
Psi and Perception Without Awareness: A New Comparison

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To my Parents

Acknowledgements

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Declaration

I composed this thesis and the work is my own. The publication arising from this thesis is contained in the appendix. The publisher has granted permission for its inclusion.

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Abstract

Psi processing is commonly regarded as unconscious, operating outside the awareness of the individual. If this is the case, then it is possible to make a comparison between psi as a psychological process and other unconscious mental phenomena. The phenomena most often compared to psi is that of perception without awareness (PWA; see, e.g. Beloff, 1967; Roney-Dougal, 1986; Schmeidler, 1986).

Despite the apparent close association between psi and PWA, it has been over 15 years since any systematic research was conducted comparing the phenomena, and the field of PWA has changed considerably during this time.

The current thesis makes a new comparison between psi and PWA, taking into consideration the recent methodological advances in the latter. Experiments 1a and 1b used a word association paradigm developed by Stanford (1973). In this study, homophones were used, the interpretation of which may be influenced by unconscious primes. This paradigm was initially developed as a nonintentional psi test. Experiment 1a replicated the experiment using psi as a biasing stimulus. Although a significant main effect was not found, there was a trend in the hypothesised direction. Furthermore, there was evidence suggestive of an experimenter effect. Post hoc analysis revealed a “response bias” effect, whereby less common responses were associated with more psi “hits”. Experiment 1b was an attempt to influence interpretation of the homophones using an unconscious auditory stimulus. This study introduces a new technique for masking stimuli with white noise. Additionally, the “exclusion” task implemented was the first of its kind in a study of auditory PWA. Results of this study failed to reveal any unconscious influence on the homophone interpretation.

Experiments 2a and 2b focus on an effect first demonstrated in PWA research. Known as “false recognition”, the effect demonstrates that when a stimulus is viewed outside of awareness, it can influence recognition memory in such a way that conscious perception cannot. Experiment 2a was a replication of this effect, whereby participants viewing a biasing stimulus outside of awareness tended to “falsely recognise” this stimulus when it was presented in a recognition test. Experiment 2b was designed to investigate whether a similar effect on recognition memory could be obtained using psi as a biasing stimulus. Participants saw a large list of words before being given an “old/new” recognition test. On half of the “new” trials, a “sender” was attempting to psychically influence the participant to respond “old” (i.e. falsely recognise). No significant main effect was found, although non-significant gender interactions were observed.

Study 3 combined the most promising aspects of the previous studies in a repeated measures design. The results from experiment 1a were not replicated, while a replication of study 2a was successful. Various personality variables were also included. There was no correlation between performance on psi and PWA tasks, and the personality measures did not support the hypotheses. Gender, however, appeared to be an important factor in the psi task.

Results of the studies are discussed and the validity of the comparison is re-assessed. Together, these studies represent a new approach to the comparison between psi and PWA, and it is hoped that further research will be sensitive to developments in the PWA field.

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Chapter 1 – Introduction, definitions and outline of thesis

Introduction

Throughout history people have reported strange experiences. There have been instances of people apparently knowing what will happen in the future. Cases where people separated by a great distance appear to have some linkage, seemingly allowing communication. Situations in which an individual claims to have obtained information about an object or a place by means that are beyond their normal sensory capabilities. There are many examples, in all cultures, of phenomena suggesting that the mind may have abilities beyond those that it is normally credited with. Abilities we might call “psychic”. But these phenomena have been traditionally ignored by mainstream science. This is unfortunate. Regardless of what might underlie these experiences, it is undeniable that they have been a part of human experience throughout all of history. This fact alone makes them a worthwhile topic of investigation. Whenever scientists have considered these phenomena, they have traditionally taken the role of sceptical observer, attempting to explain the experience in terms of statistics, or delusions, or by some “normal”, well understood factors. While there is nothing wrong with this approach, it fails to look at the whole picture. It may be the case that these experiences represent something more than mere statistical anomalies, delusions, deliberate fraud, etc. Perhaps there is something else behind these experiences. Something that mainstream science has yet to confront. It is possible that these experiences represent instances of organisms interacting with the environment in ways which we currently do not understand. If this were the case, then we might call such instances “paranormal”. The term paranormal tends to have an emotive component that causes many people, particularly those in science disciplines, to react against it with suspicion. For the purposes of the present thesis, the term “paranormal” shall refer to phenomena which

appear to exceed the limits of what is deemed possible according to current scientific assumptions (Journal of Parapsychology Glossary, 1999).

In studying ostensible paranormal phenomena, it is important not to rule out *a priori* any hypothesis, no matter how much it may contradict the accepted model of the world. On the other hand, it is also important to fully understand the factors that may give rise to such experiences without positing anything paranormal. Perhaps there are environmental factors that give rise to the types of experience that are traditionally labelled as psychic. These environmental cues may be extremely subtle. It is possible that they are so subtle, that they are not consciously noticed by the individual. If the cause of the experience is not known, then there is a great temptation to conclude that something paranormal was playing a part. Mainstream psychology has closely studied many phenomena that could conceivably play a role in apparently psychic experiences, although the link has rarely been made outside of parapsychology. One such phenomenon is currently known as perception without awareness (PWA). Perception without awareness is a cognitive phenomenon whereby a stimulus which is completely outside an individual's conscious awareness, enters the cognitive system and can have an influence (see below for more detailed definitions of PWA and "conscious awareness"). If an individual's behaviour or cognitions can be influenced without their knowledge, then this might appear to them to be similar to "psychic" phenomena. To the individual, all that he/she is aware of is the final outcome, i.e. the behaviour or cognition. If this behaviour or cognition is sufficiently salient as to be noticed by the individual as strange, then they may ponder what gave rise to it. If the source is a weak sensory stimulus, perceived outside of awareness, then the individual will have difficulty making an accurate attribution as to the cause of the behaviour/cognition. Thus, it is possible that the event will be interpreted as being "psychic" in nature. It is important for those interested in "psychic" experiences to be well versed in the various ways in which "normal" factors might play a part in psychic events. To fail to do so may lead to grave errors when assessing the experiences. Likewise, it is also important to consider that there may be a paranormal component to the experiences. Again, to rule this out would be to fail to see the bigger

picture. It is possible that a particular behaviour or cognition was influenced by *another* factor. Perhaps there was no weak sensory stimulus (in the traditionally understood sense), and the transfer of information between the individual and the environment occurred in some other way, which is not presently understood. To the individual experiencing the phenomenon, there might be no discernible difference between the two forms of influence. The effects may be identical. But the factors underlying the effects might be extremely different.

But how are they to be teased apart? How do we know what factors are at work? Through the scientific method. Science has given us the tools to study these phenomena in a controlled, systematic way. By studying them in a controlled environment, we are in a better position when attempting to discern what factors might play a role.

It is important to note that it is not an “either/or” scenario. It is entirely possible that both normal *and* paranormal factors contribute to the types of experiences labelled as “psychic”. Thus, if they are comparable, and give rise to similar experiences, then learning about one may give us important clues concerning the nature of the other. Again, it is only by studying these things in a controlled environment that we will be able to assess the role of each, and the similarities between them.

Definitions

At this point it is worth giving some definitions of terms that will be used throughout the thesis. This is a particularly difficult thing to do due to the fact that much of the controversy in the field of perception without awareness has revolved around the meaning of the terms. For the moment, however, and with the knowledge that the controversy will be covered in a later chapter, these are the key terms that will be used throughout the thesis.

“Psi” is a generic term, used to describe interactions between an organism and its environment through means other than the recognised senses. Psi is traditionally divided

into several sub-categories. For the purposes of this thesis, the most important of these is extrasensory perception (ESP). ESP can be defined as “knowledge of or response to an external event or influence not apprehended through known sensory channels” (Wolman, 1977, p. 926). Another sub-category of psi is “precognition”; defined as “knowledge of a future event which could not have been predicted or inferred by normal means” (Wolman, 1977, p. 930). Further sub-divisions include “clairvoyance” (“extrasensory awareness of objects or objective events” Wolman 1977 p. 923), “psychokinesis” (PK - “the influence of mind on external objects or processes without the mediation of known physical energies or forces” Wolman 1977, p. 931) and “telepathy” (“extrasensory awareness of another person’s mental content or state” Wolman 1977, p. 935). These terms are generally referred to under the inclusive term “psi”, and it is this term I will predominantly use, except where clarification of the subdivision is deemed necessary.

The field which conducts research into alleged psi phenomena is known as Parapsychology.

Perception without awareness (PWA) is the current name for what has variously been called subliminal perception, implicit perception, preconscious processing, non-conscious perception, unconscious perception, subception and subsensory perception. The most common of these titles has been subliminal perception. This term, however, implies the concept of a sensory threshold (or “limen”), which is a controversial issue, and difficult to prove or disprove (see chapter 3). Perception without awareness is a relatively neutral term that accurately describes the phenomena in which we are interested. How this is operationally defined is a matter of some contention, and it is this question that chapter three will address. For the meantime, PWA is defined as an occasion whereby a percipient is influenced by a stimulus in some way, despite the stimulus being completely unavailable to conscious awareness. Closely related to this are the concepts of “conscious” and “unconscious”. These terms will be used throughout the thesis, so it is important at the outset to clarify what is meant by them. Conscious awareness is when a percipient is aware of a stimulus (or a mental process) such that

they can use the relevant information to guide action or thought (Searle, 1992). In this sense, consciousness is intimately associated with awareness. This is a very narrow definition but it is useful for the purposes of the present thesis. It is acknowledged that the current definition does not address the “hard problem” of consciousness (see Chalmers, 1996) in which the actual nature of phenomenal conscious experience is considered. For something to be unconscious, on the other hand, it must be unavailable to the individual’s conscious mind, completely outside of awareness in such a way that prohibits the individual to actively use this information to act upon the world. It is important to note that the “unconscious” of this thesis is conceptualised as a “cognitive unconscious” (see, e.g., Kihlstrom, 1987). The term “cognitive unconscious” was coined by Rozin (1976), and refers to the study of unconscious processes from a cognitive science perspective, as opposed to a psychodynamic position. Although the term “unconscious” in common usage refers to a particular state (e.g., being asleep or being in a coma), the present work uses it to describe the processes (or perceptions) that are performed outside of awareness in a nevertheless conscious (i.e. awake and alert) individual. Although the definitions outlined above may give rise to certain other questions, chapter 3 will address many of the core issues that arise as a result of using these terms (e.g. measuring awareness).

Parapsychology and Psychology

Parapsychology is inextricably linked with psychology. The very fact that human experience forms the basis for much of the work in parapsychology means that, despite also having close connections to physics, biology, and many other disciplines, it is psychology that is parapsychology’s closest relative. The links have been there implicitly since the beginnings of “psychical research” but were made explicit when Rhine re-christened the scientific investigation of ostensibly paranormal events “parapsychology”. The term “para” means “along side of” and Rhine hoped that using this term would emphasise the closeness that existed between psychology and the study of ostensible “psychic” experiences (see Edge, Morris, Palmer and Rush, 1986).

Schmeidler (1988) has comprehensively reviewed many of the parallels between parapsychology and psychology. She comments that one of the most frequent criticisms of parapsychology is that it has no theory. In response, she points out this is not the case, and that the theory is so obvious and simple that it is often overlooked. The most fundamental theory of parapsychology is that psi is a psychological function. When stated like this, it does not seem like much of a theory, but it does have testable consequences. Schmeidler (1988) states;

“[i]f psi ability is a psychological function, then psi responses will be processed as other psychological responses are. Variables that affect how other abilities are used will also affect how psi ability is used. Psychological findings about what facilitates or inhibits effective response to other tasks will correspond to parapsychological findings about facilitation or inhibition of psi scoring” (Schmeidler, 1988 pp. 6-7).

Parapsychologists aim to find a certain degree of lawfulness about psi functioning, and the best way to do this is to compare it with other, similar, psychological phenomena. Even researchers who are primarily concerned with how psi may work in terms of physics must have some form of working model concerning how psi works psychologically. Although it is possible that psi is independent and separate from all other psychological phenomena, we have no real reason to adopt this hypothesis.

The link between psychology and parapsychology also extends to what each can learn from the other. Due to its controversial nature, parapsychologists have long been aware that their methods for investigating psi will come under close scrutiny, perhaps closer scrutiny than would be afforded to other, less controversial, areas of science. It is for this reason that the general field of psychology can learn much from studying the stringent methodologies employed in parapsychological research, and what follows are some examples of what parapsychologists have brought to psychology.

Joseph Jastrow was one of the first people to be awarded a PhD in psychology in America, and, along with C.S. Pierce, conducted what was arguably the very first psychological experiment in America (see chapter 3 for details of this experiment). Kaptchuk (1998) notes that Pierce and Jastrow were responsible for one of the very first psychological studies incorporating “blind” methods. Interestingly, Pierce was one of the founding members (along with, among others, William James) of the American Society for Psychical Research (ASPR), whose formation in 1885 was inspired by its British counterpart, formed three years earlier. Both of these societies endeavoured to investigate phenomena labelled as “psychic”. Jastrow was also involved in the ASPR, serving on its Scientific Advisory Council. It is, therefore, entirely plausible that the blind methods introduced into psychology by Pierce and Jastrow were influenced by their appreciation for the importance of such methods in what was then known as psychical research. Indeed, Richet had pioneered the use of randomisation and blind methods in psychical research during the 1880s by Richet (Watt, 1999). There are many other ways in which the methodologies used in parapsychology have subsequently been adopted by the mainstream, and there is reason to suggest that this process is ongoing (see Watt, 1999).

In addition to the methodological contributions parapsychology has made to psychology, there are also various conceptual contributions. One of the biggest issues in psychology (and science in general) is the actual nature of “consciousness” (see, e.g. Chalmers, 1996). Perhaps not surprisingly, parapsychologists have had much to say on this topic. If psi is indeed a real phenomenon, then it may have far-reaching implications for how we view the nature of consciousness (see, e.g. Beloff, 1965; Child, 1984).

Purpose of thesis

Having such close ties to psychology means that parapsychologists must keep up to date with current issues in the psychological literature. Failure to do so would mean that parapsychology would evolve independently of psychology, inevitably moving away from it. If this were to happen, then it would be extremely difficult for the field of parapsychology to be taken seriously by other psychologists. It is, however, unclear just how up to date parapsychologists are in their knowledge of recent advances in psychology. The aim of the current thesis is to take a new look at an area of psychology that has long been linked with parapsychology (by parapsychologists at least). The phenomenon to be compared with psi is what was previously known as “subliminal perception”, but is now called “perception without awareness”.

Despite the close associations between the two fields, and the proclamations made by parapsychologists concerning the overlaps, it has been over fifteen years since there was any systematic research looking into the comparison. In these years, the psychological literature has moved on a great deal, and there have been major methodological advances. These, however, seem to have gone unnoticed (or at least uncommented on) by parapsychologists, even the ones who still hold that the comparison is a valid one. Thus, the current thesis aims to bring the comparison up to date, using new techniques, avoiding old criticisms and hopefully assessing whether the comparison is still valid given the changed climate in which it is made.

Does psi exist?

There is a distinction in parapsychology between “proof-oriented” and “process-oriented” research (see, e.g. Honorton, 1975). Proof-oriented research aims to convincingly demonstrate the reality of “psi”¹, while process-oriented research is more concerned with questions regarding how psi may work. Research aimed solely at

¹ Although it is the case that the term “proof” should be reserved for exclusive use in mathematics and statistics, it is used here to illustrate the distinction between research solely aimed at demonstrating psi, and research aimed at investigating the processes involved.

demonstrating psi's existence is now relatively rare. Most studies in parapsychology take the existence of psi as a working hypothesis, based on previous work that seems to suggest that psi is a real effect and can be studied in the laboratory (see, e.g. Radin, 1997 for an overview of the evidence in favour of psi's existence). In taking psi as a working hypothesis, researchers are then able to incorporate various components looking at how it might work or what psychological correlates it may have. This is not to say, however, that the reality of psi is not in question. This is as much in dispute as it ever has been. In 1999, Milton and Wiseman once again raised the most fundamental question in parapsychology: Does psi exist? The way they chose to approach this question was through a meta-analysis of "Ganzfeld" ESP experiments.² The Ganzfeld technique has been at the forefront of ESP research since it was introduced into the field in the 1970s. The reason for its popularity has been its apparent success in providing evidence of ESP. In 1994 Bem and Honorton conducted a meta-analysis on the most advanced ganzfeld studies. Their analysis revealed positive and statistically significant overall results, suggesting that the psi might actually be a real effect as tested in these conditions. This analysis has been cited by parapsychologists as demonstrating the reality of psi (or, at least, of ESP). In 1999, Milton and Wiseman conducted an updated meta-analysis of ganzfeld studies and obtained non-significant overall results and a near-zero effect size. However, meta-analyses are not wholly objective methods to analyse data. The process involves the individual(s) conducting the analysis making various judgements concerning which studies are appropriate for inclusion, what aspects of the individual studies are important to pay attention to, etc. A great deal of subjectivity is involved during this coding procedure, and it is vulnerable to potential biases, either leading to conservatism resulting in a type 2 error, or liberalism leading to a type 1 error. These biases may not necessarily be intentional, but they probably do derive from the initial starting position of the person conducting the analysis. As such, two people starting from different positions could conceivably obtain radically different results when meta-

² This technique attempts to reduce external stimuli (and thus internal 'noise') to a minimum in the hope that any weak extra-sensory input will be attended to. It also aims to promote free imagery related to the target.

analysing the same data set. Indeed, this has previously been found in parapsychology (Hyman and Honorton, 1986). For these reasons, Milton and Wiseman's (1999) meta-analysis has been criticised within the field of parapsychology, with critics pointing to the way they chose to deal with the data as being a possible reason for their null results. Additionally, Storm and Ertel (2001) conducted their own analysis and reported significant results indicating a psi effect (though see Milton and Wiseman, 2001 for a critique of this). In an attempt to allow the parapsychological community to discuss the issues raised by the Milton and Wiseman (1999) meta-analysis, Milton (1999) coordinated an e-mail discussion that was edited for publication by Schmeidler and Edge (1999). Furthermore, Bem, Palmer and Broughton (2001) present an updated meta-analysis, including ten studies conducted since the Milton and Wiseman (1999) analysis. Bem *et al* note that the effect is significant, and there is a correlation between the effect size of a particular study, and how much it conforms to a "traditional" ganzfeld procedure (although what a "traditional" ganzfeld consists of is a contentious issue, as noted in the e-mail debate; Schmeidler and Edge, 1999).

The exchanges concerning the evidence (or lack thereof) for psi since the Milton and Wiseman meta-analysis serve as a microcosm for debate in parapsychology since Rhine brought the study of psychic phenomena into the modern era (Rhine, 1934). The bottom line is that the debates concerning the existence of psi still rage on, and there would appear to be no easy resolution to them.

Given that there is no consensus concerning the reality of psi, and given that proof-oriented research does not really add a great deal to knowledge, then the current thesis takes the existence of psi as a working hypothesis. Essentially, the question we are concerned with is not "does psi exist?", rather "if psi does exist, how is it likely to work?". However, it is useful to bear in mind that whenever "psi" is mentioned in the thesis, its existence is not assumed to be established beyond doubt. As such, "psi" may be considered shorthand for "the psi hypothesis", which carries with it the caveat that this hypothesis may or may not be correct.

Outline of Thesis.

As stated above, the primary question of the thesis concerns exploring one possible way in which psi might work, and whether its mode of functioning can be compared with a similar psychological phenomenon. In asking the question “how does psi work”, the current thesis is more concerned with the cognitive mechanisms involved in processing psi information than any physical process that has been posited for psi. In other words, the question concerns the way in which psi information is processed once it is present in the cognitive system. Chapter 2 reviews literature on this issue, paying particular attention to the models proposed by Stanford (1974a, 1977, 1982, 1990) and Irwin (1979). Chapter 3 consists of a review of the perception without awareness literature, tracking the evolution of the field, in terms of the criticisms it has faced and the way in which these criticisms have been addressed. Chapter 4 reviews the work that has compared psi and PWA over the years, before assessing this work in light of the more recent developments in PWA. The following three chapters describe a series of experiments aiming to compare psi and PWA. The first experiment (1a) takes a parapsychological effect and attempts to replicate it with some degree of success. The second experiment (1b) attempts to recreate this effect using new techniques in the field of PWA. The next pair of experiments take the opposite approach. Experiment 2a is a replication of an established effect in the field of PWA, while experiment 2b is an attempt to obtain this effect using psi. Experiment 3 takes the most promising methodologies from the previous studies, and combines them in a within subjects design. Additionally, study 3 has various exploratory components, aimed at shedding light on any possible correlation between psi performance and PWA performance in the within subjects experiment. The final chapter summarises and synthesises the findings of these experiments, before making some suggestions for future directions.

Chapter 2 - How does psi work?

“If you want to know how it works, first find out what it’s for”

Richard Broughton, 1987

General Introduction

This purpose of this section is to outline what parapsychologists have had to say concerning how psi information might be processed by the cognitive system. It is important to note, at this point, that no assumptions are being made about how the psi information actually reaches the cognitive system. While psi (particularly ESP) has been traditionally thought of as some form of signal (see, e.g. Sinclair, 1962), more recent theorists have questioned this view, based on the suggestion that psi is independent of space and time (see, e.g. Walker, 1979). While it is possible that there may be some kind of as yet unidentified signal transferring psi information, this is not relevant to the present discussion, which concerns how psi information might actually be processed once in the system. Although at some points the term ‘psi input’ may be employed, this does not imply that any particular model concerning the nature of the psi ‘signal’ is being expounded.

Before actually moving on to theories of psi processing, I will first outline some of the arguments that have been put forward for psi’s place in nature. This is important for two reasons. Firstly, it is a worthwhile venture to place various human abilities within an evolutionary framework. Pondering on why an ability might exist and what use it might be will often lead to other speculations concerning how it might work. It is hoped that, from this discussion will emerge a justification for psi being an unconscious process. I will then give an account of what various authors have said about the cognitive nature of psi. This is intended as an introduction to approaching psi from a cognitive standpoint,

leading into some of the theoretical work that has been proposed about its functioning. In the following chapter, a comparison will be made between unconscious psi processing, and the psychological phenomenon known as perception without awareness.

Why Psi? – Psi and its place in nature

Before we ask the important question about how psi might work, it is important to take a step back and consider why psi might exist in the first place. This is a question which is often overlooked by parapsychologists, which is unfortunate, as asking it can lead to further insights concerning the nature of the phenomenon.

Implicit in most psi research is that psi is an ability¹ (though see White, 1985 for an argument to abandon this view). This is evident in most of the experimental work, in which the ability is teased out under specific experimental conditions. Like all abilities, psi must have evolved over time for a reason, and if it has survived the process of natural selection, then it must be of some use to the organism. It is unlikely psi would have survived the process of natural selection if it had no purpose.

Jule Eisenbud was among the first to start asking these questions. In his seminal paper “Why Psi” (1966-67), Eisenbud contemplates why psi might exist, and explores the consequences of this in terms of the way psi might work. Indeed, he states explicitly that “nature would hardly be so foolish and wasteful as to contrive a great ‘force’ and then not use it economically” (p161).

He suggests a role for psi in which its influence goes beyond that of precognitive dreams and other impressive, ‘large-scale’ manifestations, postulating that psi might also have a part to play in everyday life “determining where [an individual] goes, whom he meets, what he does, and so forth” (p151). He even goes as far as to claim that psi is operating at some level in *all* events (presumably meaning all events involving living organisms).

¹ The term ‘ability’ when applied to psi is a contentious one. I use it here in the sense that psi would appear to be linked to an individual’s personality in a way a ‘capacity’ such as blinking is not. See Braude (1992) for a discussion on the relative merits of treating psi as an ‘ability’.

Eisenbud holds that psi is likely to be a widely distributed function, the purpose of which is to assist the organism in achieving certain goals. These goals need not be consciously held, but may instead result “from the interplay of [its] unconscious needs, determined by [its] genetic and psychosocial history...” (p152). He then goes on to emphasise the evolutionary significance of psi, suggesting that it might act as a linking mechanism between organisms, serving to moderate the fine balance of the ecology in which the organism exists. Thus Eisenbud can, in some way, explain why certain animals, in a way contrary to survival, appear to surrender themselves to predators. It is, according to Eisenbud, all part of a more complex system of equilibrium in which many factors, including the need of the predator and (say) the need for population control among the species of the prey, play a part. Eisenbud’s assertion is that psi has an important role to play in this complex process, acting as a means of information transfer between component parts of independent systems (e.g. between species in an ecosystem).

In terms of how psi might work, Eisenbud states that “psi must operate at an unconscious level, beyond voluntary control...[and] the goals it serves, as is the case also with the rest of the functions in the cognitive spectrum, may often be at variance with those which are consciously espoused...” (p149). The proposal that psi is unconscious is important, as if it was not, then the world would be full of people volitionally using their psi for their own ends. This is clearly not the case. If psi does indeed exist, then it would appear to be typically weak and unreliable

Eisenbud’s paper was highly influential as it influenced researchers (e.g. Rex Stanford; see below) to think about why psi might exist in the first place and to propose a place for it in our lives. However, the paper does, at times, give the impression that Eisenbud is only too keen to use psi as a convenient explanatory tool which can fill various gaps in knowledge (e.g. how species can relay information related to keeping the ecology in a balanced state). It is important that parapsychologists avoid implicating psi whenever

there appears to be a puzzle in nature that has yet to be addressed, especially when there is little justification in doing so.

The idea of psi as an unconscious process has been around since the beginning of parapsychology itself (see following chapter). The implications of this idea are far-reaching. Some parapsychologists have gone as far as to state that the primary function of psi might be an unconscious one. It was this idea that led Targ and Puthoff (1974) to suggest that psi ability may be “widely distributed in the general population, but because the perception is generally below an individual’s level of awareness, it is repressed or not noticed” (p 607).

