombouts, Barkhof, Scheltens, & Witter, 2000; Peeling & Downing, 2005). In this experiment, cluster overlap was practically nonexistent. Content differences are more likely to explain the shifting activation because each set of differentially activated clusters could reflect a heterogeneous set of content differences instead of one perceptual or cognitive process.

In contrast with psi research that employs behavioral methods, psi research that employs neuroimaging affords researcher great flexibility in data analysis. This analytic flexibility allows us to test alternative hypotheses in ways impossible with behavioral research. For example, in this study we were able to rule out a "gleam in the eye" alternative explanation for our results by matching the activity in question with the activity that underlay target selection. In many behavioral designs, one must conduct additional experiments to rule out alternative explanations; in neuroimaging, because the data are less theory-laden, one can re-analyze the same results to disambiguate interpretation.

Conclusion

Our goal in this study was to develop, implement, and test a new approach for testing the psi hypothesis using intellectual and technological advances from cognitive neuroscience. We believe that this approach – parapsychological neuroscience – can circumvent three problems that chronically plague parapsychology: the ambiguity of positive results, the ambiguity of negative results, the lack of experimental replicability. We are hesitant to draw any strong conclusions from our single experiment; the psi hypothesis should be confirmed or disconfirmed based upon a series of results. Nevertheless, we present strong evidence against the psi hypothesis. If these results replicate over a range of participants and situational contexts, we believe that one can safely conclude that psi, in fact, does not exist.

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