In 1987 Richard Broughton speculated on the role of psi. His presidential address to the Parapsychology Association was appropriately entitled “*If you want to know how it works, first find out what it’s for*”. Broughton draws on many of the same themes introduced by Eisenbud in 1966, and his paper discusses the uses of psi. He begins by returning to the ‘psi as ability’ concept, stating that throughout the history of parapsychology, psi has not been taken seriously *enough* as an ability. Broughton then goes on to attack the popular notion that psi is an extension of other communicative abilities. This hypothesis holds that ESP serves as a complementary element to normal sensory functions. In other words, while our ‘normal’ senses are limited in scope in terms of distance and time, psi functioning offers an extension to them, with ESP allowing us to ‘see’ (or hear, or smell, etc.) things that we couldn’t be able to through the ‘normal’ channels. Likewise, while our physical influence on the world is normally limited to our immediate environment, PK would offer a form of extended motor action, allowing us to physically influence things which we would not be able to using our physical methods. However, there is a problem in adopting this position. Both ESP and PK are woefully inefficient if this is their true function. Rarely do we “see” something through ESP like we would see it through our normal senses, or move something through PK like we would move it normally. If these traditional views are to be abandoned, then we must find another function for psi. Broughton proposes that psi is

fundamentally need serving and suggests that, if we are to treat psi as an ability, then this is the only logical way to consider it. Like Eisenbud, Broughton claims that survival is the key to psi's existence, although his proposals are far more modest than those of Eisenbud. What Broughton is concerned with is the function psi might play in the organism's continued struggle for survival. He does not address the larger question of how psi might be a linking mechanism in a complex system, but is instead interested in the role it can play for the individual.

Psi functioning, according to Broughton, would be a very useful ability in nature. It would be extremely beneficial in many situations, giving the organism an extra advantage that may be the difference between continued existence or extinction. Of course, too much psi present in one individual or one species would create an imbalance. If an imbalance exists, then although it may benefit certain individuals in the short term, it will be detrimental to the group or species in the long term. It wouldn't be long before predators or rivals worked out that psi users were a threat, and therefore to be eliminated. This is why Broughton proposes that psi behaviour must be an Evolutionarily Stable Strategy (ESS). The concept of the ESS was introduced to evolution theory by John Maynard Smith (1974), who in turn borrowed it from the field of Economics. An ESS is essentially a pattern of behaviour which, if adopted by most members of a population cannot be bettered by any other strategy. For example, an ESS may call for an individual to be aggressive 60% of the time and submissive 40% of the time. Over the course of evolution this may have turned out to be the most beneficial and stable strategy. Of course, not all strategies in existence at any one time will be the most stable. This is a process that takes a long indefinite period. The important point to note is that the strategies are *unconscious* in nature, and are becoming more and more stable over time.

Likewise, psi behaviour in a population must be, if not an ESS, then at least a behaviour which will eventually become one. Broughton states that it is no accident that psi is unconscious and obscure. He states that;

“...the most effective psi might be imperceptible psi. If one’s psi becomes ostentatious then it could get one into trouble...Psi could have evolved to be deliberately self-obscuring for its own purposes; that is, it works best and most unhampered when it is not noticed by the individual it is serving, and it may even be necessary for the individual’s protection that the operation remain hidden”
(Broughton, 1987, pp 196-197).

Broughton fails to qualify his thoughts regarding the imperceptible nature of psi. He does state that “...it is only in recent times that too visible a display of psi did not severely reduce one’s chances of success” (p. 197). Why this might be the case is not made clear, however. Perhaps, as suggested above, displaying blatant psi abilities is seen as a threat by competitors. This is one interpretation. Another might be in terms of one’s own self-concept if we experience ourselves using psi. This conscious awareness could possibly inhibit psi, which is likely to work much more economically if conscious anticipation is by-passed in some way (for a further discussion on this point, see section “How does PMIR work” below). Indeed, as Broughton states, many cultures have developed a system in which overt manifestations of psi are attributed to external agencies rather than individuals.

Much of Broughton’s observations in this paper are not new. But they are important. It is necessary, I believe, to keep reminding ourselves of why we might have this ability. If we do not, then the methods we develop to test for psi may well become more and more sophisticated, while at the same time being more and more removed from the true nature of the phenomena we are attempting to study. It is also important that we have some kind of theoretical framework offering hypotheses concerning the role of psi in our daily lives. A model by parapsychologist Rex Stanford (1974a, 1974b, 1977, 1982, 1990)

attempts to address these issues, but before this model is described, there follows a brief section aimed at outlining what various authors have said about psi as a cognitive process. I will then go into the models in detail.

Cognitive Parapsychology

The idea of psi as a cognitive process is not a new one. In 1947, Tyrell postulated that the psi percipient constructed, by sub-conscious paranormal means, a product called a “mediating vehicle” which is not itself a paranormal phenomenon but “is the product of psychological machinery which all possess. It may take the form of a sensory hallucination or of an impulse or of an automatic verbalisation or of a dream” (Tyrell, 1947, p. 170). The purpose of this mediating vehicle is to convey or represent the psi information in awareness. This was the basis for Tyrell’s two-stage model of psi processing, in which the psi information is initially perceived at an unconscious level. At this level it is subject to the same distortions and transformations that other non-psi info might be. Finally, the second stage of the process sees the *product* of the psi information (e.g. a dream, impulse or hallucination) emerging at a conscious level. Indeed, Tyrell states that “the same vehicle which mediates paranormal cognition also mediates subconscious expectations and beliefs or normally acquired knowledge which has not reached consciousness independently” (p. 117). Since then, other writers have been sympathetic to the idea that psi may be closely linked to other cognitive processes.

In his seminal monograph *Extrasensory Perception* (1934/1973), J.B. Rhine commented that ESP was ‘simple cognition’ (p 191), while in 1965 Louisa Rhine stated that “ESP is a process closely integrated with the rest of mental life” (p.4). Stanford (1970) likewise notes that the ESP process is likely to be “integrated and embedded in the experiences and cognitive processes of everyday life” (Stanford, 1970, p 162). It is not difficult to see the appeal of this position. By virtue of the fact that psi is a mental phenomenon, then its processing must share properties with other mental phenomena. Whatever processes are involved will bear an intimate relation with whatever other processes are going on alongside it. A psi process that exists in isolation, while possible, would be uneconomical. Instead, it makes more intuitive sense to postulate that psi makes use of,

influences, or in some way interacts with, other cognitive processes. This line of reasoning has influenced many parapsychologists, for example Rao, Kanthamani and Palmer (1990), who suggest that “a more fruitful approach to studying psi may be found in understanding its interactions with normal abilities than in attempting to investigate it in isolation” (p247).

What follows is an outline of some models that are concerned with the cognitive nature of psi. The first model is a recent attempt by Christine Hardy to place psi within a connectionist framework. I will then move on to the memory models favoured by William Roll and Harvey Irwin. Finally, I will describe in detail a model proposed by Rex Stanford, of St John’s University. It concerns the role psi might play in our everyday lives, and has much to say about the processes involved.

Hardy’s Semantic Fields theory

Recently, attempts to model the mind have focused on connectionist systems such as neural networks (see, e.g. McClelland and Rumelhart, 1986), and it is this approach that we will now briefly consider. Christine Hardy aimed to create a theory of the mind that incorporated many of the benefits of these recent advances, while still acknowledging (and attempting to account for) the aspects of the mind that most strict reductionist theories tend to conveniently ignore (e.g. creativity and conscious experience). The fact that she sub-titled her 1998 book “A Bridge Between Mind and Matter” goes some way in indicating the level of ambition Hardy has for her theory. Hardy claims that the key to understanding the mind is in studying it as both a semantic-neural network system and an ever-changing dynamic system which interacts with its social and physical environment. Hardy’s (1998) theory is known as “semantic fields theory”. Semantic fields theory holds that the mind is a lattice of “semantic constellations”. Semantic constellations are fundamental to Hardy’s theory, and she states that they represent the “building-blocks” of our mental life. A semantic constellation is an organised but ever changing network that is “constituted by virtue of its inner interconnections and its connections with other constellations” (Hardy, 1998, p. 16). In other words, a semantic

constellation is an evolving network of meanings and related processes which are organised around a nucleus representing the central meaning of the constellation. Semantic constellations are generated by the interplay of experience, genetics and culture, and each one is dedicated to a specific activity, concept or knowledge. They are thought of as self-organised dynamical networks that interweave processes ranging from high level abstract ones to low level neuronal ones. The networks are created and constantly modified by an underlying, low-level, connective dynamic which Hardy calls a *spontaneous linkage process*. This process involves clusters of semantic elements being attracted to other clusters that are semantically related, and linking with them (for a more detailed account of this theory, see Hardy, 1998). Hardy (2000) has suggested that psi may be understood in terms of her semantic fields theory. Hardy suggests that psi is a multilevel process, which can operate at various organisational levels ranging from the highly abstract to the emotional to the neuro-physiological levels. Information might activate the semantic constellation whose elements best match the target system. That information then becomes distributed in the multilevel network. The mediation of the information into consciousness is governed by a variety of factors, including the strength of the semantic linkages and the intensity of the affect attached to the event (for a fuller account of this theory see Hardy, 2000).

While Hardy's theory is fresh, innovative and certainly ambitious, it has yet to make an impact on cognitive science. Perhaps it is this freshness that make it too soon to assess the relative merits of her theory. If parapsychology is to adapt such a theory, it must first prove to be viable in accounting for normal psychological processes before being applied to psi functioning. It is for this reason that we now turn to theories which have grown out of existing theories or concepts in psychology.

Memory Models

Some parapsychologists have placed memory at the core of psi functioning. The association between ESP and memory has been around since as long ago as 1890, when William James commented on the memory performance of a séance room medium. In 1949 Pratt suggested that memory and ESP might share a common psychic origin, but it was Roll (1966) who was the first modern day parapsychologist to propose a workable model of psi and memory. Roll suggested a model for ESP in which the operation was seen to be more like memory than perception. Roll proposed that some of the percipient's memory traces may be relevant to the ESP target, and that extrasensory activation of these traces could possibly lead to mediation of the relevant information into consciousness. This mediation, according to Roll, would be governed by the laws (as much as any mental process can have 'laws') of normal cognitive processing. For example, in serial learning trials, participants are asked to memorise a series of stimuli presented sequentially. A robust effect in this type of experiment is the 'primacy/recency' effect, whereby memory for the first few items, and last few items is better than memory for the items in the middle of the sequence. A similar effect has been observed in some ESP studies where success on the initial trials is followed by a decline in performance, sometimes resulting in a flurry of 'hits' towards the end of the trials (see, e.g. Roll, 1966; Rhine, 1934). Roll's memory model engendered a substantial amount of research into the role of memory in ESP processing (e.g. Feather, 1967; Kanthamani and Rao, 1974, 1975; Harary, 1975; Parker, 1976; Lieberman, 1976; Rao, Morrison and Davis, 1977; Rao, Morrison, Davis and Freeman, 1977, Blackmore, 1979; Rao, 1978; Irwin, 1980, Rao, Kanthamani and Palmer, 1990). Much of this research was concerned with finding correlations between memory performance and ESP. For example, Feather (1967) found a significant correlation between participant's memory scores and ESP scores, and suggests that there are psychological factors that are common to both ESP and to memory. Kanthamani and Rao (1974) tested participant's memory for word pairs, and asked them to make an ESP response after every memory response. They conducted a trial-by-trial analysis of the relationship between memory and ESP, and found that trials on which memory performance was good (i.e. a correct

recall) were associated with significant ESP scoring. The same authors in 1975 found some evidence that the memory-ESP interaction is stronger with hard to remember material than with easily remembered material. Further research followed, based around the methods employed by Kanthamani and Rao (e.g. Emmerich, 1976; Harary, 1976; Parker 1976). These studies have been important in informing us on the relationship between memory and ESP. Blackmore (1979) endeavoured to discover whether ESP was more like memory or perception, but concluded that she was unable to conclusively demonstrate either.

Harvey Irwin also formulated a model of psi processing that implicated the role of memory. This model will now be described below.

Irwin's information processing approach

Irwin was another parapsychologist who recognised the cognitive nature of psi. His model was an attempt to integrate psi within an information-processing model, which is successful in accounting for various other cognitive phenomena. Irwin's aim was to take these accounts of normal psychological processes, and apply them to so-called 'paranormal' processes.

Irwin makes the distinction between pseudo-sensory models of ESP and memory models. The pseudo-sensory models, in their most extreme form, posit some unknown sense organ (often called a 'third eye') that receives ostensibly extrasensory information. While this extreme view is rejected by the majority of parapsychologists, more diluted versions are commonly held. Irwin offers the possibility that extrasensory information may have its roots at a level of preconscious processing, and that, after this stage, it is subject to the same processes as a normal sensory input.

Memory models are, according to Irwin, more like thought processes than perception. In these models, the extrasensory input has its origin in memory. Roll's (1966) theory is, perhaps, the best example of a memory model. Memory models can be further sub-

divided into those in which psi activates existing memories, and those in which psi causes the acquisition of new memories.

In formulating a model, Irwin favours a memory model over a pseudo-sensory one. He justifies this in a number of ways. Firstly, he refutes the assumption of pseudo-sensory models that the input must be some form of physical energy. While this may be possible for forms of telepathy, Irwin claims that it is not viable in the cases of clairvoyance and precognition. His second objection to pseudo-sensory models involves the processing of extrasensory information at the level of pattern recognition. Irwin's position is that psi processing must enter the system at or below the level of pattern recognition. In a pseudo-sensory model, this leads to the assumption that psi perception must vary systematically with target discriminability, which has been shown, in various studies (e.g. Rao, 1966) not to be the case. While this may be true if one is considering psi processing as something that gives rise to conscious perceptions, it is possible to conceive a different type of process in which such rigid constraints are not required. Stanford's idea that psi may be a way of producing subtle behavioural effects suggests that psi might work in a way in which pattern recognition is not of fundamental importance.

Irwin's model of ESP processing is one in which established memory traces within the organism can be activated through so-called 'paranormal' means. Furthermore, Irwin contends that it is the structural stratum of memory that is essential to psi processing. He cites Sinclair (1962), who describes a telepathy experiment in which the percipient was attempting to guess what the sender (or agent) had drawn. Despite recreating a strikingly similar image, the percipient was subsequently unable to identify what it actually represented. Irwin argues that this is an example of the structural stratum of memory being activated while other strata that may have facilitated in recognition and naming the target (such as the semantic stratum), were not. The flaw with Irwin's reasoning here is that he is only considering one type of psi process. Most of the evidence he presents is based on the observation that many of the errors in conscious ESP tests tend to be

structural in nature. While it may be the case that conscious psi processing may rely on structural components, the idea fails to account for the more subtle forms of unconscious psi in which the structure of the 'target' does not appear to play a major role. A further criticism of this approach is that many of the examples Irwin gives may not be due to any psi processing at all. It is possible that the same errors could be made if the percipient had gained information about the target through normal sensory means. If this was the case, then they could reproduce the structure of the target, but may fail to identify what it actually represented, thus displaying the same pattern of errors that Irwin cites as evidence for a structural component to psi processing. This criticism is given more weight when one considers that many of the studies cited by Irwin are extremely old (e.g. Bender, 1936; Warcollier, 1948; both cited in Irwin, 1981) and may have employed less stringent methodologies than we would now consider adequate.

Irwin describes his model as dealing with non-intentional psi. He holds that memory traces that are relevant to the psi source can become activated. These psi-activated traces consist of coded representations of the sensory features of an input with structural relevance to the psi source. These primitive sensory features are then subjected to pattern recognition at a preconscious level. As the psi input is inherently weak, then it is at this stage of pattern recognition that structural errors can be made. For example, Irwin suggests that the structural features relating to a 'B' may be confused with those relating to an '8'. Once the information has been 'recognised' at a preconscious level, it can then be subjected to semantic analysis, again at a preconscious level. At this stage it may be evaluated for qualities such as pleasantness or unpleasantness, and it is also at this stage that phenomena such as perceptual defence are initiated (see, e.g. Bruner and Postman, 1947). It is after all this analysis that the information can be finally transferred to consciousness and a response elicited. The model itself does not encompass as wide a range of phenomena as Stanford's PMIR model.

We will now turn to Stanford's model of psi functioning, known as the "psi-mediated instrumental response" (PMIR) model. This model also has a role for memory and Irwin

recognises this, stating that it could possibly be elaborated into a memory model, despite being concerned primarily with teleological issues. Despite acknowledging the role of memory, the PMIR model does not have it as a central component of psi processing. Instead, the PMIR model suggests a wide range of cognitive processes may be involved in unconscious psi.

Stanford's PMIR

In the mid-1970s, Stanford published important theoretical work on the way psi may work. Stanford was influenced by the writings of Eisenbud (1966-67), and he undoubtedly influenced Broughton's (1987) presidential address.

Stanford (1974a, 1974b, 1977, 1982, 1990) wanted to create an experimentally testable model of psi which could incorporate spontaneous psi events. One doesn't have to look too hard to find accounts of spontaneous occurrences offered as examples of psi either by the percipient or by an investigator. Rhine (1978) has emphasised the importance of spontaneous cases in the way they might guide our understanding of the psi process, stating "observations from [spontaneous cases] should offer useful suggestions as to the process that produced them" (p. 21). Rhine collected many spontaneous cases and found that the majority of them could be placed into one of four categories. The first category was "intuitive impressions". These experiences involve the percipient "just knowing" something that, upon subsequent investigation, turned out to be true. For example, Irwin 1999 offers a personal example of this class of experience. Irwin states that, just prior to embarking on a long road trip with his family, he had an impression concerning the safety of one of the wheels of the car. He states that there was no logical reason for this, but, during the trip, the same wheel that had caused him concern actually did blow out. A second form of psi experience classified by Rhine was that of hallucinations. In these experiences, the "message" is contained in the form of a sensory hallucination. A classic example of this type of experience is known as "crisis apparitions" in which the percipient sees an apparition of a loved one (who is in some distant place) who, as it turns out, was in some kind of danger at the same time the percipient saw the apparition.

The final two classes of spontaneous cases proposed by Rhine involve dreams. In classifying ostensible psi occurrences in dreams, Rhine distinguished between “realistic” dreams and “unrealistic” dreams. Realistic dreams are those in which information is acquired by way of a clear, realistic mental image. The most common example of this form of psi experience is the “precognitive” dream, in which the content of the dream is confirmed at a later time. Unrealistic dreams contain imagery that is of a fanciful or unreal sort. The information contained in unrealistic dreams is commonly symbolic, and is open to interpretation by the percipient.

According to Rhine, the relative incidence of these types of experiences was as follows: intuitive 26%, hallucinatory 9%, realistic dreams 44%, unrealistic dreams 21%. These figures may be somewhat misleading, however. As will be expounded below, the number of spontaneous experiences that appear in the case literature share the characteristic that they were “spectacular” enough in the view of the percipient to inspire an active effort to report them. It is likely, therefore, that many more spontaneous cases go unreported, as they are not considered salient enough to deserve reporting. Of these unreported cases, there are probably a large number of “intuitive” cases. Instances such as being at the right place at the right time, or useful coincidences or lucky breaks may (or may not) represent the way psi works in “everyday” life. These events, however, are probably not reported and instead put down to quirks of fate, luck, chance, etc.

One thing most of the spontaneous cases have in common is that the information gained by the percipient has meaning to them. More often than not, this meaning relates to some form of need. In the anecdote offered by Irwin, there was an obvious need to avoid being injured as a result of the faulty wheel. It would appear that most ostensible psi cases relate to some kind of need in the individual. There are very few cases in which it appears as if psi has happened for no apparent purpose.

Spontaneous cases, however, suffer from a number of other problems. Firstly, as briefly mentioned above, there is the limitation that the experiencing person must interpret the

experience, either as psychic, or at least as anomalous, before reporting it. This means that there may be many other forms of psi experience which never get reported because they are never recognised as such. A second major problem with spontaneous cases is in determining whether they are 'authentic' cases of psychic functioning (if there is such a thing). When one is confronted by an instance of apparent spontaneous psi, there are two explanatory options. The experience can be interpreted as a genuine instance of psi, or it can be interpreted in terms of normal, non-psi factors. The major problem in studying spontaneous experiences is that it is frequently impossible to determine which interpretation is correct. Usually, any spontaneous experience is studied after the event has happened. Thus, getting an adequate account of what exactly happened, and under what conditions, is impossible. Consequently, if one is attempting to confirm or deny that the experience is due to psi, it is almost completely impossible to rule out the influence of normal non-psi factors, simply due to the lack of knowledge about the conditions in which the experience came about. This, of course, is why laboratory studies, under controlled conditions, have become the most accepted way of studying ostensible psi phenomena. While this makes for good science, it tends to de-emphasise the importance of the spontaneous experience, which is, after all, one of the primary reasons that anyone started investigating psi phenomena in the first place. The PMIR (psi-mediated instrumental response) model is an attempt to reconcile spontaneous experience with laboratory conditions. It is, in my view, the most complete theory concerned with how psi might function in the 'real world'. It incorporates many aspects of conventional psychology and cognitive science, and is easily testable. As such, a detailed account of the theory shall be expounded here, starting with some general aspects, before moving on to how PMIR might interact with and influence cognitive processes.

Stanford recognised the importance of cognitive processes in mediating psi information. In 1970, four years before formally presenting the PMIR model, Stanford was conducting studies relating to the way in which psi information might interact with cognitive processes such as memory (see below for details). In 1974, Stanford proposed

the PMIR model for spontaneous psi events. Stanford's starting point was a thorough review of the spontaneous case literature, and also reflection on his own experience. In doing this he noted that most spontaneous psi events were non-intentional in nature in that there was rarely any deliberate attempt by the individual to 'be psychic'. Stanford also noted that the type of spontaneous cases reported in the literature probably represents a "biased misrepresentative sample of the spontaneous psi events which actually occur" (Stanford 1977, p. 840). The way in which Stanford believed the sample to be biased was due to the strong focus on perceptual-cognitive cases. He believed that extrasensory influences on the organism may occur in more subtle ways, which would make them unrecognisable as psi events. For example, he claimed "ESP may occur in ways or in situations in which it is seldom if ever recognised, and, even if recognised, it may not be reported because it is not regarded as highly evidential." (Stanford, 1974a, p. 35). If this were the case, then it would result in many psi experiences going unnoticed, with the most obvious perceptual-cognitive cases being merely the 'tip of the iceberg'. The kind of things that Stanford was concerned with were those little everyday happenings that most people attribute to chance, good fortune, intuition etc. These may, according to Stanford, be examples of the subtle influence of psi in our everyday life: working consistently in the background, rather than announcing itself with a fanfare with relative rarity. Perhaps the most important observation Stanford made was that most, if not all, of the spontaneous cases he looked at, appeared to fulfil some kind of need in the percipient. Psi (if I may make the assumption that psi was present in at least some of these cases) always appeared to serve the percipient in some way. It never seemed to happen for no apparent reason². Already, the similarities with Eisenbud's work should be clear.

The PMIR model questions the assumption that the basic operating characteristic of psi is to produce perceptual experiences, and that what people experience when they encounter an extrasensory event is a degraded version of the relevant information.

² Although it may be possible that the result of a psi process is only noticed when it serves a need, and is otherwise ignored.

Instead, PMIR replaces these with the assumption that psi is actually a subtle way of producing behavioural effects in order to service one's own needs. This is not to say that psi *never* produces perceptual experiences. The latter version of the model can account for these types of experiences (see below), but the implication is that these explicit manifestations are the exception rather than the rule.

In his initial paper, dealing exclusively with ESP, Stanford (1974a) proposed nine testable assumptions. This was followed by a paper dealing with PK (Stanford, 1974b), which added another nine assumptions. The assumptions are too lengthy to describe in detail, but a brief summary of the initial model for ESP is as follows;

The organism uses psi (ESP) to scan its environment for need-related information. When this is found, a disposition towards a psi-mediated instrumental response arises, in that the organism will tend to act in ways which are instrumental in satisfying its needs (in relation to the need-relevant information obtained through psi). Then, appropriate preparatory responses occur, which may take the form of arousal, attention-focusing, or some other response that prepares the organism for a response. The strength of the disposition towards PMIR will be determined by various factors, including the strength of the need and the closeness in time of the potential encounter with the need-relevant object. PMIR can (but need not always) occur without a conscious effort either to use psi, or even fulfil the need; without prior sensory knowledge of the need relevant info; without conscious perceptions and without awareness that anything extraordinary is happening.

PMIR occurs through psi-mediated facilitation or triggering of otherwise ready or available responses and is accomplished in the most economical way possible (see section 'How does PMIR work' below). There are certain factors which can limit the functioning of PMIR. Some of these can be psychological and some can be situational. The thing such limiting factors have in common is that they somehow *prohibit* the spontaneous behaviour that might be required in order for PMIR to work. PMIR type processes work by relying on 'tipping the balance' on a particular behaviour or

cognition. In order to do this, the behaviour in question must not be subject to stronger, competing factors that prevent it being elicited. One such factor is behavioural rigidity. This is when a block of time is planned out so carefully that any deviations from it are rare. In this situation, any kind of PMIR response will be blocked due to the rigidity of the schedule forbidding the type of spontaneous behaviours that psi (according to the PMIR model) might normally prime in order to fulfil a need. For example, if an individual has a very rigid pattern that they follow without any deviations, then any PMIR type influence would not become manifest as it would require a deviation from this pattern of behaviour. There are also factors which dispose towards the *misuse* of PMIR in ways which would normally be considered as against the organism's well-being. Again, this happens at an unconscious level, and may be attributed to psychological factors (e.g. neuroticism or a low self-concept). For example, Stanford (1974a) states that those with a strongly negative self-concept may use PMIR in a way that is not in their interests, in order to validate their self-concept. This is a contentious issue, however, and needs to be experimentally tested.

This is the framework for the model. Stanford then went about testing his assumptions and revising the model as was appropriate in light of new data. The model underwent several revisions, and some major changes were made. The most significant change was the abandonment of the scanning component, which occurred in the 1982 revision. This was omitted due to the enormous processing capacity required for an organism to continuously scan its environment (assuming, of course, that the 'psi-scan' would use normal processing capacity, which may not necessarily be the case). A further problem with the scanning component was the lack of boundary conditions. ESP is, by definition, some form of interaction with the environment through means other than the recognised senses. Thus, almost anything in the environment could be potentially scanned. In addition to this, many parapsychologists have suggested that psi may also be independent of *time* (see, e.g. Dunne, Jahn and Nelson, 1983; Targ and Harary, 1984), resulting in the possibility that information from the future may also be potentially available to the organism. So if the organism were to scan its environment, then there

would appear to be an enormous (perhaps infinite) mass of relevant information to process, and Stanford acknowledged the enormous problems posed by the lack of boundary conditions. In abandoning the scanning component, Stanford appealed to work suggesting that psi may be goal-oriented in nature (e.g. Schmidt, 1974, 1975). If psi is goal oriented then the scanning component is no longer necessary, as it will be a single-step process, with the ends more important than the means. Exactly how much processing is required for such a process is unclear, but it is almost certainly less than that required for the organism to psi-scan its environment and process this massive amount of information. Subsequently, what the organism picks up through ESP, is information which is directly related to goals. Although the scanning component was clumsy, the goal-oriented aspect which replaces it is a somewhat grey area, and leaves a considerable gap in the theory. For example, there is some evidence (e.g. Vassy, 1986) that psi (or precognition at least) is not goal oriented, and is in some way dependent on task complexity. Vassy conducted a precognition experiment that varied the informational complexity of the target material (defined in terms of pseudo-random sequence lengths). If precognition was inherently goal-oriented, then the complexity of the information should have no effect on performance. However, Vassy found that, when psychological factors were kept constant, there did appear to be some dependency of success on the length of the sequence. If this research is valid, then it would rule out the single-step process required for psi to be considered as goal-oriented. Stanford himself (1990) appears very unsure of this concept, but retains it as a working assumption due to it presenting fewer conceptual difficulties than the scanning component. There are also problems defining exactly what a goal might be. Although the scanning component causes major problems, it would become feasible (and perhaps preferable to the goal-orientation component) were some boundary conditions specified for psi. Unfortunately, at this time at least, parapsychologists are not in a position to define the conditions under which psi functioning will and will not work. This is a problem which is unique to parapsychology. In all other areas of sensory psychology, conditions can be posited which place limits on what kinds of information can be acquired. Even in the field of perception without awareness (see next chapter) boundary

conditions are accepted in principle (although major disagreements exist in how we might define and measure them).

Related to the scanning component is the need component. The need component was considered to be too narrowly defined. As a result of this, various spontaneous experiences could not be explainable in terms of needs. For example, many reported experiences involve an inexplicable feeling of anxiety prior to receiving bad news. This is difficult to reconcile with the definition of need related in the original versions of the PMIR model. So, in 1990, Stanford revised the need component. He proposed that “dispositions” was a better construct, of which “needs” could be construed as a special case. Thus, the relevant revision read “the organism will respond to psi-mediated information about particular circumstances if those circumstances are of a kind to which it would respond if it had sensory knowledge of them. The extrasensory response which the organism makes tends to serve the needs or inclinations of that organism with respect to the aforementioned circumstances” (Stanford, 1990, p 60). In making this amendment, Stanford’s model could account for a wider range of spontaneous psi phenomena, including cases where a conscious cognition/perception was central to the experience

The assumptions that psi processing is unconscious, and that it operates in response to needs are arguably the most important assumptions of the model. There is considerable experimental evidence that psi can operate without conscious intention. Early studies on this were conducted in an academic examination environment. Much of this work (e.g. Johnson, 1973; Schechter, 1977; Braud, 1975) seemed to suggest that students could be influenced by concealed information relating to the exam they were sitting. The methods used and the statistics applied, however, cast doubt on these results, and have been criticised by Stanford (1990).

There is other work suggestive of non-intentional ESP. Carpenter (1971) found that subjects responded appropriately (according to their score on an anxiety scale) to ESP

targets that were paired with erotic stimuli unknown to the participants. Similarly, Johnson and Nordbeck (1972) found that a single subject tended to show psi-missing³ when the ESP targets were words relating to unpleasant experiences, and psi-hitting when they related to pleasant experiences. Johnson (1971) found a comparable result using unselected subjects and emotionally loaded targets. Ballard (1980) conducted two experiments on this theme. In his first experiment, Ballard found that psi targets paired with erotic stimuli led to significantly more ‘hits’ than neutral targets. In his second experiment, he found that, rather than the effect resulting from the erotic nature of the targets, it may have been how novel they were that was driving responses, finding that targets paired with stimuli judged to be novel elicited more hits than targets paired with erotic stimuli. What these studies seem to suggest is that people can unconsciously use ESP in order to determine the nature of a target, and formulate an appropriate response. The major drawback with these studies is that, although certain aspects of the tests were hidden from the participants, there was already an overt ESP component, meaning that all participants were attempting in some way to elicit ESP. These tests do, however, relate to unconscious motivation, which, although not explicitly mentioned by Stanford, has a close relationship with the ‘need’ component of the PMIR model. If there is an unconscious need (i.e. motivation) then this may serve to guide responses. The above studies are particularly important for the PMIR model as they seem to suggest that, not only can ESP occur, but also that people can make discriminations about the information obtained.

Experiments with animals may be considered as related to both the ‘need’ aspect and to the non-intentional psi aspect of the model. For example, Schmidt (1970) placed a cat in a cold environment, and found evidence suggesting that it could influence the output of a random number generator (RNG) when the RNG was determining how long a heat giving source remained on for. Schmidt’s results were non-significant, however. Similarly, Watkins (1971) placed lizards in a cool environment which could be heated

³ “Psi-missing” is the tendency to score significantly *below* chance on a psi task, while “psi-hitting” is significant above-chance scoring.

by means of a heat-lamp controlled by a RNG. Watkins found that the lizards showed different tendencies to receive heat from the lamp, which appeared to be related to factors such as barometric pressure and humidity. Watkins results appeared to be consistent with the differing heat needs of the lizards in the respective environments. These studies focused on an organism's ability to influence a random sequence of events, which is an aspect of Stanford's original PMIR model (1974b) that will not be expounded here. They are relevant, however, in that they seem to suggest that an organism can interact with its environment through means other than the recognised senses in order to fulfil certain biological needs.

Having established that there is some justification for considering psi as a nonintentional/unconscious process driven by needs of the organism, we will now return to the model itself, and focus on the way in which it deals with the mechanisms of producing a PMIR response.

Stanford's model goes into some very specific detail concerning how psi might interact with and influence cognitive processes. What follows is a description of the most important points as they relate to the present thesis.

How does PMIR work? - Unconscious influences.

Central to the PMIR model is the way in which the psi information is said to be processed and the way in which it can influence the organism to produce the required behaviour/cognition. PMIR is considered to be an adaptive response to implicit knowledge. At its core, the model is concerned with the consequences of priming (i.e. the influence of one stimulus on subsequent behaviours or cognitions). According to the model, psi information is apprehended, and primes various responses, which are instrumental in fulfilling the need of the organism. The first thing to consider is that PMIR works at a level below conscious awareness. This is important for a number of reasons. Firstly, it reduces the demands on the processing capacity of the organism. If every single piece of relevant PMIR information were processed, then the processing

capacity of the individual would be stretched to the point of being unable to handle anything other than psi information. Secondly, it fits in with many of the spontaneous cases. The majority of spontaneous 'psi' appears to happen without any conscious intention on the part of the percipient. This is, of course, at odds with most of the experimental procedures, which place participants in an environment and then encourage them to 'use psi'. At a more fundamental level, it is likely that working below the level of awareness is, in some cases, the *only* way in which PMIR can accomplish its goals. If the relevant information were available to consciousness it may actually serve to inhibit an appropriate response in a number of ways. For example, a person might attempt to rationalise why they feel apprehensive about a certain journey, decide they are being irrational and proceed with the journey. If the purpose of psi information was to influence the system to avoid an accident, then it would obviously be advantageous to bypass this conscious analysis. In many cases, all that is required for an effective adaptive response is a simple behaviour. Conscious cognition could possibly inhibit the required action, either through a process of rationalisation, through the time spent pondering the information, or in some other way. There may be some situations, however, where conscious cognition is entirely necessary. In these situations it is the conscious percept itself that influences the organism in the appropriate way. For example, a conscious cognition of extremely bad news may serve to prepare a person for that news, and thus lessen the emotional impact of the news when it finally arrives. Stanford (1990) states that "[f]or conscious percepts and cognitions related to the target situation to be useful (through eliciting adaptive action), special sets of circumstances have to exist....In the case of psi-mediated adaptive response to disastrous information, adaptive responses might plausibly involve a kind of emotional pre-inoculation that precedes sensory apprehension of the news" (p. 136). This is in line with many reports of spontaneous perceptual experiences, which are often associated with tragic circumstances. So, although the model is primarily concerned with non-intentional unconscious psi, it can also account for those situations where conscious perceptions/cognitions are experienced. These, according to Stanford, are special cases

in which conscious knowledge of the psi information is a better way of fulfilling the related need than the subtle influences that PMIR is usually associated with.

Probably the most important part of the PMIR model, is the way in which a response is accomplished. The whole theory hinges on the way in which Stanford proposes that PMIR works. At this point, it is probably worth reproducing the relevant assumption in full:

“PMIR is accomplished through psi-mediated facilitation, release or triggering of behaviours, feelings, images, associations, desires, or memories that are already in the repertory of the organism and that can aid in the production of an instrumental response or that can be such a response. PMIR is possible because psi information in the system in interaction with the organism’s current action plans and circumstances, primes incipient dispositions and responses that are appropriate to the psi information vis-a-vis those action plans and circumstances” (Stanford, 1990 p. 102).

What this assumption is essentially saying is that PMIR works economically. The most economic way to produce the required behaviour is through priming of behaviours that are already present in the repertory of behaviours in the organism. If someone is particularly prone to perform some kind of behaviour, then a tendency towards performing this behaviour may be primed by the psi information in order to serve the organism. This is economical in that the organism is not being influenced to do anything out of the ordinary. The behaviour will most likely be performed without much rationalisation, thus the response remains unconscious and automatic. Even if a response is considered peculiar or unreasonable, this will usually only become apparent after the fact, once a conscious analysis of the behaviour has taken place.

According to the PMIR model, there are various types of response that may be primed by psi. What follows is a brief description of some of the main ones, and, where applicable, any experimental support that exists for them.

Memory

One cognitive process that has been frequently associated with ESP is memory (see above for more detailed discussion of the memory models). Rao, Morrison and Davis (1977) have pointed out that the two phenomena are similar in that they both involve representations of objects and events which are out-with the organism's senses.

Stanford was aware of Roll's (1966) theory of psi and memory, and had noted various spontaneous cases in which it appeared that psi manifested itself as a form of memory. Stanford (1974a) suggests that an example of this type of experience may be remembering that you need to check the inflation of your car tyres. In stopping to do this, you have avoided being on a bridge further up the road, which collapsed at the time you would have been on it had you not remembered to check your tyres.

Stanford also recognised that memory was not an objective record of past events, but involved a constructive process with the participant actively involved in filling in any 'gaps' that may exist. Stanford suggested that these 'gaps', where the skeleton of the memory is filled in by the percipient, may be a vehicle for psi information to manifest itself.

In 1970, four years before the first version of the model was published, Stanford investigated the interaction between psi and memory processes. Subjects in this experiment were asked to listen to a tape recording of a girl relating a dream she had experienced (the "dream" actually being fictional). They were then administered a 'memory' test relating to the material they had listened to. This memory test was actually a disguised ESP test, as the 'correct' answers to the questions were determined randomly, and may or may not correspond to the 'dream' report. There were three types of question; those in which the answer was partially (but not completely) implied in the dream report, those in which the answer was definitely implied by the dream report and those in which the answer was not stipulated by the dream report. Stanford (1970) reasoned that the best situation for psi to manifest itself was when the random target

answer was directly contradictory to the answer stipulated in the dream report. This is due to the fact that in this situation, it is extremely unlikely that a subject will answer in a way directly opposing the story and, if they do, then something (e.g. psi) must have influenced them to do so. This is exactly what was found, suggesting that ESP (or, in this case precognition) can have an effect on ongoing memory processes. Moreover, this effect is entirely unconscious and in line with the PMIR model.

Mistakes and Associations

Mistakes and associations may also be primed. Stanford has on file a case in which a girl intending to telephone a friend dialled the wrong telephone number. Instead of her friend, she reached an elderly woman suffering a heart attack, and thus saved the woman's life by having the operator trace the call⁴. The influence of psi on associations has some experimental support. Stanford (1973) used word association as a directed free-response task. The rationale behind this approach is that word association is a free response task which is scoreable in the same manner as a forced-choice task, thus having the advantages of a free response test, without the drawbacks associated with scoring. It also has several other advantages in that it is non-intentional, and circumvents the tendency of participants to rationally pattern responses. For example, on a typical Zener card test, a participant may call a circle only because she hasn't called it for a while. Again, this may have an effect on psi performance. In a word association task, each trial is independent, and patterning of responses is not a possibility, giving psi a better chance to manifest itself without the burden of conscious rationalisation. Stanford's study employed a word association paradigm, using two lists of words. One list consisted of words which had two possible target responses according to published norms (primary response being the most frequent), while the other list consisted of homophones (words which are phonetically identical but have different meanings), which had two possible response classes (relating to a particular interpretation). Stanford hypothesised (influenced by a previous suggestion by Roll, 1966) that frequently reinforced memory

⁴ This is offered as an anecdote and is intended only as an example of the type of mistake that Stanford suggests might be PMIR related. It is entirely possible that other, non psi, factors such as chance were responsible for this particular case

traces should serve as vehicles for ESP more often than less frequently reinforced ones (associative mediation hypothesis). This hypothesis is based on the assumption that frequently reinforced memories are more likely to be remembered, and are thus 'ready to fire' if triggered by a psi input. Stanford also proposed that, in an ESP task, responses favoured by a non-ESP bias tend to be less accurate than responses not favoured by such a bias (response bias hypothesis). This suggests that if subjects tend strongly to produce primary responses then they should perform poorly on these responses. This is conceived to occur because of the increased false-alarm rate expected when responding in a way that is favoured by a bias. These two hypotheses seem contradictory but can be reconciled. The response bias hypothesis suggests that the first hypothesis (outstanding scoring for primary responses) should occur among subjects who show little tendency to produce primary responses. Thus subjects, to the degree that they do not produce a large number of primary responses, should show the effect predicted by the first hypothesis, and should be accurate in making primary responses. The actual procedure involved a "sender" attempting to influence the direction of the response on each trial. Both of the above hypotheses were confirmed, demonstrating that psi can indeed have an influence on associative processes.

In 1977, Stanford and Schroeter attempted to replicate this study. In this replication, the associative mediation hypothesis was not confirmed, though the authors state that this may have been due to the paucity of scorable secondary responses. Likewise, the response bias hypothesis was also non-significant. Worryingly, the authors cannot adequately explain why this might be.

In summary, mistakes and, in particular, associations seem to be the kind of processes that might play a part in spontaneous psi events.

Timing

The PMIR model also incorporates an unconscious timing mechanism, causing the individual to be "in the right place at the right time". Stanford and Stio (1976) conducted

a study investigating both the effect of psi on associative processes in the individual and also the possibility that an unconscious timing mechanism may be one of the ways PMIR might work. In order to test this, subjects were asked to take part in a word association task. This consisted of 10 words, one of which would be randomly assigned as the “target” word. Unknown to the subjects, the speed of their response on the target word would determine whether they were to experience a pleasurable or boring situation. For half of the subjects, if they produced their fastest response on the target word they would enter the favourable condition, and for the other half they would enter the favourable condition if they produced their slowest response on the target word. Again, the results were significant, suggesting that psi can influence the timing of ongoing associative processes.

The important thing about all these mechanisms is that they are already in the repertory of the organism, and are easily primed. Stanford (1990) gives an example from his own experience in order to illustrate this. He relates how he was driving home one evening with his wife. On approaching the appropriate turn-off, he found that he had a sudden urge to carry straight on and take another turn-off; an option that was made more appealing by the fact that the alternative route offered the opportunity to do some bird-watching (despite the fact it was late at night and they were unlikely to see much). Stanford recognised that this might be a PMIR response designed to avoid danger, so he decided to check his speculation by taking the original route, and proceeding cautiously. His thoughts were confirmed when, further up the road, he discovered an accident which had occurred a short time before his arrival, and would have been genuinely dangerous had they been in the vicinity earlier. The PMIR model would explain this by postulating that Stanford had received information about the accident through ESP at an unconscious level. This information primed a response of the kind Stanford would have made had he gained the knowledge through the usual channels. Stanford claims that “the mere knowledge of the undesirability of continuing on the planned route would have primed a very well-learned sequence of other thoughts” (Stanford, 1990, p. 103). These thoughts would have been appealing to Stanford and made him more inclined to take the

alternative route.

This anecdote is not supposed to present any evidence supporting the model. Instead, it is designed to highlight how the model *might* work in everyday life. There is nothing in the above example which can rule out any weak sensory cues.

The studies cited above tie in to the PMIR model in that they offer either experimental or conceptual support for it. It would seem that there is some evidence that psi can operate outside of awareness, in relation to needs, interacting with other cognitive processes. It would appear that the processes described above are the type that could conceivably contribute in mediating psi information. Memory and associations might be cognitive capacities that psi might influence. In the context of the PMIR model, then they are ongoing processes which might be primed (or inhibited) when doing so would contribute towards fulfilling a need in the organism. Likewise, mistakes and timing mechanisms, while probably at a higher cognitive level than memories and associations, also appear to be amenable to psi functioning as posited by Stanford in the PMIR model.

Limitations of PMIR

Although Stanford's model is comprehensive, it can be criticised on certain issues. A key aspect of any scientific model is that it is testable, and, in principle, falsifiable. One of the key strengths of the PMIR model, according to Stanford, is that it offers testable assumptions that, taken together, are thought to account for many spontaneous psi events. The assumptions set forth in the model are testable in principle, but some of them may actually be impossible to test in practice. There are a number of reasons for this. Certain of the assumptions relating to "need" strength would be extremely difficult to test without serious ethical considerations. The model suggests that the psi response will be directly related to the strength of the need. In order to test this, participants would have to be placed in situations in which it was in their biological interest to use psi to satisfy a need. Obviously such manipulations would be ethically dubious, and as such, a major weakness of the theory concerns the practical issues involved in actually

testing it. As such, making assessments concerning the strengths and weaknesses of the model is extremely difficult given these limitations. This is a major problem given that one of Stanford's primary goals was to create an empirically testable, and thus practically useful, model of spontaneous psi events.

A further problem with the PMIR model is that it might be argued that it is *too* wide ranging. It appears to be flexible in accounting for seemingly contradictory information. For example, the model is primarily concerned with unconscious psi, but it can also account for conscious psi events. This flexibility might, at our current level of knowledge, be considered a weakness rather than a strength. In being so flexible, the PMIR theory essentially narrows the amount of information that could potentially falsify the model. As it stands, just about any psi event can be subsumed within the PMIR framework. This, coupled with the practical difficulties in testing the model, may actually make the model less useful than it might otherwise be.

Finally, it is also difficult to reconcile apparently intentional psi within the PMIR model. Although the model deals with spontaneous psi events, and Stanford argues forcefully in justifying why these particular cases may be PMIR-type cases, the model cannot account for events which appear to be intentional and do not have any particular need component (at least not in the sense that Stanford sets out as PMIR relevant needs). Such intentional psi events are, however, rare, and it remains to be seen whether they actually represent "true" psi functioning in a manner that contradicts PMIR.

Summary

These, then, are various ways in which parapsychologists have conceptualised ESP to work once it is in the cognitive system. Hardy's approach may yet make an impact if it can engender research. The memory models and Irwin's information processing approach are useful in describing the possible ways in which psi may interact with and influence the cognitive system, although they may be slightly outdated. Stanford's PMIR

model is comprehensive and easily tested, incorporating many aspects of mainstream cognitive psychology.

Although the present thesis does not wholly subscribe to one particular theory, it does make the assumption that, if psi does indeed exist, then it must be processed in a similar manner to other comparable stimuli. Additionally, the PMIR model presented by Stanford offers, in the author's eyes, the most appealing model for psi processing. The model itself emphasises that psi may 'prime' certain kinds of responses in the organism. One of the main aims of this thesis, therefore, is to assess whether this is, indeed, the case. In addition to this, it would be useful to discover whether psi works in similar ways to other psychological phenomena. Thus, a further aim of the thesis is to demonstrate that psi can prime the *same* types of responses as a similar non-parapsychological phenomenon; perception without awareness.

Chapter 3 - Perception Without Awareness

General Introduction

The idea that we may be influenced by stimuli of which we are unaware has been around for a long time. Almost 300 years ago Leibniz (1704/1981) commented on the importance of recognising the importance of unconscious cognitive processes and their subsequent effects on behaviour, stating;

“[t]here are hundreds of indications leading us to conclude that at every moment there is in us an infinity of perceptions unaccompanied by awareness or reflection; that is, alterations in the soul itself, of which we are unaware because the impressions are either too minute or too numerous, or else too unvarying, so that they are not sufficiently distinctive on their own. But when they are combined with others they do nevertheless have their effect and make themselves felt, at least confusedly, within the whole (Leibniz, 1704/1981, p.53).

There is indeed a wealth of information surrounding us, and it would be impossible for us to attend to all of it. The question we are currently concerned with is whether the information we are unaware of can have any effect on our behaviours and/or cognitions.

The study of perception without awareness has become one of the most controversial areas in mainstream psychology. The basic premise states that information can be processed which is either too weak in intensity or too brief in duration to be consciously identified. This seems simple enough, but as we shall see, this issue has been the cause of huge controversy in the field of psychology. Despite this controversy (or perhaps as a

result of it), there has been an enormous amount of experimental work conducted on this question. It would be impossible to review all the relevant studies here, and the reader is referred to the following sources that adequately cover research in PWA: Dixon (1971, 1981), Bornstein and Pittman (1992), Underwood, (1996). As the field has been defined by the controversy that surrounds it, it shall be this that the current chapter focuses on primarily, citing relevant experimental work where necessary.

Early work

The first work in this area can be traced back to the late 19th Century. Pierce and Jastrow (1884) were interested in the problem of the “just-noticeable-difference” in psychophysics. These authors wanted to know what would happen if the difference between stimuli were reduced *below* what Weber (1834) and Fechner (1860/1978) had previously characterised as the physiological threshold. Acting as their own subjects, Pierce and Jastrow devised a test which forced them to choose which of two pressures was the heavier, and to assign a confidence level to their judgements. This task was extremely difficult, and the authors noted that sometimes the difference was so minute “that it seemed nonsensical to answer at all” (p. 77). When the data were studied, Pierce and Jastrow discovered that they were accurate more often than chance would allow, even though they were not conscious of any difference between the stimuli (this being measured by their confidence estimation). This study and the ones which followed it led Pierce and Jastrow to attack the psychophysical concept of threshold, defined as “a physiological limit beyond which there would be no registry whatever” (Jastrow, 1930, p.136). Instead, the authors argued for subconscious registration of stimulus differences. Importantly, they realised the possible implications of this hypothesis, stating; “The general fact has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another’s minds in large measure from sensations so faint that we are not fairly aware of having them, and can give no account of how we reach our conclusions about such matters. The insight of females as well as certain ‘telepathic’ phenomena may be explained in this way.” (p. 83)

Fourteen years later, Sidis (1898) began his investigations into the possibility of obtaining information about a stimulus outside of awareness. Sidis showed subjects cards containing a single printed digit or letter, but placed them at such a distance that the character shown was so far away that it appeared only as a “dim, blurred spot or dot” (p. 170). Indeed, Sidis reported that many subjects complained that they could see nothing at all. However, when Sidis asked the subjects to name the character on the card, their responses were correct considerably more than would be expected by chance, despite the fact that many subjects believed that they “might as well shut their eyes and guess” (p. 171). Studies such as this led Sidis to conclude that there was within us “a secondary subwaking self that perceives things which the primary waking self is unable to get at” (p. 171).

These two studies at the end of the 1800s can be seen as the start of research into perception without awareness. They influenced several follow up studies (e.g. Stroh, Shaw and Washburn, 1908), the findings of which were so robust that, some years later, Adams (1957) suggested using the technique as a classroom demonstration. So, these early experiments appeared to provide reliable evidence that participants can make accurate perceptual discriminations even when they believe that they cannot consciously identify the stimuli. Various other researchers subsequently became interested in these effects. One of the most notable was Poetzl (1917/1960). Poetzl noticed that some of his neurologically impaired patients exhibited paradoxical enhancement of visual sensitivity. One of the most intriguing phenomena was what he called “delayed piecemeal delivery” into consciousness. The patient, fixating on a stimulus would initially see very little (due to loss of central vision). Soon after fixation, however, coherent fragments would emerge in consciousness, often combined with ongoing percepts. Poetzl suggested that normal people have certain inhibitory mechanisms. These serve to inhibit certain processes that would otherwise lead to a massive amount of perceptual information. For example, in principle all of us can perceive our ‘blind

spot' but the inhibitory mechanisms prevent us from doing so. Poetzl reasoned that in his patients, these inhibitory mechanisms had been destroyed, leading to an increased sensitivity to such percepts. The implication of this is that much more of the input is registered and processed than is usually accessible to consciousness.

Poetzl attempted to demonstrate similar effects in neurologically normal subjects. To do so, he decided that dreams were the best state in which to produce such effects. Poetzl presented his subjects with a complex pictorial stimulus for a brief period of 10ms. The subject was then asked to reproduce the stimulus in the form of a drawing. This was something that the subjects could rarely do. Finally, subjects were instructed to have a dream that night, and give a detailed verbal and pictorial report of the dream. Poetzl's finding was that stimulus elements which were initially inaccessible emerged in the content of the subject's dream.

The dissociation paradigm

These early studies (and the majority of subsequent work) relied on what was to become known as the "dissociation paradigm". Fundamentally, the logic which lies behind the majority of experiments into PWA concerns demonstrating a dissociation between two response measures (see Erdelyi, 1986). The dissociation is usually between a measure of conscious awareness (a "direct" measure of perception) and some kind of index measuring unconscious perception (an "indirect" measure). The study by Sidis (1898) is a classic example. Sidis' participants reported that they could not make a distinction relating to the identity of the letters presented to them (i.e. no conscious awareness on a direct measure of perception). However, when they were asked to guess the character (an indirect measure of perception), they performed at above chance levels. Thus, a dissociation between the direct measure (discrimination) and the indirect measure (guessing score) suggested that, when the conscious measure was null (i.e. when participants claimed they could not make a discrimination), the unconscious measure (above chance guessing) nevertheless suggested that some form of perception had

occurred¹. If a dissociation between the two measures used can be found, then it suggests that a source of information unavailable to consciousness has an effect on thought and behaviour. Although the logic underlying the dissociation paradigm is straightforward, experimental work implementing it has been widely criticised. Obviously, the crucial factor in any study employing the dissociation paradigm is whether or not the index of conscious awareness is adequate. As Merikle (1992) points out, depending on one's answer to this question, the evidence for subliminal perception is either overwhelming or non-existent. Whatever method of measuring conscious awareness is used, it must satisfy strict criterion. Firstly, it must be shown to exhibit null sensitivity to the stimuli. If it cannot be shown that the measure shows null sensitivity, then any dissociation observed cannot be assumed to be evidence of PWA. Secondly, the method used must be an exhaustive measure of conscious awareness. If a measure is used that satisfies the first criteria, but may have failed to measure some crucial aspect of conscious awareness, then, once again, any dissociation observed may not strictly be posited as evidence of PWA (see e.g. Reingold and Merikle, 1988 and the example from Erikson, 1960 below).

These are the core issues in the field of PWA. What now follows is a review of what researchers and commentators have had to say concerning these issues, starting with two of the first major critiques of the field from Israel Goldiamond and Charles Eriksen.

First criticisms

Psychology in the 1950s was dominated by behaviourism, and as such, the idea that people might be influenced by stimuli of which they were not consciously aware was not a popular one. It was in this climate that the first major criticisms of the PWA field began to appear, with Goldiamond (1958) being among the first dissenters. He criticised the dissociation paradigm due to the possibility that any observed effects might actually

¹ The distinction is often conceptualised as being between a “subjective” measure (e.g. I can't see the stimulus) and an “objective” measure (e.g. above chance performance on a discrimination task)

be representative of systematic differences between the two tasks used to measure conscious and unconscious perception, rather than actually representing a dissociation between conscious and unconscious perception. Goldiamond was particularly concerned with the use of the psychophysical techniques used to define “thresholds” in PWA experiments. These techniques usually consist of presenting participants with a number of stimuli at different levels, and obtaining some form of response pertaining to the discriminability of the stimulus. When this measure drops below (or rises above) a certain pre-defined value (say, e.g. chance performance or an arbitrarily set level) then this is said to represent the threshold of awareness. Goldiamond criticised researchers using these techniques for not controlling for “false negatives”. False negatives are trials in which participants report they haven’t experienced a stimulus when they actually have. Goldiamond states that the existence of false negatives in threshold setting procedures may account for any observed PWA effect, given that they might result in the threshold being set at the wrong level (i.e. a level that allows conscious awareness – See Goldiamond, 1958 for a comprehensive critique of psychophysical methods used in PWA research).

Related to this issue was Eriksen’s (1959) suggestion that placing the responsibility for establishing awareness on the participant may lead to biases that in turn lead to artefacts. Eriksen claimed that there might be various factors influencing responses that are not related to awareness. These might include demand characteristics and preconceived biases that may cause subjects to adopt conservative response criteria. If such factors were indeed influencing the participant, then they may report null awareness under conditions in which conscious information is actually available. To illustrate this, Eriksen offers the following (exaggerated) example:

“A psychophysicist in setting about to determine the absolute threshold for light would not use an experimental situation where the subject was to press a button directly in front of him to indicate that he did not see the light and to get up and walk across the room to press a button on the wall if he did. Few would expect such an arrangement would yield as low an absolute threshold as would be obtained if the subject had the two buttons directly in front of him” (Eriksen, 1959, p. 205).

Relying on the individual, it would seem, is not the most valid way of measuring awareness.

Eriksen’s (1960) critique closely followed, and it was this appraisal of PWA research that was to set the tone for the controversy that would dominate the field for the next forty years. Eriksen objected to the use of verbal reports as a measure of conscious awareness in the early PWA studies. He points out that, by asking participants to state whether they were aware of the presented stimulus places a “heavy burden upon the adequacy of language to reflect the richness of perceptual experiences and images” (Eriksen, 1960, p. 280). Eriksen offers a good example of this. He states that a person’s verbal description of another individual might be inadequate in order for a third person to recognise the described individual in a crowd. However, if the describer possessed artistic talent, then he could conceivably draw a sketch of the person that would greatly increase the chances of subsequent recognition. Thus, it would make no sense to state that, because the verbal description was insufficient, there existed no awareness of the relevant facial features. In this sense, then, verbal reports are not an adequate representation of conscious experience.

Like Goldiamond (1958), Eriksen was also highly critical of the various ways in which researchers set perception “thresholds”. Usual practice in these types of experiments is that the thresholds would be measured in terms of the verbal report to the stimuli, and some form of discriminative response. If it could be shown that the latter threshold is lower than the former (i.e. discriminative responses could be made in the absence of verbal reports) then it could be construed as an example of perception without awareness. However, Eriksen strongly objected to the fact that the thresholds were set in terms of verbal reports and not, as he would have preferred, in terms of the discriminative response. Eriksen’s distrust of the verbal response as a means of indexing awareness led him to suggest that the most reliable means of establishing whether a stimulus is consciously perceived or not is the discriminative response itself. This creates a rather unfortunate (and seemingly insurmountable) paradox, because above chance performance on a discriminative response test is the very means by which perception without awareness is defined. Thus Eriksen succeeded in defining perception without awareness out of existence.

This could have conceivably sounded the death knell for the field, given that the criticism appeared to rule out the possibility of ever demonstrating perception without awareness (using the dissociation paradigm at least). However, interest in PWA continued largely due to the efforts of Norman Dixon (1971, 1981). Dixon argues strongly for the reality of PWA, but he offers relatively little in response to the major criticisms of the field, instead advocating the use of phenomenal reports as measures of awareness that Eriksen was so opposed to. Around the same time, various researchers from the psychoanalytic tradition became interested in PWA (see, e.g. Shevrin, 1992, for a review). However, despite having relatively stringent methodologies, these researchers did not contribute to the “big” question of PWA, i.e. the search for an adequate measure of awareness. In fact, it would appear that the general hostile position of experimental psychologists towards most things psychoanalytic ensured that this brief flurry of activity went unnoticed in the field.

Addressing the criticisms

The early 1980s saw some new techniques in the quest to demonstrate PWA. Studies by Marcel (1983a, 1983b) apparently addressed many of Eriksen's reservations. Marcel used a semantic priming paradigm. In semantic priming experiments, participants' response to a stimulus word is typically enhanced when they have previously viewed a semantically related priming word. Marcel used lexical decisions (i.e. decisions about whether a target stimulus is a word or a non-word) as a response measure, but the key aspect of his work was that he masked the prime in such a way that it became extremely difficult to detect consciously. Marcel reported that, when the prime and the target word were semantically related, priming (as measured by decreased response latencies) was observed on the lexical decision task relating to the target words. Moreover, this effect occurred when participants were apparently unable to discriminate between the presence or absence of the prime (thus satisfying Eriksen's criterion for conscious awareness).

Kunst-Wilson and Zajonc (1980) also reported positive influence of stimuli in the absence of stimulus detection. They used a "mere exposure" paradigm. The idea behind the mere exposure effect is that merely being exposed to a stimulus will increase subsequent liking for it. Kunst-Wilson and Zajonc (1980) presented participants with 10 meaningless geometric shapes. Each shape was presented 5 times for 1ms each. They were then given a forced-choice discrimination test in which they had to indicate which of two stimuli they had seen before. Participants were then given an identical task, but asked to indicate which shape they preferred. The result was that subjects could not perform better than chance when asked to indicate which shapes had been presented before, but demonstrated preference for the stimuli that they had previously been exposed to for extremely brief durations.

These studies and the replications that followed them (e.g. Balota, 1983; Fowler *et al.*, 1981) appeared to be the breakthrough that researchers had been looking for, and, in the words of Merikle and Reingold (1992) "[they] convinced many former skeptics that

perception without awareness was indeed a valid phenomenon” (p. 60). It was probably inevitable, however, that the methods employed would come under close scrutiny, and various commentators (e.g. Cheesman and Merikle, 1985; Holender, 1986) took issue with the measure of stimulus detection. Once again, the criticisms relating to using the dissociation paradigm became pertinent, and the question of whether the measure of conscious awareness actually indicated “true” null sensitivity placed the results in doubt.

While the present-absent discrimination tasks appear in principle to be a bias-free measure of conscious awareness, the critics claimed that the implementation of them in the above studies meant that they were probably inadequate to establish discrimination at chance levels. Holender (1986) criticised the use of different illumination patterns in the threshold-setting procedure, suggesting that this could have led to the selection of thresholds that allowed for conscious awareness. Cheesman and Merikle (1985) pointed out that the threshold-setting technique may have located “subjective” rather than “objective” thresholds (see below for a discussion on the difference between these two). Indeed, the critics claimed that the studies by Marcel (1983a, 1983b) and Kunst-Wilson and Zajonc (1980) were probably better conceptualised as being demonstrations of perception in the absence of subjective confidence rather than perception without awareness.

Holender's critique

In 1986, Holender published his review of work relating to unconscious perception. Holender's critique was damning, and the field was once again thrown into disarray by the apparent inability of researchers to conduct studies that would adequately appease sceptics. Much of Holender's criticisms are not too dissimilar to those of Eriksen (1960). He basically dismisses most of the evidence for PWA as likely being the result of conscious (or at least part-conscious) identification of the PWA stimuli. Indeed, Holender reiterates Eriksen's criterion for demonstrating PWA as being chance performance on discriminative response tasks. According to Holender, above chance

performance on these tasks represents conscious awareness of the stimuli. However, above chance performance on discriminative response tasks are also the way in which researchers have defined whether a PWA stimulus has been unconsciously perceived. Thus, Holender creates the same paradox Eriksen had thirty-six years previously, essentially ruling out PWA by definition.

Research at this point essentially split into two. Some researchers (e.g. Greenwald, Klinger and Liu, 1989) continued to look for ways of demonstrating PWA according to the criteria laid down by Eriksen (1960) and Holender (1986). Others (e.g. Cheesman and Merikle, 1986), while acknowledging the drawbacks of the dissociation paradigm, started looking for other methods for revealing PWA.

Greenwald's approaches

Greenwald *et al* (1989) used similar technique to Marcel (1983) in that they attempted to semantically prime lexical decisions. However, rather than the traditional task of deciding whether a stimulus was a word or a non-word, Greenwald *et al* asked participants whether a presented word was evaluatively positive or negative in meaning. Target words were preceded by masked primes, which themselves were either positive, negative or neutral. The masking technique used is known as dichoptic masking, and involves presenting the mask and the prime to different eyes. In determining conscious awareness of the stimuli, Greenwald *et al* adopted a stricter measure than merely asking participants to report the presence or absence of the prime. Instead, they measured ability to report whether the prime appeared to the left or to the right of the fixation point. This adheres to Eriksen's and Holender's criterion for measuring awareness, in that the participants were unable to make a discriminative response based on the position of the prime. Greenwald *et al* (1989) report that, despite participants being unable to detect where the prime was presented, there still appeared to be an influence of the priming word when it evaluatively matched the target. Further research on this effect (Greenwald 1992) revealed an interesting finding in that the effect did not hold up when

the primes were two word phrases rather than single words. As an example, the phrase “enemy loses”, which is positive in evaluative meaning despite consisting of two negative words, appeared to prime negative rather than positive targets.

Greenwald and his associates (e.g. Greenwald, Klinger and Schuh, 1995; Draine and Greenwald, 1998a) continued their quest to demonstrate PWA under conditions that satisfy the criteria demanded by Eriksen (1960) and Holender (1986). Their most recent development has been a statistical regression technique. They recognise the problems inherent in attempting to demonstrate the null sensitivity of conscious awareness (i.e. proving the null hypothesis). To counter this, Greenwald, Klinger and Schuh(1995) introduced a statistical technique in which the indirect measure (i.e. the measure of the unconscious influence) is treated as criterion, and examined as a linear-regression function of the direct measure (i.e. the measure of conscious awareness). This technique allows for a measure of association between the direct and indirect measures. If the direct and indirect measures are evaluated on scales that have rational zero points, then the technique yields an intercept estimate that can be used to test the hypothesis that perception has occurred on the indirect test, but not on the direct test (based on the intercept being greater than zero). Thus, the technique uses a method of null hypothesis rejection (i.e. that the regression intercept is greater than zero) rather than the traditional null hypothesis acceptance procedure that traditionally is associated with the dissociation paradigm. This statistical technique is quite complex (see Greenwald *et al*, 1995 for a full explanation), though Greenwald has reported considerable success in demonstrating that semantic activation can occur in the apparent absence of conscious awareness. Their technique, however, has been criticised by Merikle and Reingold (1998) due to the possibility that the direct and indirect measures used were not sufficiently comparable. The regression technique has also been criticised by Doshier (1998; though see Greenwald and Draine, 1998b for a reply to both criticisms) and Miller (2000). The attempts of Greenwald to address the critical issues in PWA research have been innovative, although they have been criticised. The debate concerning the

regression technique is ongoing, and it remains to be seen whether Greenwald can refine it in a way that satisfies his critics.

Greenwald, however, was not alone in attempting to find a way to measure PWA that obviated the traditional criticisms. Other approaches, which are arguably more successful, are outlined below.

Qualitative Differences

Another approach to the PWA problem can be attributed to Cheesman and Merikle (1986). They essentially rejected Holender's criterion and went on to propose a distinction between "subjective" and "objective" thresholds. A subjective threshold is defined as "the level of discriminative responding at which observers claim not to be able to detect or recognize perceptual information at a better than chance level" (Cheesman and Merikle, 1986, p. 344). An objective threshold is defined as "the level of discriminative responding corresponding to chance level performance" (Cheesman and Merikle, 1986, p. 344). According to Cheesman and Merikle, PWA occurs at levels below subjective awareness and above objective awareness. It makes intuitive sense to conceptualise thresholds in this way. Not many people would argue with the existence of an objective threshold, below which stimuli are either too weak or too degraded to have any kind of influence. The question then becomes whether this is in any sense separate from the point at which people claim unawareness. This, of course, is directly opposed to Holender's (1986) and Eriksen's (1960) criteria. These authors would argue that the objective threshold is the only useful boundary, and anything above chance responding on a discrimination task represents conscious awareness. But this criterion appears to rule out PWA by fiat, and Cheesman and Merikle were keen to demonstrate that the phenomenon was a real one. Their first objective was to demonstrate that the subjective/objective distinction was useful, and that stimuli presented at a level between the two could have a meaningful influence on the cognitive system. Which, of course, brings us back to the question concerning finding adequate measures of conscious awareness and all the problems that go along with that. Cheesman and Merikle (1986)

were aware that a mere change in definitions does not solve the problems associated with distinguishing conscious from unconscious processes. Moreover, they were aware that a “subjective” measure of awareness could be criticised in much the same way Eriksen criticised the verbal reports used as a measure of awareness in the early PWA studies. Thus, Cheesman and Merikle (1986) propose an additional criterion, emphasising the importance in demonstrating qualitative differences between conscious and unconscious processes. Much of the previous work in PWA was criticised by Cheesman and Merikle due to the fact that they, at best, merely demonstrate that a certain class of stimulus can be perceived both above and below a particular awareness threshold. By demonstrating that conscious and unconscious processes can have qualitatively different effects, then the distinction between them becomes more concrete. Additionally, by placing the two in opposition to each other, it might be possible to finally move away from the dissociation paradigm.

One aspect of unconscious processes is that they are thought to lead to automatic responses (see, e.g. Hasher and Zacks, 1979, 1984). This is in direct contrast to conscious processes, which allow an individual to act upon the world (Searle, 1992). When a stimulus is perceived outside of awareness, then it is not available to the individual to guide actions. Any effect that it may have, therefore, must be automatic in the sense that it is not under the conscious control of the individual and consumes few (or no) attentional resources. This has been the basis for some of the most convincing demonstrations of PWA. If a single task could be found that was differentially affected depending upon whether a stimulus was perceived with or without awareness, then this would obviate the need for two separate tasks measuring conscious awareness and unconscious influence respectively. This has an obvious advantage in that a demonstration of null sensitivity in a measure of conscious awareness is no longer required. Instead, testable predictions are made concerning the responses on a single task, when conscious and unconscious processes are placed in opposition to each other.

Merikle and his colleagues used this line of reasoning in a series of experiments based on a variant of the classic Stroop effect² (Stroop, 1935). This version of the Stroop task uses two colour words (e.g. RED or GREEN) which are used to prime responses to two target colours (also red or green). However, in this variation of the task the typical Stroop effect is reversed whenever incongruent (e.g. GREEN printed in red ink) prime target pairings occur with greater frequency than congruent (RED printed in red ink) target pairings (see, e.g. Cheesman and Merikle, 1986; Merikle and Joordens, 1997). So, while the traditional Stroop effect shows that reaction times on incongruent trials are slower than they are on congruent trials, when the incongruent trials are presented more often in a session, this effect is reversed. A possible explanation for this effect is that participants are utilising a predictive strategy based on the frequency of occurrence (see, e.g. Merikle, Joordens and Stolz, 1995). In other words, when incongruent trials occur more than congruent trials, then the intelligent strategy would be to expect that the target colour on each trial will be the colour *not* named by the prime. Implementing this strategy would facilitate performance on the incongruent trials, while slowing performance on the congruent trials. However, Merikle and his colleagues (e.g. Merikle and Cheesman, 1987; Merikle and Joordens, 1997; Merikle *et al* 1995) have demonstrated that this predictive strategy is adopted only when the primes are perceived with conscious awareness. When the primes are presented outside of conscious awareness, the typical Stroop effect was observed (i.e. faster responding on congruent than incongruent trials). This represents a qualitative difference in performance on a single task, depending on whether the prime is perceived with or without awareness. The reversal of the effect suggests that there is something fundamentally different about the two modes of presentation and the subsequent effects they have. The fact that the traditional Stroop effect was found when the primes were presented below the level of conscious awareness gets around the criticism that the difference in performance was based on the “unconscious” condition being at mere chance. This was not the case.

² Whereby the time taken to name the colour a word is printed in is significantly affected if the word itself represents an incongruent colour (e.g. BLUE printed in green ink).

There was an effect here that would not have been observed had participants been performing at chance levels. This looks like a demonstration of PWA. It was at this point, in the late 80s and early 90s, that research in PWA can be said to have come of age. Researchers finally began to move away from the dissociation paradigm, and the seemingly hopeless quest to define and measure 'awareness'. As Merikle (1992) pointed out, there will probably never be a measure of conscious awareness that is universally accepted. Instead, researchers began looking for alternative ways to demonstrate that PWA existed.

Process Dissociation Approach

A major advance was made when Jacoby and his colleagues (e.g. Jacoby *et al* 1993) proposed what they called the "process-dissociation" approach (henceforth PDA). This approach begins with the rejection of the assumption that any single process (whether conscious or unconscious) is "pure" in the sense that it is not affected by anything else. This was a major underlying assumption of the dissociation paradigm, which attempted to get a "pure" (i.e. absolute) measure of perception without awareness, when a measure of conscious perception (again assumed to be "pure") showed null awareness. The PDA holds that most tasks are probably sensitive to both conscious and unconscious influences, and that these joint contributions are the rule rather than the exception. This position has appeal as it seems to reflect how unconscious processes might work in real life, where any unconscious effects are unlikely to occur in isolation.

The PDA is constituted of two conditions which, when considered together, give a measure of the relative contributions of conscious and unconscious processes to a task. The first condition is known as the "exclusion" condition. In exclusion tasks, participants are asked to actively avoid using a particular stimulus when responding. This is assumed to be under conscious control. However, at a certain 'critical' level (for example if the stimuli they are asked to avoid have been degraded) then they might stop being able to exclude the stimulus, and actually include it as a response. This is measured in relation to a baseline condition. If it can be shown that, relative to the

baseline, participants are using the stimulus they have been asked to avoid more often, then it can be said that the stimulus is influencing them at an unconscious level (and thus automatically influencing their response). For example, Debnar and Jacoby (1994) presented participants with a masked five-letter target word (e.g. spice). Immediately after the presentation of each target word, a three-letter word stem (e.g. spi__) was presented, and the participants were asked to complete the stem with any word they could think of *except* the target word that had been presented immediately before. Debnar and Jacoby (1994) reported that, when the initial masked words were presented for a period that permitted conscious identification, participants could perform this task efficiently. However, when the masked words were presented for a duration that was supposed to be outside of awareness, participants were unable to exclude the previously presented target word, and actually used it as a response (called “exclusion failure”). This was significantly different from a baseline condition in which no target word was presented before the test word (to determine how many times the participants would use the target word by chance). An assumption behind this task is that the ability to exclude the target word when completing a word stem is under conscious control. Participants have to have consciously seen the target word, and then used this information to actively avoid using it in their response. Exclusion failure, therefore, reflects a situation in which there is either no conscious influence guiding the response at all, or the conscious influence is extremely weak, and does not oppose the unconscious influence. As the process that leads to exclusion failure is, for the most part, unconscious in nature, the participant cannot perform the required conscious tasks that would result in the accurate exclusion of the target word. Instead, the target word is included (as it has been unconsciously “primed” and to exclude it would require conscious knowledge of it). This “priming” effect is a result of the automatic processing associated with unconscious influences. One of the major advantages of this exclusion task is that it relies on a positive deviation from baseline performance. This is in contrast to the previous methods conducted under the dissociation paradigm, which required the demonstration of a “null” effect to show that conscious awareness was absent. Thus, the exclusion task does not

require that researchers “prove the null hypothesis”. It is also an example of a qualitative difference that Cheesman and Merikle (1986) deemed so important for demonstrations of PWA.

The second condition in the PDA is the “inclusion” condition. As its name suggests, this is the opposite of the exclusion condition, and requires participants to include the stimuli presented to them as much as possible in their responses. Again, baseline conditions are used (i.e. stems which do not correspond to any of the target words). If the baseline rates are similar on the inclusion and exclusion tasks, then the amount of completions using the target word in the two tasks can be compared. This will allow an estimation of the proportion of trials in which conscious factors were influencing performance. So, in the inclusion condition, the proportion of trials completed with target words will be a result of both consciously controlled trials and/or unconsciously influenced trials. This is because, in the inclusion task, participants are being asked to include as many of the target words as they can. So, inclusion of a target word can either be due to the conscious ability of the participant to follow the instructions, or it might be due to the target being “primed” and automatically coming to mind. The inclusion task has no way of distinguishing between these. However, in the exclusion task, the proportion of trials completed with the target word solely reflects the unconscious influence of the target word, “priming” (that is, bringing automatically to mind) the target word. When this happens, the participant cannot help but use the target word as a response due to the automaticity associated with unconscious processes. It is also important to note that, in the exclusion condition, if a trial is influenced by both conscious and unconscious factors, then the conscious factors are assumed to over-ride the unconscious influence. This is due to the conscious factors allowing the participant to actively follow the instructions.

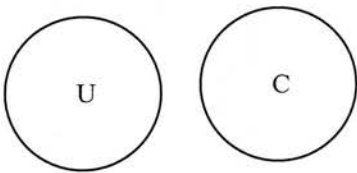
What is important at this stage is to obtain an estimate of the proportion of consciously controlled trials that *also* involved unconscious influence (“overlap” trials). Without

knowing the proportion of trials on which both conscious and unconscious influences are present, there is no way to determine the proportion of trials on which *only* an unconscious influence is present. How one obtains this estimate, however, depends on the way in which conscious and unconscious processes are thought to relate to each other. Whatever model is adopted will determine how much “overlap” between unconscious and conscious processes there is likely to be, subsequently allowing for an estimate to be made concerning the magnitude of the unconscious influence. What model to adopt has been the subject of one of the more recent debates within the field, and it is to this issue we now turn.

PDA - The relation between conscious and unconscious processes

There are three ways in which it is possible to conceptualise the relationship between conscious and unconscious processes. The first view holds that they are mutually exclusive of each other (fig. 3.1). In this model, one is either conscious or unconscious of a type of influence. This is the view that lies behind the dissociation paradigm, and the majority of early work on PWA. As we have seen, it is not very useful due to the difficulties involved in measuring the respective processes. For example, it is probably impossible to convincingly demonstrate that unconscious factors have influenced a particular task while conscious influences have been completely absent. As such, this view is not widely held, despite the intuitive appeal it may hold.

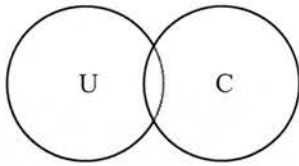
Fig. 3.1 – Exclusivity



A second model of the relationship between conscious and unconscious processes assumes that they act completely independent of each other (fig. 3.2). The

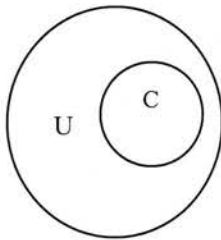
“independence” model holds that conscious processes can (but need not always) occur without unconscious processes and vice versa.

Fig. 3.2 - Independence



The final model is known as the “redundancy” model (fig. 3.3). This view suggests that consciousness is the “tip of the iceberg” and that whenever there is a conscious influence present, there is also a related unconscious influence.

Fig 3.3. – Redundancy



As previously noted, which of the above models is adopted will subsequently determine how much overlap is hypothesised to exist between conscious and unconscious processes and therefore will determine the estimation of unconscious processes in the process dissociation model (obviously the exclusivity model cannot be applied to the PDA, as one of the fundamental assumptions of the PDA states that the processes cannot be independent). The mathematical formulations for calculating the effect of unconscious influences will not be expounded here (see Joordens and Merikle, 1993; Jacoby, Yonelinas and Jennings, 1997 for a fuller account of the calculations involved).

Joordens and Merikle (1993) compared the estimations of conscious and unconscious influences as predicted by each model on the same data set, and suggest that both independence and redundancy models appear to lead to plausible estimations.

Jacoby *et al* (1997), on the other hand, argue strongly for the adoption of the independence model. As justification for this position, they cite (amongst other things) several studies in which certain experimental manipulations have resulted in the magnitude of unconscious influences being found to be stable across conditions, while the magnitude of conscious influence has varied. This is to be expected under an independence model, where the two are distinct from each other.

There has been a great deal of controversy concerning the adoption of the independence model (see, e.g. Buchner, Erdfelder and Vaterrodt-Plunneke, 1995; Curran and Hintzman, 1995; Joordens and Merikle, 1993; Jacoby, Toth, Yonelinas and Debner, 1994; Joordens and Li, 2000).

Rather than go into extended details about this debate, I will briefly outline a recent discussion on the issue by Joordens and Li (2000). They suggest that there is something circular about generating estimates based on an assumption, and then using these estimates when testing the assumption that generated them. Joordens and Li claim that, if the independence model is wrong (i.e. the estimate of unconscious influence is invalid), then the estimate of unconscious influence will contain a great deal of error (the amount of error relating to the degree that the model is wrong). If the measurement of unconscious influence as based on the independence model contains a lot of random error, then it would be difficult to find variables that systematically influence the estimate of unconscious influence (as it measures more noise than signal). Thus, the evidence put forward by Jacoby *et al* (1997) as being supportive of the independence model, might be a result of the model being inaccurate. One further problem noted by Joordens and Li (2000) is that the PDA was originally proposed due to the general agreement that finding “pure” measures of conscious or unconscious influence was probably impossible. However, much of the evidence supporting the independence model relies on demonstrating that a particular manipulation purely affects conscious influences and not unconscious influences.

Joordens and Li (2000) offer further speculations that the independence model is wrong, based on the observation that certain predictions made by the model are not in line with what is found in various empirical studies. These violations of the independence assumption are extremely damaging, and seem to suggest that the model is wrong. Jacoby, (1998) has responded to this by claiming that there are boundary conditions that should be associated to the independence model. However, the implication by Jacoby (1998) is that the independence assumption is wrong only when these paradoxical effects are observed, and correct at all other times. This is not a good argument. Joordens and Li (2000) offer some convincing data that are in line with the redundancy model.

The process-dissociation approach and the assumptions that underlie it have become the latest controversies in the field of PWA and it looks like the debate will continue for some time. Merikle, Joordens and Stolz (1995), however, offer some refreshing advice. Given that the assumptions associated with the PDA are so problematic, an alternative might be to use the exclusion task in isolation to measure the magnitude of unconscious influence on a task. This would require no assumption regarding the relationship between conscious and unconscious processes. All that would need to be assumed is that participants will follow the task instructions and avoid using a particular stimulus when responding. When participants fail to do this, and actually include the target more often than chance, then it can be concluded that unconscious influences are dominant. When they successfully exclude the target word, then conscious influences are dominant. Although this procedure doesn't lead to estimations of the contributions of conscious and unconscious factors in a task, it does give a measure of the relative magnitude of each. Thus, in situations where a researcher wants to determine whether conscious or unconscious influences are dominant in certain conditions, but is not interested in estimating the magnitude of the influences, then using the exclusion task in isolation is perfectly sufficient. This makes intuitive sense, and it is the author's opinion that this approach is satisfactory whenever it is necessary to obtain an idea of whether conscious

or unconscious processes are dominant in a particular task. The issue of what contribution is made by each is one which, although potentially useful in a theoretical sense, does not seem to be too important practically.

Although most of the work in PWA is conducted in the visual domain, there have been many attempts to demonstrate similar effects using auditory stimuli, which poses its own unique set of problems. Experiment 1b of the current thesis utilises these techniques, therefore there will now be a brief review of this area of PWA research.

Auditory PWA

If the study of perception without awareness has been controversial, then the study of *auditory* PWA is its most controversial component. All of the problems related to the field in general also apply to the auditory domain, but presenting acoustic material as stimuli has its own set of unique problems. Many PWA researchers have avoided becoming involved in this type of research for a variety of reasons. There are certain stigmas attached to the study of audio PWA, and these tend to scare potential researchers away from working in this sub-field, preferring to pursue their research in the visual domain. One of the reasons for this is the popular conception of “subliminal auditory messages”. Much of what the general public knows about auditory PWA (and PWA in general) stems from the sensational claims that are often made in the media concerning the effect of subliminal messages. These purported messages can be benign in nature, such as the therapeutic claims made concerning commercially available “self-help” subliminal cassettes (see, e.g. Greenwald, Spangenberg, Pratkanis and Eskenazi, 1991). Or they can be more sinister, such as the claims relating to the alleged subliminal messages inserted by musicians into songs as a means of influencing listeners³. The sensational claims made on behalf of auditory PWA have tended to make the whole area disreputable and this has meant that very little research has been conducted into it. In

³ Such as the case brought against the band Judas Priest in which they were accused of embedding subliminal messages in their music, directly resulting in the suicide of one man and the attempted suicide of another (Vance vs Judas Priest, 1989; cited in Urban, 1992).

addition to this, there are also the technical complexities involved in this type of research that have possibly discouraged potential researchers.

The questions concerning the efficacy of self-help tapes etc. are not directly relevant to the present discussion, so these will not be considered further (though see Greenwald, *et al*, 1991 and Urban, 1992 for coverage of these issues). What we are currently concerned with is the practicalities involved in conducting this type of research.

Traditionally, the most popular method of auditory subliminal presentation has been the “auditory threshold” technique. This involves merely reducing the sound amplitude of a stimulus until participants cannot identify its presence or absence at better than chance levels. This method, of course, is open to the same criticisms concerning the definition and measurement of awareness that similar visual techniques are (see above). Another popular method has been to “mask” the stimulus with white noise (or sometimes music) in such a way that the mask essentially drowns out the stimulus, rendering it unavailable to conscious perception. Although this technique has been widely employed in the studies that have investigated auditory PWA (e.g. Borgeat and Goulet, 1983), it is a technically complex procedure to achieve satisfactorily. The most important aspect of any auditory masking procedure is the “signal to noise ratio” (S/N ratio). This is simply the magnitude of the masking noise in relation to the stimulus. For example, a S/N ratio of -30 decibels (db) means that the masking noise is 30db above the stimulus. While this may seem simple enough, there are a plethora of problems in achieving this. The first concerns the variability of the stimulus. If the stimulus is a spoken word, then it will inevitably vary in loudness at various points. No spoken word is uniformly loud. This has consequences for masking. As a hypothetical example, say that an S/N ratio of -30 db has been found to be the optimum level at which auditory PWA occurs. That is, whenever a stimulus is masked by noise at a level of -30 db, then it cannot be consciously perceived, but can still influence the individual. If this was the case, then it would be of paramount importance to make sure that the S/N ratio was constant at all

points of the stimulus. If the word was quiet at the start, got louder in the middle, then was quiet at the end, it would be vital to ensure that the S/N ratio was always -30db . Thus, the mask must in some sense mirror the properties of the word in order to maintain a constant S/N ratio. If the mask was kept constant, then it possible that certain aspects of the word might rise above the -30db ratio, and thus become consciously perceivable. Likewise, if the word had a quiet component, then it could possibly drop below the optimum S/N ratio, leading to it falling below the “objective” threshold, and not being perceived at all. However, if the white noise is made to track the properties of the stimulus, then this itself could be a cue as to what the stimulus is. In addition to these issues, the masking must be conducted according to psychoacoustical considerations (see Ott and Curio, 1999 for a discussion).

Urban (1993) stated the requirement that methods of presenting auditory PWA stimuli be standardised and presented a design for equipment intended for use in creating audio PWA stimuli. Ott and Curio (1999) went one step further and actually developed software for the creation of stimuli following the suggestions of Urban. Ott and Curio, however, used specialised software that they subsequently modified, making replication of their procedure extremely difficult. Unfortunately, Urban (1992, 1993) and Ott and Curio (1999) have been the only recent attempts to address the problem of auditory PWA. Despite making some useful suggestions, it would appear that standardisation of techniques has still to be achieved, and it remains to be seen whether this will happen in the near future. Experiment 1b will go into these issues in more detail, and will outline a new method of creating auditory PWA stimuli developed for this thesis.

Summary

The field of PWA has been littered with controversy for over 100 years. The most prevalent problem has been the question of how to define and measure conscious awareness. The classic dissociation paradigm required that a direct measurement of conscious awareness be shown to be null, while an indirect measure of unconscious

perception demonstrates the effect of the stimulus. However, this paradigm suffers from various drawbacks, some practical and some ideological. Finding an exhaustive and exclusive measure of conscious awareness would seem to be impossible.

In the last 15 years, major advances have been made in the field that move away from the dissociation paradigm. These began with the realisation by Cheesman and Merikle (1988) that it may be possible to find qualitative differences between conscious and unconscious processes on a single task. Using the hypothesis that unconscious perception leads to automatic responses (as opposed to conscious perception) these authors were able to show that PWA can have different (but predictable) effects from perception *with* awareness. A further development was made when Jacoby and his colleagues (e.g. Jacoby *et al*, 1992) suggested the process-dissociation approach. While this approach has itself been the subject of controversy concerning the way in which conscious and unconscious processes are thought to relate to each other, the exclusion task has been particularly useful in demonstrating that PWA leads to participants being unable to consciously follow instructions. Instead, they appear to show automatic responses, and these automatic responses are of the type that would be predicted if unconscious processes were dominant over conscious processes. When placed in opposition to each other it is possible to make predictions based on whether a task is performed with or without awareness. The exclusion task is one such task that allows such predictions to be made. This, however, is not the only task in which conscious and unconscious factors are placed in opposition, and chapter 6 will outline another. It would seem that this paradigm offers the most compelling evidence to date that we can indeed be influenced by things of which we are not consciously aware.

Now that we have considered the issues that have dominated the field of PWA, we will now turn our attention to the way in which parapsychologists have dealt with “unconscious” stimuli. As has been previously noted, psi processing has long been compared to phenomena such as PWA. This has engendered a considerable amount of research by parapsychologists into empirically investigating the relationship between the

psi and PWA. The following chapter outlines what parapsychologists have had to say about psi and PWA, describes some of the experimental work conducted on the relationship between the two, and assesses how the work by parapsychologists incorporating PWA stands up in light of what has been discussed above concerning the mainstream PWA field.

Chapter 4 - Perception Without Awareness and Psi

General Introduction

Comparisons between psi and perception without awareness have been made throughout the history of parapsychology. Frederick Myers (1905) was perhaps the first researcher to suggest a relationship between the paranormal and information below the level of conscious awareness, placing emphasis on the phenomena of the séance room. In his presidential address to the Society for Psychical Research, French philosopher Henri Bergson emphasised the importance of unconscious perception to the field of psychical research. Although studies looking at PWA in a parapsychological context had been conducted (e.g. Eisenbud, 1965; Nash and Nash, 1963) it wasn't until the early 1970s that the relationship between the two phenomena became a hot-topic for parapsychologists. The resurgence of interest in comparing psi and PWA can be attributed to a seminal book published in 1971 by Norman Dixon on PWA (then known as subliminal perception). Beloff (1972) was probably the first parapsychologist to make the connection, noting how impressed he was with the apparent similarities between the two phenomena after reading Dixon's book. At that time, Dixon was Britain's leading researcher on subliminal perception, and Beloff deems it noteworthy that the subtitle of his book was 'the nature of a controversy'. Indeed, when reading Dixon's writings on 'subliminal perception' from the 70s and early 80s, it is difficult not to be impressed by the apparent parallels between his subject matter and that of parapsychology. At no point in either his original book or its follow up (in which subliminal perception is re-named 'preconscious processing') does Dixon make the comparison explicit, although he did later acknowledge the similarities (Dixon, 1979).

In the early 1980's Serena Roney-Dougal became one of the first parapsychologists to take a serious look at the relationship between the two phenomena, with Dixon as her PhD supervisor. Interest in psi and PWA reached a peak in 1986 when Roney-Dougal (1986), Schmeidler (1986) and Nash (1986) published three separate reviews. These authors, and, in particular Roney-Dougal, remain the last parapsychologists to have a close look at the two phenomena in question.

There are various ways in which the comparison can be made. What follows is a review of what parapsychologists have had to say about psi and PWA, starting with the conceptual similarities.

Conceptual Similarities

The starting point for any comparison between psi and PWA is the assumption that the psi process is an unconscious one (at least in certain conditions). Justification for psi being unconscious has been proposed in chapter 2, but it is probably worth reinforcing the case once more. Psi may be considered an unconscious process in the sense that it is extremely rare for a percipient to be aware that they might be receiving a psi signal. The outcome of most psi events consists of a behaviour or a cognition, but the actual stimulus that gives rise to these effects often remains unavailable to conscious awareness. All the percipient is aware of is the resulting effect. Thus, at the most basic level, the psi process could be construed as a special case of 'perception without awareness', with the perception being of an extrasensory kind. If one considers this to be a valid assumption, then the comparison between the two phenomena can proceed. Indeed, most parapsychologists who have considered psi processing from a cognitive standpoint have reached the conclusion that it is unconscious in nature, and might be similar to other forms of unconscious perception. To start with, given how rare it is for percipients to be aware of being influenced by a psi stimulus, it would seem that psi information is weak in nature. Studies into PWA

use weak or degraded stimuli in order to determine whether such weak (and by definition, unconscious) influences can have any subsequent effect on the cognitive system. In this sense, the comparison should be clear. It would follow, if the two phenomena are indeed comparable, that such similar inputs might be processed in similar ways. While it is possible that psi may have its own dedicated processing system, it would be highly uneconomical and therefore this is likely not to be the case. Instead, it is more parsimonious to expect psi inputs to be dealt with in much the same manner as other weak, unconscious stimuli (though see Kreidler and Kreidler, 1973, described below). If this were so, then it would add considerable credibility to the psi hypothesis. It would do this in the general sense that learning that psi has certain properties indicates that it has existence, and that it can be studied in similar ways to other, similar, psychological phenomena.

It is important to note, as Beloff (1972) does, that the analogy can begin only at the point at which the information about the psi target has entered the cognitive system and is awaiting processing. A stimulus that is of such intensity to allow PWA is, nonetheless, a sensory stimulus. It has known physical qualities (albeit weak) and is available to the known sensory apparatus of the percipient. A psi input, however, is, by nature, not available to any of the recognised senses, and, in an experimental situation, is actively shielded from the percipient's known sensory channels. In 1972, Gertrude Schmeidler suggested that "whatever psychological laws apply to the processing of ambiguous sensory material will apply also to the processing of ESP information" (Schmeidler, 1972, p.137). Likewise, J.B. Rhine, the father of modern parapsychology, has also commented on this issue, stating that "[i]t is here, in the common unconscious functions of both sensorimotor and extrasensorimotor (or psi) character, that parapsychology comes closest to psychology" to which he further added that it would "be advisable to keep our attention on all the psychological research on unconscious mental activities" (Rhine, 1977, p171).

In 1962 Rhine's wife Louisa stated that: "[t]he operation of extrasensory perception [is] not differentiated in consciousness and the person is unaware when it is or is not operating. The roots of ESP are obviously in the unconscious" (Rhine, 1962, p 89).

In addition to the Rhines, a number of other researchers have been impressed by the apparent similarities between psi and PWA (see, e.g. Beloff 1972; Dixon, 1972; Schmeidler, 1986; Nash 1986 and Roney-Dougal, 1981, 1986), and, conceptually at least, the comparison appears to be a valid one.

Previous Reviews

There are comprehensive reviews available by Roney-Dougal (1986), Schmeidler (1986) and Nash (1986). The reviews by Roney-Dougal and Schmeidler offer a more cogent discussion than Nash, who covers many issues, but does so without going into the depth required. Schmeidler presents 3 hypotheses derived from her review of the literature. Her first hypothesis states that when subliminal (PWA) stimuli are so weak as to be far below the threshold of conscious awareness, there would be a positive relationship between ESP and PWA scores. In other words, the weaker the PWA stimulus is, the more like an ESP stimulus it becomes. Schmeidler reports that, of 22 relevant studies, 5 showed the predicted positive correlation between PWA and ESP scores, 12 showed a trend in the predicted direction, and a further 5 reported either a nonsignificant trend in the opposite direction or null results. Schmeidler's second hypothesis was that this trend would reverse when the PWA stimuli were so strong as to be considered almost conscious. She reports only two relevant studies relating to this hypothesis, both significantly supporting it. The third hypothesis stated that the relationship between ESP and PWA should be stronger with free response tasks than with forced choice tasks. Neither of the two relevant studies reported by Schmeidler supported this hypothesis. Stanford (1990) conducted a meta-analysis based on the information presented by Schmeidler in relation to her first two hypotheses, and reported a significant ($p < 0.001$) positive correlation between psi and PWA responsiveness (see section PWA and PMIR below).

Roney-Dougal (1986) offers a review in which the comparison is made between PWA and psi from various angles, including a cognitive aspect, an emotional aspect, an individual differences aspect and a physiological aspect. Within each of these sub-groups, Roney-Dougal reviews converging work from each field in order to show that, in many instances, the phenomena in question are closely related. One puzzling claim made by Roney-Dougal, however, is the assertion that PWA effects are negatively correlated with stimulus energy, such that the further a stimulus is below the perception threshold the stronger its subsequent influence will be. This has been criticised by Stanford (1990) and it does seem to be a rather strange claim to make given what is known about human sensory capacities. One would expect that the intensity of any particular stimulus would be positively related with its effectiveness. Nash's (1986) review follows a similar line to Roney-Dougal's, reviewing properties of PWA that were first discovered in parapsychology and *vice versa*, and suggesting findings in each field that could plausibly be tested in the other.

There is general agreement between these reviewers that there is considerable evidence of parallels between the phenomena. Although the arguments of each author will not be replicated here, it is worth looking at some of the more important parallels in more detail. The interested reader is encouraged to consult the original articles to get each author's opinion on the issues.

Similarities between PWA and Psi

So, exactly in what ways are the two phenomena similar? What follows is a summary of some of the most striking comparisons, starting with general similarities that have been noted in independent studies, before moving on to research in which the relationship has been directly addressed. A further section below deals with specific comparisons relating to Stanford's PMIR model of psi functioning.

The first comparison is that both unconscious psi and PWA appear to be similar at a subjective level. Participants taking part in both types of experiment frequently report that their responses are 'mere guesses'. This fact has led some researchers to disguise tests of PWA as ones testing ESP ability (e.g. Calvin and Dollenmayer, 1959; Miller, 1940) and vice versa (e.g. Eisenbud, 1965; Lovitts, 1981). What these studies tend to show is that, to the participants, there is no subjective difference in taking part in either a PWA study or a psi study. Miller (1940) gave subjects a bogus telepathy test in which they had to gaze into a crystal ball and try to 'see' figures that were being sent telepathically. In actuality the target figures were projected upon the crystal ball at extremely weak intensities. Subjects scored well above chance, although they were never aware that it was not a genuine test of telepathy. In 1965, Eisenbud used the same strategy in reverse. He presented numerals 2, 3 or 4 at supposedly 'subliminal' levels, and asked subjects to state which number had been reported. On some trials (the psi condition) all three numbers were presented superimposed upon each other. Eisenbud found that subjects scored above chance only on the PWA trials and non-significantly on the psi trials, a finding he attributed to the small number of trials. Eisenbud also found a significant 'sheep-goat' effect¹, suggesting that certain personality variables may be important factors in psi and PWA tasks. In 1974, Stanford re-analysed Eisenbud's data, and reported a positive correlation between the psi and PWA scores. This correlation seems to suggest that people who perform well on PWA tasks also perform well on psi tasks. This may be due to these subjects being more 'open' to weak, unconscious stimuli (see below for discussion on correlations between psi and PWA performance).

Indeed, there are other personality variables that seem to affect performance on both psi and PWA tasks in similar ways. Gordon (1967; cited in Dixon 1981) discovered *post hoc* that students drawn from the arts departments of his institution performed

¹ This is the phenomenon whereby subjects who are open to the possibility of psi ('sheep') tend to score significantly better on psi tests than others who do not believe in psi (Schmeidler and McConnell, 1958).

better on tests of PWA than students from the Science and Engineering faculty. It may be argued that this is analogous to the finding concerning the apparent superiority of creative individuals in an ESP test (see Dalton, 1997b for a review of this literature), although it is not clear how much of a link there is between creativity and being a member of an art faculty. Additionally, Schmeidler (1986) cites a number of personality traits associated with high scorers in PWA studies, and draws direct comparisons with various personality characteristics that have been associated with good ESP subjects. She cites Eagle (1962) who researched personality and PWA, and compares these measures with her own work with personality and ESP (Schmeidler, 1960). For example, Eagle claimed that subjects responsive to PWA stimuli “show cognitive and affective openness rather than constriction; are receptive to ‘inner’ cues in the sense of being intuitive, introspective and insightful, and capable of fantasy and vivid imagery; are oriented towards and responsive to people” (Eagle, 1960 p. 3). Schmeidler compares these statements with her own observations about subjects in ESP experiments “subjects who were freely responsive to intellectual or emotional stimuli tended to score higher than subjects who were inhibited” (Schmeidler, 1960, p. 55). Also “subjects with signs both of marked inner (intellectual) activity and of (emotional) responsiveness to outer stimuli tended to have high ESP scores” (Schmeidler, 1960, p. 68).

Unfortunately, there has been very little work conducted on individual differences in PWA since then, and as such, the work by Eagle and Schmeidler must be treated with caution, given the differences that exist between the field of PWA as it is now and as it was in 1960.

A further comparison is in the conditions that have been found to be beneficial to performance in both types of task. There is some evidence (see Dixon, 1981) that PWA is best demonstrated when subjects are in a state of ‘relaxed passivity’. This is similar to the technique in parapsychology known as the “Ganzfeld” (see chapter 1 for

definition). Dixon cites a study by Fiss (1966), in which subjects were presented with 'subliminal' stimuli. Half of these subjects were instructed to concentrate and to report exactly what they had seen, while half were instructed to relax and draw their free imagery. Although the authors report that there was no difference between the two groups in terms of responsiveness to the stimuli during the stimulation period, they did find that during the reporting period, subjects who were relaxed responded more positively to the PWA stimuli. This finding is slightly different to that using the Ganzfeld, as, in the Ganzfeld, the reception of the stimulus and the reporting are simultaneous (or only slightly delayed). Indeed the philosophy behind the Ganzfeld states that it is the reception that is benefited by the reduced sensory input. Despite this, the finding by Fiss is interesting in that it emphasises that being in a certain mental state may promote openness to weak stimuli. It would certainly seem to make intuitive sense to surmise that the reduction of mental noise and incoming sensory stimuli will lead to an increased chance of weaker stimuli, be they weak perceptual stimuli or extrasensory, becoming prominent. Likewise, Fisher and Paul (1959) showed that the recovery of PWA stimuli in subsequent imagery is maximised by making the subject adopt a supine position in the dark. Studies in which a PWA component has been incorporated into a ganzfeld setting are discussed below.

The phenomenon of so-called 'psi-missing' also has a parallel in the field of PWA in what is known as 'perceptual defence'. Perceptual defence is the apparent elevation of perceptual thresholds when dealing with emotional material (as compared to neutral material). This appears to occur unconsciously, before the relevant information is available for conscious analysis (see, e.g. Bruner and Postman, 1947). The suggestion is that incoming stimuli are unconsciously processed at some semantic level, and an appropriate strategy for either allowing or denying the information into conscious awareness is formed. Psi-missing may be considered similar as it also implies that information is processed unconsciously and a strategy formed based on this information. If this so-called 'psi-missing' is a real effect, then it would seem to imply

that the subject somehow knows at an unconscious level what the correct answers are, but manages to avoid them. This may be due to attitudinal reasons (e.g. 'goats' may actively avoid making correct guesses), motivational reasons, or emotional reasons. When comparing psi-missing with perceptual defence, then it is the latter explanation that offers the most fruitful comparison. In perceptual defence, it appears as if the subject is aware of an emotional stimulus at some preconscious level, and then somehow alters awareness thresholds accordingly. Johnson (1975) has stated that ESP targets that are negatively emotional tend to evoke psi-missing. Like perceptual defence, it looks like subjects have gained information about the targets at an unconscious level, then have adjusted their response appropriately.

Another PWA effect which seems to have some parallel with parapsychology is what is known as the Poetzl effect, in which information presented outwith awareness becomes manifest in dreams. At the Maimonides Medical Centre, Ullman, Krippner and Vaughan (1973), report similar experiments in which extrasensory information was apparently reported in the dreams of the participants (though see Belvedere and Foulkes, 1971 and Foulkes *et al*, 1972 for failed replications of this work). Similarly, Shevrin and Fisher (1967) investigated PWA effects in REM and non-REM sleep and reported that sleep may enhance the recovery of unconscious stimuli.

A further comparison lies in the curious 'experimenter-effect', in which certain experimenters tend to consistently obtain psi-hitting or psi-missing in the experiments they are involved in. Wiseman and Schlitz (1997) empirically tested the experimenter effect and found the psi proponent (Schlitz) tended to elicit positive psi results, while the sceptic (Wiseman) did not, despite using identical methodologies. This effect has become part of parapsychological lore, with many researchers being labelled as to whether they are 'psi-conducive' or 'psi-inhibitive'. An identical trend in the field of PWA was commented on by Dixon (1971), who stated that "[w]e have, after all, to explain why it is that some experimenters, some academic departments, and even

some countries, invariably provide positive evidence for subliminal perception effects while others with almost equal monotony do not” (p. 242). He could almost have been talking about psi. This issue, however, has never been picked up by latter-day researchers in the field of PWA, and it is probably true to say that it is no longer the case, with replicable results the rule rather than the exception. The fact that Dixon noticed this trend, and its subsequent disappearance, suggests that there were potential problems with the methodology employed in many of the early PWA studies. This in itself has obvious implications for parapsychology.

Although these are not all the comparisons that have been made between the two phenomena, they are among the most important and most striking, and illustrate the extent to which parallels might exist.

Experimental studies

We shall now turn our attention to experimental studies which have incorporated psi and PWA components. What follows is not an exhaustive list, but serves to illustrate the kinds of experiments and approaches researchers have undertaken. There are a number of ways in which parapsychologists have incorporated a PWA element into their studies. The first approach we shall consider involves the possible effect of ESP on PWA performance.

Kreitler and Kreitler (1972) investigated the influence of ESP on the degree of PWA. The authors presented stimuli at ‘below threshold’ levels and asked participants to guess which stimuli had been presented. Meanwhile, on certain trials, a sender would be attempting to influence the participant’s decisions. What Kreitler and Kreitler found in this study was that trials using stimuli which were, in their words, ‘definitely’ below threshold (i.e. stimuli that were not identified in more than 33% of trials in the control condition) seemed to benefit most from the presence of a sender (in the sense that the sender’s influence seemed to help the participant make a correct choice). This

effect did not occur with stimuli which were either near threshold or above it (see chapter 3 for a discussion on the drawbacks of using thresholds). An interesting aspect of this study was that the participants were naïve to the existence of the sender, suggesting that the participants were not intentionally trying to obtain any psi information relevant to the task. A follow-up study in 1973 focused on whether a ‘subliminal’ stimulus could give rise to various perceptual illusions, such as the Muller Lyer illusion². In Kreitler and Kreitler’s (1973) study, stimuli intended to create a perceptual illusion were presented outside of awareness. Kreitler and Kreitler found no difference between two forms of ‘sending’ (actively ‘transmitting’ versus passive thinking) nor between the relationship between the sender’s message and the subliminal stimulus. There was, however, an interaction between these factors suggesting that, when the subliminal and sender’s message contradict each other, a ‘transmitting’ (rather than a passive ‘thinking’) sender is more successful. This is a curious finding. The authors attempt to explain it by suggesting that ESP has its own ‘channel’ which is separate to those used for normal stimuli. The ESP information is largely unattended and as such has little effect on subsequent cognitions, especially when there is a contradiction between the information available from each source. However, if the ‘normal’ information is of an extremely weak nature (e.g. subliminal or PWA information), then the ESP information, has more chance of being attended to. While there is a vague plausibility to this account, positing an ‘ESP channel’ would appear to be premature, given that there very little other evidence suggesting that ESP has its own dedicated processing system. Lubke and Rohr (1974) successfully replicated the Kreitler and Kreitler findings, although Child (1977) has strongly criticised both the Kreitler’s two studies (and the subsequent replication by Lubke and Rohr), suggesting that their data might contain a possible statistical

² For example, the Muller-Lyer illusion consists of two lines of equal length with each line flanked by 2 ‘V’ shapes, pointing either inward or away from the line. This leads to the illusion that the lines are actually different lengths

regression artefact (see Child, 1977 and Kreitler and Kreitler's (1977) reply to this criticism for a more detailed discussion).

While Kreitler and Kreitler were attempting to determine the possible influence of ESP on PWA performance, some studies adopted the inverse position and looked at the influence of PWA stimuli on ESP performance. Palmer (1992) used what he called the 'perceptual ESP test' or PET. The PET is based on eye fixations directed towards a matrix of stimuli consisting of typewriter carets (e.g. < > ^ \). The participants task was, when presented with an array of these symbols, to choose which is the most salient. Meanwhile, a sender is remotely attempting to influence the choice in accordance with a randomly chosen target symbol. In addition to this, participants listened to baroque music during the session. This is due to the rationale for the PET, which states that conscious attention should be focused on something else, as more automatic/involuntary responses (e.g. eye movements) might be better indicators of psi (see, e.g. Stanford, 1975 and chapter 3 of the current thesis). Palmer also presented, either for 2ms or for 2 seconds the phrase "Ashley and I are one", where Ashley was the sender in the session. Results of this study indicated that presenting the phrase outside of awareness did seem to positively influence scores on the ESP test. Similar studies were conducted by Palmer (1993) and Palmer and Johnson (1991) using 'threatening' stimuli presented outside of awareness as a means of influencing ESP responses. Results were mixed, but there was some suggestion of a possible 'response-bias' effect (see chapter 5).

Other studies (e.g. Smith, Tremmel and Honorton, 1976; Roney-Dougal, 1979a 1979b 1987; Kanthamani and Palmer, 1993) investigated the relationship between psi and PWA using the Ganzfeld technique. Smith *et al* placed both sender and receiver in a Ganzfeld environment. Prior to being placed in the Ganzfeld, the target image was displayed to the sender via a tachistoscope either for 1ms or for 10 minutes. Smith *et al* report above-chance scores, with the effect reaching significance only when the

target was presented for 1ms. Kanthamani and Palmer (1993) report a similar study, aiming to test whether a target presented outside of awareness (10 repetitions, each for 1ms) to a sender could be picked up by a receiver in the ganzfeld. No significant effect was found in this study. Roney-Dougal's (1987) work utilised the ganzfeld technique and involved either a sender attempting to influence the mentation of the participant, or various target related 'subliminal' words being presented to the participant masked by white noise (see chapter 3 for a discussion on the problems of auditory PWA work). Roney-Dougal reports that the target was chosen significantly more than chance would predict in both the psi and the PWA conditions. Indeed, she reports that there is no significant difference between the scoring rates in each type of task, stating that "there seemed no clear way of differentiating between the two at the response side of the process, although they are clearly different at the stimulus side" (Roney-Dougal, 1987, p 174). She also reports that both PWA and psi scores seem to be related to the same psychological variables, such as attitude to psi and openness to experience. Roney-Dougal's study does have drawbacks, however. She only used a limited number of subjects (eight in the main study) and tested them several times over the course of a year. Additionally, six of these participants had taken part in her pilot study. This leads to the possibility that learning effects may have affected the results. If the participants, over the course of several sessions, learned (consciously or unconsciously) to adopt a strategy (e.g. if there were parts of the masked stimuli that were conscious at any particular time) then this may have influenced results. This may be a harsh criticism, but such implicit learning effects do appear to occur (see, e.g. Berry, 1997), and their implications for parapsychology are not yet known.

Another approach has been to attempt to correlate performance on psi and PWA within one study. As mentioned above, justification for this approach lies in the possibility that the two types of stimuli are processed in similar ways. If this is the case, then performance on one type of task should be correlated with performance on the other. If certain individuals can be shown to be more 'open' to weak sensory

stimuli, then it follows that they might also be open to weak extrasensory stimuli. This has been the basis for much of the experimental work comparing psi and perceptual defence (see above for definition). Although the perceptual defence work is more concerned with the 'grey' area between conscious perception and unconscious perception rather than true unconscious perception, it is relevant to the current discussion in the sense that it offers a potential correlation between susceptibility to weak sensory information and susceptibility to psi. Studies making this comparison have usually used the Defense Mechanism Test (DMT) developed by Kragh (1962). Without going in to too much detail, there are a number of studies suggesting that a correlation exists between how defensive an individual is and his subsequent performance on an ESP test. The most consistent finding being that 'high defensive' people score lower on the ESP tasks than 'low defensive' people. Thus, it may then be posited that 'low-defensives' are generally more open to weak stimuli (including ESP), while 'high-defensives' tend to block both weak sensory and extrasensory stimuli. A more detailed account of these issues can be obtained from the following sources: Johnson and Kanthamani, 1967; Johnson, 1975; Johnson and Lubke, 1977; Haraldsson, 1978; Johnson and Haraldsson, 1984; Haraldsson, Houtkooper and Hoeltje, 1987; Haraldsson and Houtkooper, 1992; Watt and Morris, 1995.

Apart from the studies on perceptual defence, there have been a number of other studies investigating how psi and PWA might correlate with each other.

As previously mentioned, Stanford (1974) reanalysed Eisenbud's (1965) data and discovered a correlation between both types of task that the original author had not been aware of. Nash and Nash (1963) presented participants with blank slides which either had the target superimposed for 0.01 seconds or, in the clairvoyance condition, merely had the target written on the slide mounting. Nash and Nash found significant results for the PWA condition, but not for the psi condition. The method of

presentation in this study, however, has been criticised by Roney-Dougal (1986) as being long enough to allow for conscious perception.

Kelly, Kanthamani, Child and Young (1975) used ordinary playing cards as a means of testing ESP and PWA. Using only one participant, who was apparently gifted at predicting playing cards, Kelly *et al* tested under ESP and PWA conditions. Confusion matrices were tabulated from the set of incorrect responses in each condition, and there appeared to be a strong correlation between the two matrices, suggesting parallel cognitive parameters for both tasks. There are, however, several problems inherent in this study. The use of a single subject means that fraud is less easily ruled out. Indeed, the individual used by Kelly *et al* had been previously accused by a magician to have been seen cheating on a test of ESP (see, e.g. Hansen, 1988). This reservation is reinforced when one considers that the participant had some influence over the method of testing. Additionally, the means of testing ESP was far from ideal, and could hypothetically have allowed for non-psi identification of the targets by the participant. Finally, the method of presenting the PWA stimulus was criticised by Roney-Dougal (1986) as being close to conscious recognition parameters.

Wiklund (1975) predicted that participants who were shown to be influenced by a PWA stimulus would be more successful at a psi task. Wiklund's PWA task was based on a perceptual illusion. The illusion was based on the fact that, when a fan-like pattern of lines is superimposed upon a square, then the perception of the square is distorted such that the upper line appears longer than the lower line. Wiklund presented participants with a square, but immediately beforehand, exposed them briefly (how briefly is not made clear by Wiklund) to the biasing pattern of lines. Those who reported the upper line to be longer were said to have been influenced by the PWA stimulus. A clairvoyance test in which participants had to guess concealed target pictures was used as the ESP test. In his first series, 3 out of 8 subjects appeared

to be influenced by the PWA stimulus. However, these subjects saw the illusion in the *opposite* direction from what was predicted (i.e. they saw the bottom line longer than the top one rather than *vice versa*). Interestingly, these participants also displayed significant psi-missing, adding a novel spin on the psi-PWA correlation. Unfortunately, an attempted replication with more participants was unsuccessful.

Haight, Morrison and Kennedy (1978) used separate psi and PWA tasks, and reported a non-significant positive correlation between performance on each ($p = 0.07$ two tailed). However, while the psi scores were marginally significant, PWA performance was found to be at chance levels. Nash (1979) attempted to discover whether various well documented psi effects would show up in a PWA test, and reported five effects that had been documented in early Zener card ESP work also appeared in his PWA test.

In 1980, Munson reported an interesting study in which he explicitly informed participants that some targets were psi targets while some might be 'subliminal'. Munson also reduced exposure time for the PWA trials over the course of the session and reported that when the exposure time decreased, psi scores tended to increase. Munson suggests that psi and PWA might exist on a continuum whereby they are different expressions of one particular mode of processing. Alternatively, the two effects may be conflicting modes of processing, and PWA actually obscures ESP, such that ESP works best in situations where the possibility of PWA processing is reduced to a minimum.

Finally, some studies have found a *negative* correlation between the two types of task, such that success on one type of task correlates with failure at the other. Rao and Puri (1977) hypothesised that meditation would be a variable that would inhibit sensory perception and enhance psi performance. Participants were presented very briefly with slides which were either blank (ESP condition) or pictorial (PWA condition).

Participants were tested both before and after being instructed in transcendental meditation. Overall, a significant negative correlation was found suggesting that the lower one's PWA scores were, the higher one's ESP scores would be. The apparent effect of meditation training was that there was a non-significant increase in PWA scores after training, and a non-significant decrease in ESP scores. The authors raise the possibility that psi may have been operating in the PWA trials, and as such the results may have been a consequence of the so-called 'differential effect'³ (see e.g. Rao and Sudhakar, 1987). This interpretation posits that participants with high PWA scores may have used psi to hit the PWA targets and miss the ESP targets, while those with low PWA scores may have used psi to miss the PWA targets and hit the ESP targets. In a follow up study Rao, Sundari, Rao and Rao (1977) conducted another study in which the sensory signals were weaker than in the original Rao and Puri study. Rao *et al* used a weight discrimination task, in which a participant held a weight in each hand and had to report which was heavier (PWA condition – although awareness in this experiment is *very* poorly defined). In a separate room, another participant was asked to guess which of the weights held by the other participant was heavier. Responses by participant two were checked against the actual weights (clairvoyance condition) and the response of participant 1 (telepathy condition). Despite the PWA scores being at chance levels, there was a significant negative correlation between PWA performance and the telepathy condition. Again, the differential effect cannot be ruled out. Rao and Rao (1982) conducted two further studies. The first study consisted of a PWA condition and a PWA + psi condition, using picture slides as targets. Contrary to their previous work, they found a (non-significant) positive relationship between ESP and PWA in both conditions. In a second study, they returned to using transcendental meditation as a way of influencing performance on the tasks. A meditation group was instructed in meditation for a week,

³ The differential effect has been defined by Rao (1966) as a "tendency to score positively on one condition and negatively on the other in the same experiment consisting of two different conditions such as two kinds of targets" (p. 244)

while a control group was not. Rao and Rao (1982) report a significant interaction in that the effect of meditation on ESP appeared to depend on the PWA performance, and the effect of PWA on ESP depended on whether the subjects meditated or not. In other words, the ESP scores appeared to depend on the interaction of TM and PWA, suggesting that those who are sensitive to PWA and psi stimuli are influenced more by meditation. The authors state in their conclusions that “the way SP [PWA] and ESP are related may be dependent upon the strength or gradation of the subliminal signals and the state of the individual. Depending on the physiological and psychological state of the individual, the information processing of subliminal and extrasensory impressions may take place in different ways” (Rao and Rao, 1982, p. 202). Thus, this study, in finding a positive correlation between performance on the two tasks, also uncovered various mediating variables that can alter the relationship in various ways. This goes some way to accounting for the previous negative correlations found by Rao and Puri (1978) and Rao, Sundari, Rao and Rao (1977).

In summary, research into psi and PWA has been wide and varied, with mixed results. It is disappointing that a common thread has never been established, as a consistent line of research into the nature of the comparison would be desirable. Instead, various researchers have adopted PWA for a variety of reasons, with mixed results. A more specific comparison between psi and PWA will now be made, placing emphasis on the PMIR model.

Perception Without Awareness and PMIR

Stanford was aware of the importance of PWA in relation to his PMIR theory. Indeed, in his 1990 revision, he devotes large sections to reviewing both the relevant psychological literature and the work on psi and PWA conducted by parapsychologists. He is particularly concerned with the potential correlation between performance on psi tasks and performance on PWA tasks, and, as previously mentioned reports a significant positive correlation between psi and PWA. Stanford is

encouraged by this, as it seems to fit into the conceptual framework of the PMIR model; that psi is processed in a similar manner to other unconscious information. Given that the PMIR model is explicitly concerned with psi processing as it relates to other forms of unconscious processing, we will now turn to the various aspects of the PMIR model that place emphasis on the comparison.

The PMIR model is very psychological in nature, and it is important that various characteristics are demonstrable in other areas of psychology. The similarities between psi and PWA have been stated above. However, when one considers specific aspects of the PMIR model, one can immediately locate common factors both in terms of the underlying theory and in the way both forms of perception are thought to operate.

In formulating the PMIR model, Stanford placed great emphasis on the role of “needs” and the way in which psi might be need-fulfilling. This makes sense from an evolutionary point of view, and it is no surprise to find that other authors have also recognised the importance of needs when considering cognitive phenomena. For example, Dixon states that “so called cognitive processes (whether conscious or unconscious) must have evolved for the gratification of need.... Whether biological or social in origin, these needs and the drives or motives to which they give rise may continue to exercise a selective function upon perception, memory, thinking and upon the entry into the consciousness of sensory inflow and material from long term memory” (Dixon, 1981 p 81). The ability to perceive information that is either outside of awareness or is at degraded levels would appear to have survival value. Likewise, as discussed previously, if extrasensory abilities have evolved, then they also are likely to serve needs and have survival value. Dixon emphasises the importance of these processes by stating “even (perhaps especially) during conscious perceptual experience of innocuous events it is biologically important that the organism be alerted by the arousal of emotionally important or threatening stimuli, which for one

reason or another (e.g. because masked, unattended, brief or of low intensity) cannot themselves achieve conscious representation” (p. 128).

Dixon also cites articles by Brown (1953) and Mikelson (1913) which may be relevant to the need component in PMIR. Brown is quoted as claiming that drives (needs) such as hunger may lower an individual's perceptual threshold so that he becomes sensitive to reduced cues from need-related objects. Similarly, Dixon cites Mikelson, who reported how hunger gave him a “sort of second sight, making it the easiest thing to find anything in the shape of food” (Mikelson, 1913, cited in Dixon, 1981, p. 114). Although Stanford did not directly address this issue, it is interesting to speculate that the existence of a particular need may perhaps make one *more* open to psi (or unconscious) information relating to that need.

Probably the most striking similarity between the PMIR model and PWA is in the way the information is thought to be processed, and the effects it has on the system. Recall the way in which PMIR is accomplished, according to Stanford; “PMIR is possible because psi information in the system in interaction with the organism's current action plans and circumstances, primes incipient dispositions...” (Stanford, 1990 p. 102). Compare this with Kihlstrom (1987) who states that “preconscious processing might be confined to the facilitation of pre-existing inclination” (p. 1450). These statements are remarkably similar, and there appears to be some agreement concerning the nature of the influences that the inputs have. It would seem that both authors share the opinion that weak stimuli (be they sensory or extrasensory) tend to evoke simple responses which are already inherent within the system. To these we can add Dixon (1981), who claims “sensory inflow may be subjected to successive levels of analysis and have significant effects upon many sorts of psychological functioning without ever itself being consciously represented” (p. 19). Again, this supports the suggestion that ongoing processes may be influenced by weak, unconscious inputs.

Stanford also stated that “PMIR is accomplished through psi-mediated facilitation, release or triggering of behaviors, feelings, images, associations, desires, or memories” (1990, p 102). Likewise, Dixon (1981) states that “though unrepresented in consciousness, [the] residue of subliminal and/or unattended inflow may have profound effects not only upon the processes of perception, memory and emotional responses, but also upon consciousness itself” (p. 4).

There has been considerable work suggesting that PWA information may influence these processes. For example, Dixon (1981) cites a number of his own studies in which he found that PWA information could influence ongoing associations. Compare this to Stanford’s 1973 study in which he found that a “sender” could influence the associations of a receiver in a separate room. Kihlstrom (1987) suggests a similar role PWA by stating that “preconscious processing [i.e. perception without awareness] can influence the ease with which certain ideas are brought to mind” (p 1448). Studies cited above by Zuckerman (1960), in which subjects were unconsciously influenced to write more, and Somekh (1976) in which unconscious perceptual information appeared to influence story writing suggest that behaviour may be affected by stimuli perceived without awareness. This is another sense in which Stanford believes PMIR might work. Affect as a PMIR response has also been proposed, and work by Zajonc (1968) and Niedenthal (1990) also seem to suggest that affect can be influenced unconsciously. In short, when one considers the various studies investigating the effects of PWA, it looks as if almost every “vehicle” for PMIR suggested by Stanford has also been shown to mediate weak sensory information.

One further similarity would seem to be the fact that much of the recent PWA work has relied on the hypothesis of unconscious influences leading to automatic responses (see, e.g. Hasher and Zacks, 1979 and chapter 3 of the current thesis). This appears to tie in with much of what Stanford says about the processing of psi stimuli and the subsequent effects they can have. According to Stanford, the majority of PMIR

responses will be automatic in order to fulfil the “need” in as economic way as is possible, and in order to bypass conscious awareness of the action being carried out. In this sense, then, the way PMIR-type information is processed appears to have the typical characteristics of other unconscious influences.

Additionally, Schmeidler (1972) comments extensively on various ways in which psi information might be processed once it reaches the brain. She draws comparisons with the way that unconsciously perceived information may be processed, and many of her comments are particularly relevant to the PMIR model. For example, she states “for ESP as for incidental stimuli, what’s relevant to a person’s needs...is likely to ‘come through’ either by facilitating ESP hits...or by facilitating psi missing” (Schmeidler, 1972, p. 136).

One aspect of the PMIR model which differs from PWA is the problem associated with scanning. While the PMIR model suffers due to the lack of boundary conditions (see above) this is not a problem in PWA, as the boundary conditions are defined within a certain range (i.e. any PWA stimulus must be available to the senses). One problem that does apply to both, however, is the problem of filtering. Presumably, not every unconscious sensory stimulus is processed semantically, which suggests the existence of some form of filter. This is not an issue that has been adequately addressed in the field of PWA, which has tended to focus primarily on laboratory studies, choosing to largely ignore the ‘real-world’ consequences of the findings.

There is, of course, the danger of attributing too much intelligence to unconscious processing, in much the same way Freud and those of the psychodynamic tradition did. While this approach probably overestimates the capabilities of the unconscious (to the point where it is conceived as a ‘self within a self’), it still remains to be discovered exactly what kind of weak information gets processed in the real-world, and, if any filtering of information does take place, exactly how this is carried out.

This is an important issue, and one that needs to be considered by researchers in both PWA and parapsychology.

Methodological Problems

There is one underlying problem with all the previous work comparing psi and PWA; with the exception of Stanford (1990) none of it addresses or even acknowledges the methodological criticisms levelled at the field of PWA. This is somewhat curious, as the basis of most criticisms can be traced back to Eriksen (1960). It is particularly worrying that parapsychological work incorporating PWA conducted after Holender's critique in 1986 shows such little sensitivity to the important issues in the field. It is unfortunate that such a lack of awareness has existed in the field of parapsychology, as it in some way suggests that parapsychologists are not keeping up to date with current issues in psychology, despite continuing to make comparisons between psi and psychological phenomena.

Although they do not directly address the problems raised by Holender, Roney-Dougal (1986) and Schmeidler (1986) do make a distinction between studies in which the purportedly PWA stimulus is likely to be below the perception threshold and studies in which it appears to be close to the threshold. Exactly where the threshold should be set, however, is somewhat arbitrary, and, in the current climate, it might be argued that thresholds are meaningless constructs (see chapter 3). Thresholds are a useful analogy however, as it is almost certainly true that weak stimuli must cross over some kind of critical threshold in order to be processed at all. Likewise, there must also exist some form of boundary at which stimuli become conscious. However, while such concepts are useful for explanatory purposes, they are impossible to define or experimentally measure. As we have already noted, the only way to set such thresholds is to appeal to the dissociation paradigm in which a measure of conscious awareness is compared to a measure of unconscious perception. Adopting this strategy leads to the problem of defining and measuring conscious awareness, which as we

have seen, is extremely difficult, if not impossible to do adequately. The question of awareness is central to all studies in which claims of unconscious processing are made, although in the psi/PWA studies cited above it is all but ignored.

Table 4.1 illustrates how previous researchers in parapsychology have approached the presentation of PWA stimuli. It outlines the modality used, the definition of awareness (if any), the stimulus presentation details and any particular comments that should be noted concerning the study. In many cases, the researcher seems to arbitrarily choose how the stimulus should be presented on the assumption that the resulting signal will be weak enough to be unconscious. This is inadequate, as without any measure of conscious awareness, it is impossible to know whether any effects observed are due to the influence of conscious or unconscious perception. In the studies that do have some form of awareness measure, this usually consists of asking the participants whether they are aware of the stimuli. Again, this is far from ideal for the reasons given in chapter 3. Several studies attempted to set the optimum level of stimulus presentation either by conducting a pre-test on the individual participants, or a pilot test on a separate group drawn from the same population. Again, this technique uses the dissociation paradigm in that performance on the pre-test is measured, and once it drops below a pre-defined level, the stimuli are considered to be outside of awareness.

Table 4.1 – Previous studies looking at psi and PWA

Author(s)	modality	Definition of awareness	Stimulus presentation	Comment
Eisenbud (1965)	Visual	Dissociation paradigm – pre-tests conducted on which Ps claimed to have no awareness, yet consistently scored above chance.	0.83ms	Illumination of stimuli also adjusted.
Kreitler and Kreitler (1972)	visual	Threshold setting – defined as the level at which a participant recognised at least 1, and no more than 3 (out of 4) stimuli at a given illumination. Degree of illumination appropriate for correct identification of 1-5 out of 12 letters then used for participant.	10ms	Illumination of stimuli varied while duration held constant
Kreitler and Kreitler (1973)	visual	None stated	Not stated	
Kelly, Kanthamani, Child and Young (1975)	visual	None stated	8ms	
Lubke and Rohr (1975)	visual	Threshold setting – defined as the level at which a participant recognised at least 1, and no more than 3 (out of 4) stimuli at a given exposure time.	Initially 3ms – subsequently adjusted in accordance with threshold measurements	
Terry (1976)	visual	None stated	1ms	
Smith, Tremmel and Honorton (1976)	Visual		1ms	
Haight, Morrison and Kennedy (1977)	Visual (light discrimination)	None stated	8ms (from Schmeidler, 1986)	
Rao and Puri (1977)	visual	None stated	40ms (from Schmeidler, 1986)	Performance on PWA task consisted of 30% ‘hit’ rate where 10% expected by chance.
Rao, Sundari, Rao and Rao (1977)	Weight discrimination	None stated	No absolute measures given.	Difference between the weights reported by the authors to be “too small to permit veridical guessing at the sensory level” (p. 80)
Munson (1980)	visual	‘baseline established’	16.67ms and 8ms	No account of baseline offered.

Munson, Davis and Rao (1981)	visual	None stated	Not stated	
Rao and Rao (1982)	visual	Less than 30% discrimination on pilot deemed as unaware	10ms	
Roney-Dougal (1987) – exploratory study	Auditory	Pre-defined for all participants prior to study	Words masked by white noise at an undefined level.	Presentation method acknowledged by author as being inadequate.
Roney-Dougal (1987) – confirmatory study.	Auditory	Masking gradually increased until participant could no longer identify word. This was then set as the threshold level.	3 levels used below threshold; -5db, -10db, -15db	Reliance on verbal report as a measure of conscious awareness.
Palmer (1992)	visual	Ps asked to make judgement of stimuli	10ms	70% of participants were able to report some (non-critical) aspects of the stimuli
Palmer (1992)	visual	Pilot test conducted to obtain exposure time at which stimulus could not be identified	2ms	Possibly below objective threshold (based on pilot). No Ps able to consciously identify targets when asked to give verbal report.
Kanthamani and Palmer (1993)	visual	Post-session questions relating to nature of stimulus	1ms	No Ps able to fully verbally identify PWA stimulus. Three Ps able to partially identify stimuli.

Table 4.1 illustrates previous studies between psi and PWA. As can be seen, there is large variation in the techniques used and relatively little uniformity. The majority of studies have been in the visual modality (15 out of 18) with 2 using auditory stimuli (Roney-Dougal, 1987) and 1 using a weight discrimination task (Rao, Sundari, Rao and Rao, 1977). The visual studies used a variety of different exposure times for the “subliminal” stimulus, all of which were unmasked, ranging from 0.83ms (Eisenbud, 1963) to 40ms (Rao and Puri, 1977; stimulus duration cited in Schmeidler, 1986). The majority of other visual studies tend to use duration times of around 1ms (as in the case of Smith, Tremmel and Honorton, 1976) or 10ms (e.g. Palmer, 1992). Some studies (e.g. Kreidler and Kreidler, 1973; Munson, Davis and Rao, 1981) failed to state any stimulus

duration times. Of the two auditory studies (Roney-Dougal, 1987), the signal to noise ratio was undefined in the first one, and set at 3 levels (-5db, -10db, -15db) in the other. The problems previously stated concerning masking stimuli should immediately call into question the validity of the masking procedure used by Roney-Dougal, which did not seem to take variability of the intensity of the stimuli used into account when masking them.

The definitions of conscious awareness employed are also variable. Out of 17 studies 8 fail to explicitly state exactly how conscious awareness was conceptualised, which is obviously a major problem, given that defining conscious awareness is necessary in order to determine what might be unconscious. The remaining studies relied on the participant to determine when a stimulus was outside of awareness. This consisted of either a discriminative response in which the participant failed to make discriminations above an arbitrary cut-off point (e.g. Kreitler and Kreitler, 1977), or some form of self-report (e.g. Palmer, 1992). Again, as we have seen in chapter 3, there are major drawbacks in using such techniques.

As was described in the previous chapter, the field of PWA has moved on considerably in recent years, and it seems that parapsychologists have been left behind. Just about all of the studies conducted by parapsychologists incorporating PWA would not be considered legitimate by current researchers in PWA. This is not to say that they are worthless. It is highly likely that many of the studies conducted actually *do* get at some aspect of unconscious processes. The problem is that most, if not all, of the studies employ the dissociation paradigm, either explicitly or implicitly. As was explicated in the previous chapter, the assumptions on which this paradigm are based on are highly questionable, and it is for this reason that it has been largely abandoned in the field of PWA. Those that do not adopt the dissociation paradigm tend to set arbitrary criteria for stimulus presentation which leave it impossible to conclude anything from any observed effects. This leaves parapsychologists in a somewhat awkward position. While many

still claim that psi processing is directly comparable to other unconscious psychological processes, there is not a single piece of supporting evidence that would currently be considered valid in the field of PWA. This thesis presents the first attempt to bring the comparison up to date, using methodologies from the field of PWA. It represents a somewhat ‘back to basics’ approach. This is necessary, as the most pressing issue is to re-examine whether there is an actual similarity. While much research has previously considered the psi/PWA comparison in terms of free-response methodologies and the ‘symbolism’ that these lead to, the present paradigm is much more modest. As such, the present thesis will avoid testing any of the more grandiose claims sometimes made by parapsychologists when comparing psi to PWA. Instead, the primary aim of the thesis will be simply to demonstrate some basic effects in PWA/parapsychology, and then attempt to obtain a comparable effect in the other field. If this can be shown to be the case, then perhaps the comparisons can become more adventurous, but until then the most important thing is to establish whether or not the current findings in the field of PWA can also apply to psi research.

Summary

Conceptualising psi as an unconscious process has been common since the early days of modern parapsychology. It would appear that there are many similarities between the two phenomena. Experimental studies have taken a number of different approaches, with varying degrees of success. There has been, however, little uniformity in the methods employed by parapsychologists to present stimuli that are outside of awareness, and many parapsychologists have failed to define awareness in a satisfactory way. As such, the majority of the previous parapsychological studies using “unconscious” stimuli would fail to stand up to the main criticisms in the PWA field. This leads to serious doubts concerning the validity of these studies. The present thesis aims to address these issues, and bring the comparison up to date.

Chapter 5 – Psi, Word Association and Auditory PWA

General introduction

The following two studies were conceived as a way of comparing psi and PWA using a protocol that has been previously successful in Parapsychology. Experiment 1a is an attempt to replicate an effect found by Stanford (1973). This effect focuses on word association as a potential automatic process that might be susceptible to an unconscious psi influence. Experiment 1b uses new techniques in PWA research to investigate whether a stimulus presented outside of awareness can influence word association in a similar way. If it can be shown that psi and PWA have comparable effects on word association, then it would support the hypothesis that psi can be meaningfully compared to other unconscious phenomena. The methodology employed in experiment 1b was innovative in two important ways. Firstly, the task used was an exclusion task (in which participants had to actively avoid using a presented stimulus when responding), and was the first of its kind to be adapted for auditory PWA research. Secondly, the method of creating PWA stimuli for experiment 1b was developed especially for this study, and represents a new technique for this type of research.

Experiment 1a – Psi and word association.

Introduction

One process which may be useful as a psi vehicle is associations. As Roll (1966) noted, mediation of psi information is likely to be governed by the principles of normal cognitive processing, including those governing associative connections. For example, Roll stated that the arousal of a particular memory trace might, in turn, arouse other memory traces which are associated with it. He then went on to suggest that, if the function of ESP involves activation of memory, then it may be the case that associated memory traces may also be activated by the ESP stimulus. Similarly, in 1973, Stanford

suggested that extrasensory information would have to become imposed on, mediated by or interact with, ongoing processes. Stanford (1973) conducted a study testing this hypothesis, the results of which seemed to suggest that a word association test might be a useful way of looking at the way in which psi information is processed. An attempted replication in 1977 was less successful, however, although there were trends suggestive of an effect. Stanford's PMIR model also emphasised the role of association in psi processing.

Using word association as a psi test has several further advantages. Firstly, participants need not know they are participating in a parapsychology study. Consequently, participants don't have to be asked to 'turn on' their ESP, which may cause considerable self-consciousness or may even lead to dissonance depending on their worldview. This may have a considerable detrimental effect on psi performance. A word association task only asks participants to report the first associated word that comes to mind after presentation of a stimulus. In many of the word association experiments used in parapsychology, the assumption is made that ESP will work automatically on an unconscious level (e.g. Stanford, 1973). A related advantage is that it is more ecologically valid, reflecting how nonintentional psi may work in the 'real' world. It is unlikely that the psi experiences typically reported in the spontaneous case literature are a result of a conscious intention on the part of the percipient. Indeed, as stated above, many of these spontaneous experiences take the form of associations, feelings etc., suggesting an interaction with ongoing processes.

Given the conceptual and methodological advantages and the potential usefulness of using word association, it was decided to use word association as a non-intentional psi task. In line with Stanford (1973, 1977) an experiment was designed using homophones (words which are phonetically identical but have different meanings). The primary aim of this experiment was to establish whether or not a psi effect would appear given this protocol. If an effect was found, it would support the idea that psi could have an influence on a particular psychological process (i.e. word association). It was

hypothesised that a sender would be able to influence the participant's responses to homophones, in accordance with a randomly chosen target interpretation. There were two exploratory aspects to this study; the validity of matching each homophone pair for familiarity was to be determined (see below), as was any potential experimenter effects. In the present experiment there were two researchers taking on the roles of experimenter and sender. This was necessary as the equipment needed to be manually operated. Having two experimenters allowed one to manually operate the equipment while the other acted as a sender. It also permitted a post-session interview of the participant to be carried out by the experimenter who was blind to the targets. In accordance with previous research suggesting that different experimenters may influence the outcome of a session in different ways (see, e.g. White, 1977; Wiseman and Schlitz, 1997), it was decided to investigate in the present study whether any effect of experimenter was present. As the study progressed, a further hypothesis emerged: that responses which appeared to have a bias against them would result in a greater number of 'hits', in accordance with Stanford (1967).

Method

Overview

Participants were presented with homophones and asked to report the first associated word that came to mind. Each homophone had two predominant interpretations, and these interpretations were assigned to two lists. Before each session, one list was randomly designated the 'target' list. A sender viewed images relating to the target list, and to attempt to influence the associations of the participant accordingly.

Design

A mixed design was used in which participants' responses to homophones was compared to a "target" response (repeated measures). A between subjects "sender" factor was also incorporated in which one of two possible senders was randomly chosen for each session.

Participants.

Twenty unpaid participants were used. All were students or employees of the University of Edinburgh, and were known to the author (SW), ranging from work colleagues to students in his tutorial group. They were recruited through word of mouth, through e-mail appeals and through SW's tutorial group. Participants were told that they would be taking part in a study of word association, and at no point were they told of the parapsychological nature of the experiment. There were two experimenters, SW and Eric Pronto (EP), a visitor to the KPU who expressed interest in helping with any studies that were going on. Both experimenters participated in each session and assumed the role of either sender or experimenter during the course of the study.

All sessions were conducted in the Koestler Parapsychology Unit. The rooms used in the present experiment have been previously used in various Ganzfeld experiments, and are considered to be secure (see appendix 1; for a full discussion of the security issues involved in this set-up see Dalton *et al*, 1997).

Materials

Twenty homophone pairs (e.g. mail/male) were selected. These were chosen from a database on the basis that each pair was roughly matched for familiarity. The familiarity ratings were derived from merging three sets of familiarity norms: Pavio (unpublished), Toglia and Battig (1978) and Gilhooly and Logie (1980)¹. The method by which these three sets of norms were merged is described in Coltheart (1981). It was hoped that each interpretation thus had an equi-probable chance of being elicited on any one trial. Familiarity was chosen as opposed to other possible properties (e.g. concreteness) because this was the measure that showed the closest matches between pairs².

¹ Thanks to Dr. Martin Corley for providing me with these materials.

² See Gernsbacher (1984) for a justification of using familiarity as a measure.

Each homophone's interpretation was randomly assigned by computer to one of two lists (List A or List B) such that one list had a complete set of 20 interpretations, while the other list contained the alternative interpretations.

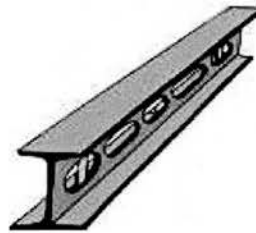
Homophones were recorded using the voice of Dr. Caroline Watt, a research fellow at the Koestler Parapsychology Unit. Words were recorded onto DAT (digital audiotape; Sony DTC A8 DAT recorder). Each word was recorded approximately 3 times, and the best one chosen by the author for inclusion as a stimulus. Further editing resulted in each word being preceded by 15 seconds of white noise, followed by a 5 second, quiet 'answer' period.

Visual cues relating to each meaning of the homophones were obtained from the Internet (via the image search at www.altavista.com), and were transformed into 'jpeg' files consisting of the image, and the word it related to. The images could either be illustrations or actual photographs. An example of the two images obtained for the homophone 'steal/steel' is given below.

Figure 5.1 Visual images relating to the two interpretations of homophone "steal/steel"



Steal



Steel

A computer program for the conduct of each session was written in Visual Basic and was designed in a way such that during the 15 seconds of white noise on the tape, the visual cue related to the forthcoming homophone was displayed on a 17" monitor viewed by the sender. The program also randomly selected List A or List B to be the 'target' list for the session in progress.

Procedure.

Before commencement of the session, a coin was tossed between SW and EP in order to determine who was to be 'sender' and 'experimenter' during the session. SW usually tossed the coin.

SW greeted the participant, and he then introduced EP as a co-experimenter. Whoever was designated 'sender' then guided the participant into a sound-attenuated experimental room, and asked them to sit in the chair, and make themselves comfortable. He explained what was going to happen during the session, and what the participant's task was. Participants were instructed to respond to each word that they heard with the first associated word that came to mind. They were not told that the words they would hear were homophones. The sender had the most interaction with the participant, so as to build up some kind of rapport. At no point was the parapsychological nature of the experiment made explicit. The participant clipped on a small microphone, and the recording levels were checked. The sender asked the participant if he/she had any further questions. He then asked how the participant would like the lights (dimmer/brighter). The participant was instructed to put the headphones on, and was told that the experiment would start shortly.

The sender retired to the 'sender's' room, which was approximately 25 metres away from where the participant was situated. The sender entered the room and closed the door (indicated by the ceasing of a flashing light in the experimenter's room showing when the sender's door had been closed). At this point, the experimenter (situated in a room adjacent to the participant) simultaneously started the computer program and the DAT recording.

The experimenter listened to what the participant was hearing in one ear of his headphones, while monitoring participant's responses in the other ear. Each response was noted down on a response sheet by the experimenter.

After completion of the trials, the experimenter went in to the participant's room with their responses. He then went through all the homophones, and the associated response with the participant. The participant was asked to clarify which (if any) of the alternative interpretations was meant by the response. This was conducted by presenting two words, each associated with one of the meanings, and asking which of the words was most like the response. For example, if the response to 'steel/steal' was "robbery", participants were asked if their response was more like 'metal' or like 'thief'. This served to clarify any ambiguities, and also to get an indication of which words were misheard etc. At this point, the experimenter was blind to what the target list was, and so could not influence responses. Ambiguities were, in fact, relatively rare, and on the majority of the trials where the word was correctly heard it was clear what the association was.

After this the participant was informed that the session was over. At this point the 'sender' had returned to the experimental suite. The participant was asked if he/she had any questions, which the experimenters would attempt to answer.

Once the participant had left, the experimenter checked the computer to see which list was the 'target' list, and scored each response according to whether it matched the target interpretation (a "hit"), matched the non-target interpretation (a "miss") or did not match either.

At no point during the session was the participant made aware of the psi nature of the task. It is possible, however, that some worked it out independently, given their knowledge of the parapsychology research unit, and SW's involvement with parapsychology.

Results

Any responses which were not one of the two meanings assigned to each homophone were discarded, as were occasions where no response was offered. This was conducted blind, before any other work was conducted on the responses. Of the 400 trials (20

subjects with 20 trials per subject), there were 109 which could not be used in the analyses. All results are, therefore, in terms of 'appropriate responses'. Because the computer randomly chose the 'target' list, the probability of any one response being a hit was 0.5.

Planned Comparisons

Out of 291 appropriate responses, there were 161 hits (55.3%) and 130 misses (44.7%).

These figures give a z-score of 1.82 ($p = 0.07$, two tailed).

Effect size was calculated, suggesting a small effect ($\phi = 0.1$)

Results were then split by sender. When SW was sending (and EP was the experimenter), there were 74 hits out of 125 (59.2%, $z = 2.05$ $p = 0.04$, two-tailed)

When EP was sending (and SW was the experimenter), there were 87 hits out of 166 (52.4%, $z = 0.62$, $p = 0.53$, two-tailed) (see table 5.1). The difference between the senders was not significant (chi-square = 1.33, $p = 0.25$).

Table 5.1. Hit rates (%), z-scores and effect sizes for all appropriate responses, and by sender (significant z value in bold).

	Overall	SW sending	EP sending
Score	161/291	74/125	87/166
%	55.3	59.2	52.4
Z	1.82	2.05	0.62
Effect Size	0.1	0.18	0.04

Post Hoc Response Bias Comparisons

As the study progressed, it became clear that the strategy of matching the interpretations for familiarity had not been successful. Several homophones were eliciting one type of response considerably more than the other. This led us to consider testing for a 'response bias' effect.

In 1967, Stanford suggested that psi-mediated impressions may come to the attention of the percipient because they somehow feel out of place in their context. Stanford proposes that the feeling "something terrible has happened to John" is more likely to be noticed or acted upon if it appears inappropriate in its context. On the other hand, if a person often thinks that 'something terrible has happened to John', (say, for example, if John has a dangerous job) then this feeling will not be unusual and will most likely be dismissed. In relation to laboratory ESP research, there are certain responses which are more or less favoured (e.g. in an experiment using Zener cards, the star tends to be the most popular choice, with the wavy lines the less popular). Stanford has suggested that responses with a low probability of occurrence may be more accurate than more popular responses, due to the extra 'push' of psi triggering a response against which a bias normally exists. Stanford has some experimental evidence for this effect (Stanford, 1967; Stanford, 1973), and, as mentioned previously, his 1973 experiment made use of homophones as stimuli.

During the course of the study, it became clear that certain responses were being elicited more than others, despite attempting to counter-balance the familiarity of the interpretations. This led to the possibility of testing for a potential "response bias" effect. The response bias hypothesis was formulated prior to statistical examination of the data, and before the study was complete, so in this respect, many of the shortcomings of performing post-hoc tests were avoided. In light of Stanford's reasoning, it was hypothesised that responses which were found to be the less favoured of the two alternatives would display a greater psi score than its more favoured counterpart. Out of all the minority responses (responses which were the less favoured of the two

interpretations for each homophone), it was found that, out of 70 minority responses, there were 43 hits (61.4%, $z = 1.91$, $p = 0.06$, two-tailed) (see table 5.2).

Table 5.2. Scoring for minority responses; overall scores and by sender (significant z values in bold).

	Overall	SW sending	EP sending
Score	43/70	21/30	22/40
%	61.4	70	55
Z	1.91	2.19	0.63
Effect Size	0.23	0.4	0.14

Again, data were broken down by sender, and it was found that when SW was sending there were 21 hits out of 30 minority responses, which is significant (70%, $z = 2.19$, $p = 0.03$, two-tailed). When EP was the sender there were 22 hits out of 40, which was nonsignificant (55% $z = 0.63$, $p = 0.53$, two-tailed) (see table 2). Again, the difference between the senders was not found to be significant (chi-square = 1.63, $p = 0.20$).

Stacking Effect – a possible artefact?³

Technical limitations in the current study meant that the order the homophones were presented in could not be properly randomised. This meant that each homophone interpretation was randomly assigned to a target list (A or B) and each participant was randomly assigned a target list at the outset of the trials. As a result of this technique, the targets were not independent of each other, with half of the participants receiving the same target order, while the other half received the alternative target order. Situations in

³ I am grateful to Rex Stanford for highlighting the limitations of the randomisation used in the current study, and to Niko Tilliopolous for his statistical help in addressing the problems.

which participants in an ESP test call from the same target order have been criticised as being vulnerable to what is known as the “stacking effect” (see, e.g. Burdick, 1977). The stacking effect occurs when participants share a particular response bias. This commonality in responding between participants can lead to spuriously high (or low) correlations between target and responses. This is due to the subjects sharing the same calling bias, which may coincide with (or deviate from) the target order (Thalbourne, 1982). In other words, the stacking effect assumes that respondents may share common “guessing habits”. Grimmer and White (1986) demonstrated the presence of such habits in an Australian sample. They reported that, when participants were asked to guess which two geometrical shapes the experimenter was thinking of, the circle-square guess sequence appeared more frequently than the circle-triangle one.

Naturally, when the target order is random and unique for each respondent, this guessing bias can safely be ignored, since its effect is counterbalanced by the design (especially when the target items have equal probabilities of occurrence). However, if respondents are guessing from a single target sequence, and if they share a common response bias, then the results cannot be thought to be independent from each other (for example, if the circle-square sequence is consistently included in the target sequence, then the tendency to call “circle” followed by “square” will result in hits that are not due to psi, but are an artefact of that particular response bias). In the latter case, the potential increase in the degree of agreement between the results can produce a reduction of the variance within each target and consequently it may have a substantial, yet spurious effect (a stacking effect) on the statistical significance of the tests employed.

Several procedures have been proposed to account for the presence of the stacking effect, such as the Greville formulas (Greville, 1944; Pratt, 1954), the majority-vote technique, or the index of preference (for an overview see Thouless and Brier, 1970; Burdick, 1983). However, these techniques make the assumption that the stacking effect is inevitably present in all cases in which the respondents share a target order. As such, the methods advocated by previous authors have consisted of manipulating the variance

estimates and thus making the tests more conservative. However, it is not necessarily the case that the stacking effect becomes manifest whenever single target orders are used, and it may be possible to detect situations in which there is actually a shared response pattern for the participants in the study. Such a technique would be more revealing as it would determine whether the stacking effect should be taken seriously in a particular study. The following method was developed for assessing any potential stacking effect in data-sets using a fixed target sequence. This represents a significant new technique that may be of use to other researchers, and as such, it is described in detail here.

The present study poses a novel version of the stacking effect due to the existence of two sequences (lists A and B). However, the fact that two lists are used gives more scope for dealing with any potential artefacts.

Given that the effect may or may not be present in designs such as the current one, the main question becomes one of determining whether or not systematic response patterns were present in the data. In order to answer this, a number of testable hypotheses were generated, which collectively can assess the presence and the magnitude of any potential the stacking effect in the data.

If there is a stacking effect in the data then we would expect that there will be a pattern (a calling bias) in the response sequence for each subject. As we are interested in a shared calling bias, then we could extend the above supposition and suggest that subjects will not only tend to exhibit similar and systematic patterns of responses, but also the number of random sequences of their responses will be minimal. Consequently, in the presence of the stacking effect we would expect respondents to form very few groups of similar response patterns. The reason for this is as follows. Suppose everyone in the group shared a common response pattern, throughout the sequence of trials (i.e. a classic stacking effect). This would lead to a response-pattern cluster of one. However, if there were fewer similarities in the response patterns, then the number of clusters of similar patterns should increase.

In addition, if respondents tend to produce the same guesses within each target, we would expect each target to show a rather small variability in the responses.

Analyses

The first issue to address was whether or not the target lists differed from each other. If one particular list was related to a particular calling bias, then it would lead to this list displaying spuriously high (or low) “hits” in relation to the other list. This effect was analysed and it was found that there was no difference between the lists in terms of the proportion of hits to misses ($t(18) = 0.09, p = 0.93$). Thus, it can be argued that any potential psi effect was not due to one list leading to more hits or misses either through a stacking effect or some other artefact.

Secondly, more detailed analyses were carried out to determine whether any response patterning existed between the participants in the study.

A *Q*-sort runs test of randomness was employed to test for individual subject’s patterns. The runs test assesses the degree of randomness in a sequence of individual responses (Siegel and Castellan, 1988). A *run* is defined as an array of identical responses, which is preceded or followed by a different kind of responses or no responses. If a sequence of individual responses has too many or too few runs, in respect to the total number of targets, then responses cannot be thought to be random, and a systematic bias is assumed to be present. The results of the subject-wise runs test are presented in Table 5.3.

Table 5.3: The runs test results for each of the 20 subjects. Participants in grey cells belong to the first target list, while all others received the second list; the mean was used as a target value (though its meaning in this case is arbitrary), since it appeared to produced the most balanced results.

Subjects	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Test Value ^a	2.05	2.05	1.90	1.67	1.76	2.14	1.57	2.19	2.00	2.05	2.10	2.33	1.71	1.62	1.57	1.90	2.19	1.71	2.05	1.90
Cases < Test Value	14	12	8	10	10	13	11	12	8	13	15	10	9	12	12	9	11	11	14	10
Cases >= Test Value	7	9	13	11	11	8	10	9	13	8	6	11	12	9	9	12	10	10	7	11
Total Cases	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21	21
Number of Runs	10	11	8	12	6	12	16	12	9	11	7	11	11	10	10	9	15	11	7	6
Z	.000	.000	-1.145	.011	-2.234	.283	1.806	.098	-.669	.000	-1.150	.000	.000	-.360	-.360	-.817	1.357	.000	-1.437	-2.234
Asymp. Sig. (2-tailed)	1.000	1.000	.252	.981	.026	.777	.071	.922	.504	1.000	.250	1.000	1.000	.719	.719	.414	.175	1.000	.151	.026
Exact Sig. (2-tailed)	1.000	1.000	.233	.835	.022	.642	.047	.827	.479	1.000	.187	1.000	1.000	.651	.651	.362	.120	1.000	.122	.022

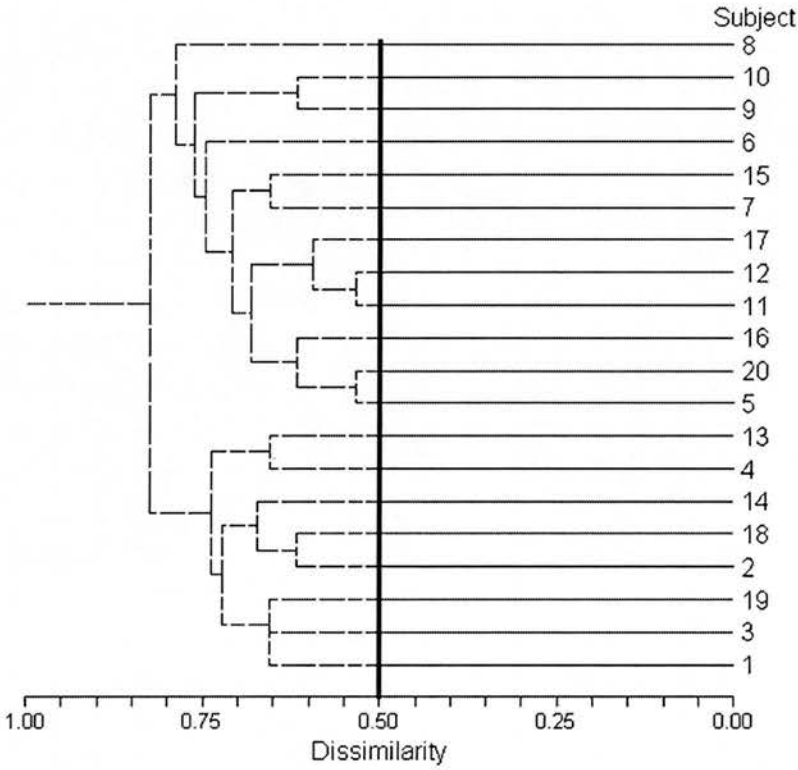
^a. Mean

As can be seen, of the twenty participants only three appear to exhibit some sort of calling bias (Ps 5, 7, and 20). Since all three participants had taken the same list (List 2) it was possible to check whether their individual biases had a common thread. In order to do so, we treated these participants as rating an item on the same scale and thus we employed Cohen's *Kappa* to check for their pairwise inter-rater agreement. In this case, a high *kappa* would imply that participants possessed to a degree a common calling bias. The test results suggest that only participant 5 and 20 appear to share such a calling bias ($Kappa = .55, p < .0005$). Looking more closely at participants 5 and 20, they were both female, and both scored above chance on the psi task (10 hits out of 16 appropriate responses for participant 5, 10 hits out of 13 appropriate responses for participant 20). Participant 5 was British, while participant 20 was American. They did not know each other prior to the experiment. The sender for both participants was SW.

Staying within the framework of *Q*-sorting, an agglomerative hierarchical clustering method was used to test whether participants formed groups (clusters) based on proximities in their responses, irrespectively of whether individual responses were random or systematic. The additional information gained from this technique is that, unlike the runs test, which checks for patterns within each participant independently, clustering methods looks at responses across subjects and classifies individuals into groups of similar responses. If participants tended to share a guessing bias that the runs test failed to identify (because perhaps it was not systematic within each subject, but between the subjects), one would expect through cluster analysis to see very few and distinct groups of participants formed rather early in the clustering process. The results of the hierarchical cluster analysis using unweighted group average distances (cophenetic correlation, $r = 0.76$; Everitt, 1998) with Euclidean distances as a proximity measure, and a cut-off point at $delta_{(.5)} = 0.05$ (Mather, 1976) clearly show a total absence of any grouping among the participants. This is shown graphically in figure 5.2. In this figure, the further apart the participants are vertically, then the fewer commonalities in responding they share. Thus, participants 1 and 8 have the less in common, as they are furthest apart in the figure. Working from the right to the left,

clusters can be seen when parallel lines come to an end. For example, participants 1 and 3 form a cluster at approximately 0.65 level of dissimilarity, which is above the cut off point of 0.5 (although this cut off point is arbitrary to some extent). It can be seen that no clusters are formed before this cut-off point, suggesting that the pattern of participant's responses do not form cohesive groups. Interestingly, the first cluster to form is indeed between subjects 5 and 20, although it is still above the optimum cutoff point of reference.

Figure 5.2: Hierarchical representation of 20 participants based on the proximity of their responses on 21 targets; vertical line demotes the optimum cut-off point



Finally, in order to assess whether respondents tended to produce the same guesses within each target, once again the runs test was used, this time in an *R*-sorting

configuration. It was predicted that, if participants tended to give similar responses to each target, targets would independently show a clear pattern of responses. Also, it was expected that if participants showed similar response patterns to each target, the variability within each target would be minimal, resulting in a significantly low number of runs (as defined earlier) being observed. The analysis was executed separately for each list, and the results (Tables 5.4a and 5.4b) do not seem to suggest the presence of such tendency in any of the targets.

Tables 5.4a and 5.4b The results of the runs test presented separately for each of the two lists respectively.

LIST 1	TRIAL1	TRIAL2	TRIAL3	TRIAL4	TRIAL5	TRIAL6	TRIAL7	TRIAL8	TRIAL9	TRIAL10	TRIAL11	TRIAL12	TRIAL13	TRIAL14	TRIAL15	TRIAL16	TRIAL17	TRIAL18	TRIAL19	TRIAL20	TRIAL21
Test Value ^a	2.3000	1.5000	2.0000	1.9000	2.4000	1.8000	2.0000	1.8000	2.2000	2.0000	1.4000	1.2000	1.9000	1.8000	2.8000	2.1000	1.6000	1.4000	1.1000	2.3000	2.2000
Cases < Test Value	6	6	2	4	3	5	2	2	8	4	7	8	4	6	1	6	5	6	9	5	5
Cases >= Test Value	4	4	8	6	7	5	8	8	2	6	3	2	6	4	9	4	5	4	1	5	5
Total Cases	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Number of Runs	4	5	3	6	5	6	5	4	3	7	5	5	6	7	3	5	5	4	3	6	7
Z	-.913	-.211	-.791	.000	.000	.000	.339	.000	-.791	.492	.000	.339	.000	.492	.000	-.211	-.335	-.913	.000	.000	.335
Asymp. Sig. (2-tailed)	.361	.833	.429	1.000	1.000	1.000	.734	1.000	.429	.623	1.000	.734	1.000	.623	1.000	.833	.737	.361	1.000	1.000	.737
Exact Sig. (2-tailed)	.310	.714	.222	1.000	1.000	1.000	.689	1.000	.222	.500	1.000	.689	1.000	.500	1.000	.714	.714	.310	1.000	1.000	.714

a. Mean

LIST 2	TRIAL1	TRIAL2	TRIAL3	TRIAL4	TRIAL5	TRIAL6	TRIAL7	TRIAL8	TRIAL9	TRIAL10	TRIAL11	TRIAL12	TRIAL13	TRIAL14	TRIAL15	TRIAL16	TRIAL17	TRIAL18	TRIAL19	TRIAL20	TRIAL21
Test Value ^a	2.0000	2.0000	1.2000	1.7000	2.5000	2.1000	1.2000	1.7000	1.2000	2.1000	2.2000	1.7000	2.1000	2.1000	2.9000	1.7000	1.8000	1.9000	1.9000	2.6000	2.5000
Cases < Test Value	5	1	9	6	4	7	9	6	9	5	8	3	5	7	1	5	4	4	1	2	4
Cases >= Test Value	5	9	1	4	6	3	1	4	1	5	2	7	5	3	9	5	6	6	9	8	6
Total Cases	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Number of Runs	5	2	3	5	6	5	3	7	3	5	4	5	5	6	3	3	7	7	3	5	6
Z	-.335	-.750	.000	-.211	.000	.000	.000	.492	.000	-.335	.000	.000	-.335	.245	.000	-1.677	.492	.492	.000	.339	.000
Asymp. Sig. (2-tailed)	.737	.453	1.000	.833	1.000	1.000	1.000	.623	1.000	.737	1.000	1.000	.737	.806	1.000	.094	.623	.623	1.000	.734	1.000
Exact Sig. (2-tailed)	.714	.200	1.000	.714	1.000	1.000	1.000	.500	1.000	.714	1.000	1.000	.714	.700	1.000	.079	.500	.500	1.000	.689	1.000

a. Mean

Summing up the findings, it appears that seventeen participants gave non-systematic independent responses to the targets, and, of the three subjects who seem to show some sort of individual response bias, only two appeared to be sharing it to any extent. In addition, the variability of responses within each target appears to be within acceptable levels, suggesting that participants gave diverse enough answers in each target. Combined the above results seem to suggest that the stacking effect is not present in the data, or if it is, it is insignificant and thus its influence on the study results can be confidently ignored.

These findings and the methods used to assess the stacking effect are innovative and suggest that the problem of the stacking effect can be re-assessed using methods that are more sensitive to the effect under study. Where the previous methods have failed to actually account for whether a stacking effect may be present in the data, the current technique assesses the extent to which response patterns are present in the data and the effect they might have. This in itself represents a potential step forward in the statistical methods used within parapsychology, and calls into question the validity of previous criticisms of studies using fixed target sequences.

Discussion for Experiment 1a

Before discussing the actual results, it is worth noting that many subjects had difficulty comprehending certain stimuli. Caroline Watt was chosen to record the stimuli as it was felt she had a voice which was easy to understand. Due to the diversity of subjects used in this experiment, it is possible that regional accents may have caused some subjects to misunderstand what word was being presented. Using a computer to generate the verbal stimuli was considered, but none of the programs considered sounded natural, and this would likely have caused more confusion than using a human voice.

The main hypothesis in this experiment was that interpretations of homophones would be influenced by a sender viewing a randomly chosen image relating to one of the

interpretations. Overall, a hit rate of 55.3% was achieved. This is not significant, but is in the predicted direction and must be considered encouraging.

When these data were analysed by sender, it was found that, when SW was sending, there were more hits than would be expected by chance (59.2%), while when EP was sending, hits were close to chance (52.4%). This looks like an experimenter effect (see, e.g. Wiseman and Schlitz 1997). However, if it is a real effect, it is difficult to see where its origins may lie. Wiseman and Schlitz offer a number of suggestions concerning the basis of this effect. One suggestion is that the effect may have its basis in the prior expectations of each experimenter. This would not seem to apply to the present experiment, as there was no clear sceptic/proponent divide. Furthermore, it is impossible to establish from the present experiment whether the effect may be due to the sender or the experimenter. It is possible to speculate that the effect was due to the sender, based on the fact that a large proportion of participants were known to SW, and not to EP. The remainder were known to both experimenters, but probably better known to SW.

It is, perhaps, premature to speculate further on this effect without further studies, although the experimenter effect, if it is genuine, has far-reaching implications for both the field of parapsychology and the practice of science in general (see, e.g. Rosenthal, 1966; 1980).

During the course of this study, it became clear that matching homophones for familiarity did not have the desired effect of making them equally biased. This was part of the exploratory nature of the experiment and was not in itself disappointing. This is most probably due to other factors associated with the homophones. For example, some homophones, although well matched on familiarity, may have differed on factors such as 'concreteness' and this may have influenced which one was favoured.

This failure to make the homophone pairs equi-bias inadvertently gave us a chance to study any 'response bias' effects. When data were separated into 'less favoured' and 'most

favoured' responses, it was found that the 'less favoured' responses elicited more hits (61.4%). This only just falls short of significance, but once again is encouraging as it lies above chance, and is suggestive of an effect. The response bias effect was observed by Stanford in 1974, using a word association paradigm similar to the one employed in the present experiment. Although this was not an original hypothesis, it is interesting that the effect has emerged in a study so similar to Stanford's. Stanford, however, failed to replicate this effect with word association in 1977, but has reported the effect a number of times using different kinds of ESP tasks, suggesting that it may be a robust effect (Stanford, 1967; 1970). Although this hypothesis is essentially post-hoc, it did emerge at quite an early stage during the study, and the prediction was made before any data was analysed. Despite this, the effect should be treated with a degree of caution, until successfully replicated.

It is also interesting to note that the experimenter effect is also present in the response-bias data (although it should be noted that the response bias data is not independent of the overall psi data). When SW was sending, there were more hits on the 'less favoured' responses than would be predicted by chance, while when EP was sending, performance was again close to chance levels (70% v 55% respectively). It should be noted that SW's psi-hitting scores increased by about 10% when compared to the overall data, while EP's psi-hitting scores increased by about 3%. This suggests that, if anything, the psi effect is stronger in the less-favoured responses. Caution should be taken, however, when drawing any conclusions based on such a small number of trials, and it must also be taken into account that there were no statistically significant differences between the senders when their data were compared.

It is important to point out the limitations of the present experiment. Due to the exploratory nature of this study, the response bias findings were post-hoc. These must be treated with caution, and replicated if they are to be considered real effects. Secondly, it is possible that the way 'less-favoured' responses was operationalised could be open to question. All responses for each homophone were collated it was then determined how

many for each interpretation there were. The one with the less number of responses was designated the 'less-favoured' response, and the author then went through all the less-favoured responses, counting how many were hits. It could be argued that established word association norms should have been used when doing this. As it stands, the sample group itself was used to establish what these 'norms' were, and this may have been erroneous for one reason or another. However, in his 1973 study, Stanford found that his subject population did not conform to the established norms he was using, perhaps suggesting that each subject population should be considered in isolation. An alternative is to attempt to determine what biases exist in each individual participant, or to manipulate this in some way. That would eliminate the possibility that the response bias effect was due to individual idiosyncrasies in responses.

In conclusion, this experiment has been informative in a number of ways. Firstly, the results are encouraging in that they seem to suggest that psi may have an influence on ongoing associative processes. This is promising, as it may give an insight into one of the various ways which psi might manifest itself in everyday life. Secondly, this work does seem to suggest that less common responses may be better vehicles for psi than more common responses. This needs to be replicated if it is to be considered valid. Finally, data suggested that an experimenter (or sender) effect may have contributed to the results, although why this may be the case is unclear.

Experiment 1b – Auditory PWA and word association

Introduction

The results of experiment 1a suggested that word association may be a useful means for testing unconscious psi. Experiment 1b was an attempt to elicit a comparable effect in the area of PWA. If psi is comparable to PWA in terms of the way the information is processed, then they both should influence certain tasks in similar ways. Experiment 1a suggested that a weak, unconscious psi stimulus could, to some extent, influence word association. Experiment 1b aims to re-create this effect, using the influence of a weak

sensory stimulus. Such a comparison would lend to support to the hypothesis that psi processing is unconscious in nature, sharing similarities with other unconscious phenomena.

As the stimuli used in experiment 1a were homophones (and could not, therefore, be presented visually) the method of stimulus presentation in experiment 1b had to be via auditory means. As we have seen in chapter 3, the field of auditory PWA is controversial and complex. As a result, certain innovations were necessary in order to create adequate stimuli. Some of the techniques employed vary from the suggested methods of previous authors (e.g. Urban, 1993; Ott and Curio, 1999). The work of Ott and Curio (1999) represents the most sophisticated attempt to create auditory PWA stimuli, addressing many of the key issues raised by Urban (1992). While every effort was made to follow the suggestions of Ott and Curio, it was frequently found that their techniques were impossible to implement given the equipment available. For example, Ott and Curio, (1999) modified software initially designed to collect physiological data in order to create auditory PWA stimuli. However, this software was extremely expensive and would require extensive re-programming were we to use it for our purposes. Additionally, it was in German, giving us further reason to seek an alternative method. The author then contacted Ott, enquiring about the best way to create satisfactory stimuli. Ott kindly offered to use the software that he had modified, in order to manipulate the stimuli that had already been created for experiment 1b. However, shortly after sending the stimuli to Ott, he left the field, and did not continue correspondence. This meant that a certain amount of improvisation was necessary in order to create the stimuli. In collaboration with Ben Schogler of the Psychology Department, University of Edinburgh, we endeavoured to create a new way of creating auditory PWA stimuli that stayed as faithful as possible to the techniques previously recommended. We gained access to state-of-the-art audio recording and processing equipment, allowing us to manipulate the stimuli in a number of ways. Although the final method used is different to Ott and Curio's suggestions, it is our belief that the method we developed has its own strengths that the previous methods did not. Likewise,

it has its own weaknesses. Thus, a major factor in this experiment was determining the efficacy of our new auditory masking technique.

Method

Overview

Experiment 1b was conceived to investigate the effect of an unconsciously perceived stimulus on subsequent word association. In experiment 1a, psi was assumed to be inherently unconscious in nature. Conceptualising psi in this way means that many of the problems concerning demonstrations of awareness/unawareness are avoided by virtue of the way psi is defined. When dealing with sensory stimuli, however, it is important to be sensitive to the issues raised in chapter 3 concerning what is and is not “unconscious”. Unlike the form of psi being investigated in the current thesis, sensory stimuli cannot be thought of as being unconscious by definition. What makes a sensory stimulus unconscious or not is the way in which it is presented to the participants. Even then, it is essential to incorporate into the design a means by which this can be evaluated, indicating whether the information has been perceived consciously or unconsciously. The following experiment follows much of the current thinking in PWA research (see chapter 3) and places perception with and without awareness in direct opposition to each other, while making predictions about the respective outcomes.

This experiment uses an ‘exclusion’ task to test for unconscious influences. As previously mentioned in chapter 3, current thinking in PWA research makes the assumption that both conscious and unconscious influences play a part in most tasks, but under certain conditions they can be separated out. An exclusion task can do this. In exclusion tasks, participants are asked to actively avoid using a particular stimulus when responding. This is assumed to be under conscious control. However, at a certain ‘critical’ level (for example if the stimulus they are asked to avoid has been degraded) then they should stop being able to exclude the stimulus, and actually include it when responding. This is measured in relation to a baseline condition. If it can be shown that, relative to the baseline, participants are using the stimulus they have been asked to avoid

more often, then it can be said that the stimulus is influencing them at an unconscious level. In the context of the current experiment, the exclusion task was particularly effective as a means to measure ‘true’ perception without awareness. In the field of auditory perception without awareness, there is always the possibility that words that are assumed to be outside of awareness may actually be consciously identified due to some aspect of the masking procedure. While this was addressed as much as possible in the technical preparation of the stimuli, the exclusion task itself gave a further safeguard. This is because, if any part of the word at all was consciously identified, then this gives a strong clue when the response alternatives are presented, making exclusion relatively easy. Thus exclusion failure can only occur when a stimulus has definitely not been consciously identified (see below for a fuller explanation of the task). This procedure essentially makes the test for PWA more conservative than the majority of previous auditory PWA studies. As such, the current method is less vulnerable to artefactual effects caused by partial conscious awareness of the stimuli.

The current experiment was unique in some respects. While the exclusion task has been used extensively in studies of visual perception without awareness (see, e.g. Merikle and Joordens, 1997), this study is only the second to employ it in an auditory experiment. Ott *et al* (2000) have previously used an exclusion task in an auditory perception without awareness experiment, but they used it in conjunction with an inclusion task (essentially the inverse of the exclusion task) as part of the process-dissociation approach to measuring unconscious influence (see chapter 3). As previously outlined, using the exclusion task as a stand-alone measure allows us to estimate the relative magnitude of any unconscious influence that might be present in the task (as suggested by Merikle and Joordens, 1995).

Creating the stimuli - Technical Details

The main problem with pursuing this kind of research is in masking the stimulus to a satisfactory degree. “Masking” is the process by which the audibility of one sound is reduced due to the presence of another at a close frequency. This primarily depends on

acoustical energy and the frequency range associated with it. Increasing the frequency bandwidth of a masking noise, with energy held constantly to the corresponding signal frequencies band, gradually causes more masking until a certain point is reached beyond which masking does not increase. This bandwidth is known as the “critical” bandwidth, and the set of critical bandwidths is assumed to be continuous for the frequency range from 20Hz to 20 kHz (i.e. the range of human auditory perception – See Yost, 1994 for a fuller account of the psychoacoustics involved in masking).

It has been noted by previous authors (e.g. Urban, 1992) that the most desirable way to achieve auditory masking is to have a constant differential between the acoustic properties of the word and the white noise. In other words, if it has been decided that a signal/noise ratio of 17db is to be used, then it is important that the white noise is 17db above the word *at every point* during the duration of the word.

Both Urban (1992) and Ott and Curio (1999) suggest tracking the variation in stimulus intensity across the range of frequencies in the complex sound (i.e. spoken word) and adjusting the intensity within each of the “critical bandwidths” accordingly, to maintain the appropriate signal to noise ratio.

The crucial problem faced when attempting to mask stimuli for use in a PWA study is the accurate tracking and measurement of the intensity across the 24 critical bands (Ott and Curio, 1999). The process suggested by previous authors utilises a low pass filter to generate an approximation of how the intensity values change over time in reference to the frequency bands. The corresponding adjustments are then made in intensity to the signal to be masked. In this method, the tracking procedure leads to continual adjustment of the levels within the desired signal to noise ratio. Consequently, the specific ratios across frequency bands may vary. Additionally the low pass filter is producing an 'approximation' of variation which leads to a further problem. This is due to the fact that the noise tracking the stimulus might vary in such a way that gives cues as to what the stimulus actually is. Ott and Curio introduced “time weighting” parameters to try and

counteract this criticism. This time weighting was designed as a way to eliminate the cue that real-time tracking might offer. With this tracking procedure, it is necessary to account for temporal masking as well, ensuring the levels are being altered at the same time as the levels are changing.

After much deliberation, we decided that the most convenient approach would be to reduce the variation of each word as much as possible, and then mask it at the desired signal to noise ratio. This is a different method to letting the word vary and adjusting the white noise to match. Technical details of how we achieved this are given below.

Through the use of the Pro Tools digital audio workstation⁴, a frequency sensitive masking procedure was developed which allows the ratio between 'white-noise' and 'stimulus' to be monitored and adjusted within each of the 20 "critical bands" (see Ott and Curio, 1999 for a discussion on the psychoacoustics behind auditory masking).

All audio material was digitally transferred from DAT (digital audio tape) to a Pro-tools Mix Plus digital workstation run on an Apple-Mac (G3-233mhz processor)-incorporating a digi-design 888 d/a interface. All words were then trimmed to remove extraneous variables such as breath sounds and any environmental sounds that had bled across into the recordings.

Initially all stimuli (both target words and both sets of primes) were normalised to -2.5db. This process involves an automatic analysis of the maximum peak intensity value across all frequency bands and corresponding adjustment in overall intensity - either boosting the overall level if below -2.5db or a reduction if above. (It is important to note at this stage that all procedures performed on primer stimuli were also applied to the target stimuli to allow accurate interpretation of the ratios produced).

Although this provides a comparable overall decibel level for both target and primer stimuli, the frequencies that make up each word still peak at differing values

In the present study, a certain degree of experimentation was required to ascertain the best method of producing accurate signal to noise ratios. Current music technology involves a great deal of treatment for the voice but the majority of this centres on making the vocal more intelligible and avoids masking. In standard mixing, in order to gain maximum control over different signals the technique of compression is used to reduce the variation within the signal and provide a more coherent source⁵. Through investigating this technique it was found that multi-band compressors could be used to essentially normalise a signal across the critical bands. In simpler terms, instead of letting the signal (i.e. voice) vary within each critical band, and then adjust the white noise accordingly, we were attempting to “flatten” out the signal as much as possible, reducing the variation to a minimum. Then we could mask within the critical bands knowing that there was as little variation as possible.

The multi-band compressor⁶ works in real-time to normalise the db peak of the waveform within the frequency bands determined by the user (in this study the critical bands were as follows: 0-250, 250-782, 782-2361 hz). Within these bands a level can be set and the compressor adjusts the peak level within the frequency bands to produce a signal that peaks at the same level within all frequency bands. Therefore setting a peak level of 0db across all frequency bands produces a stimulus whose level is tracked in real time and normalised to a constant decibel level, in a manner which is sensitive to the constituent frequency make up of that signal.

⁴ This package is generally considered to be the industry standard for digital audio production (Schogler, 2001; personal communication).

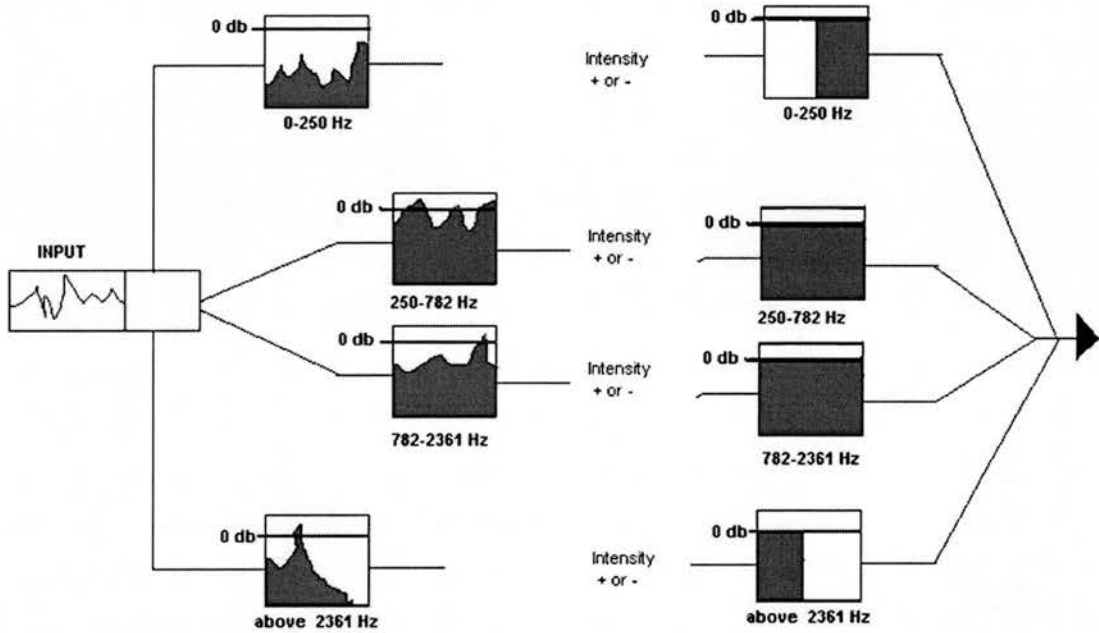
⁵ Compression is the technique whereby a noise source is kept within certain parameters set by the user. Therefore weak signals are boosted and brought above a lower limit, while loud signals are reduced and brought below an upper limit.

⁶ The compressor used was a C4multiband parametric processor 48 bit double precision , by WAVES.

Once processed in this manner both stimulus and target word can be seen to produce the same intensity levels. The ratios for masking between a white noise signal set at 0db and a stimulus level adjusted by the experimenter can now be recorded digitally within the computer. At this stage the majority of the variation within the words has been tracked and removed; thus the problem of adjusting the ratio in real-time is consequently removed. This gives a higher degree of confidence in the ratios produced and a finer control over them.

In the low pass filter tracking method advocated by Ott and Curio (1999), there will always be the issue of adjusting the ratio levels within the critical bands in real-time, leading to the issue of whether the masking is effective or perhaps slightly delayed. Other experiments have recorded the variation and then placed the masking variation ahead of the stimulus. This however, involves issues of temporal masking to be considered and makes the process more complicated. This alternative method of identifying the variation within various frequency bands and systematically removing it through compression across those frequency bands (standardising the signal) is a simpler approach, utilising the current digital technology available to us. The work done on compression techniques to allow greater control of signals relates directly to the present experiment, and it seemed that these techniques might offer an alternative to the methods previously suggested. A diagrammatic representation of the technique used in this study can be seen in figure 5.3 below.

Figure 5.3 – Graphical illustration of stimulus manipulation



Design

A repeated measures design was employed, with participants' ability to successfully perform an "exclusion" task (see below) being measured in a number of conditions. Reaction times were also measured in each condition. The conditions were as follows:

1. Baseline condition – whereby there was no actual priming word hidden in the white noise. This gave an indication of the degree to which exclusion failure would occur by chance alone.
2. Control condition – in which priming words were not masked by any white noise. This allowed for an assessment of participants' ability to follow exclusion instructions when there was no masking stimulus obscuring the cue word.
3. PWA trials – in which the cue words were masked by a number of different signal/noise ratios.

Procedure

Participants were greeted by the experimenter and shown into the experimental room. They were told to make themselves comfortable before the experimenter explained the task.

The exclusion task

The exclusion task involved presenting subjects with 5 seconds of white noise, followed by a homophone. In certain conditions, the white noise masked a 'priming' word that was related to the homophone (e.g. 'letter' may have been used to prime the homophone 'mail'). The signal to noise ratio (i.e. the amount by which the noise masks the word) varied, and was one of several ratios recorded for this experiment. There was also a control condition (in which the priming word is not masked), and a baseline condition (in which there is no primer present underneath the noise).

Participants were instructed that, in most trials, they would hear five seconds of white noise followed by a homophone. They were told that under the white noise might be a word that related to one of the meanings of the homophone, and were asked to try to attend to this if they could. After they heard the homophone, the participants were told that two alternatives would appear on the screen. One would be the word which was masked, one would not be. They were asked to respond to the word that they did *not* hear (i.e. exclude from their response the word they *did* hear). It was also pointed out that a control condition was to be used in which there was no white noise, and only the priming word, followed by the homophone. It was explained that, due to the masking, the words would be difficult to recognise consciously, but may be recognised through clues such as the number of syllables present or the length of the word. In cases where the participants felt that they had heard nothing at all under the white noise, they were instructed to make a guess, based on the first association that came to mind after hearing the homophone.

After the experimenter had explained this task, he asked the participant if they had any questions. Most enquiries at this stage involved the exclusion task, as participants clarified that they were to respond to the word that they had not heard. Once the participant fully understood the task, the experimenter retired to the experimental room and started the trials. For each trial, the computer would randomly select a homophone and one of the two primer words related to that homophone. In addition to this, the computer would randomly select what condition each trial was to belong to (whether it was one of the pre-defined signal to noise ratios, the baseline condition, or the control condition).

Once testing was complete, the number of exclusion failures in each condition (trials in which participants responded with the word that was masked, as opposed to excluding that word from their response) was tallied. Any participant who scored 3 or more exclusion failures in the control condition was removed from the analysis (N= 1 in the pilot study and 5 in the main study).

Materials and Apparatus

Forty-four homophones were recorded in addition to 2 priming words for each homophone. The priming words were masked with various levels of white noise, using the methods described above. A computer program was written in Visual Basic that presented the stimuli to the participants, and saved responses. A button box was used with two buttons, labelled A and B. This was used by participants when they made responses. All sound files were in .wav format and were transferred digitally to the computer. Stimuli were presented through Koss SB30 headphones. The soundcard used was a Creative Soundblaster Live card.

Pilot study

A pilot study was conducted in order to identify the most promising signal/noise ratio for a PWA effect. The ratios used were -13db, -17db, -17.5db and -18db. These were in addition to the control condition and the baseline condition. These ratios were chosen as a recent study (Ott *et al*, 2000), using slightly different masking techniques and a different methodology, had found an unconscious priming effect using a ratio of -17 db.

In the pilot study, there were 44 homophones used. For each trial the computer would randomly select which homophone and related priming word was to be used, in addition to randomly allocating a condition for each trial.

Participants

Fourteen Edinburgh University undergraduates were recruited through e-mail appeals. There were 6 males and 8 females, ages ranging from 18 to 29 years. All participants had normal or corrected to normal vision and did not suffer from either dyslexia or hearing impairments.

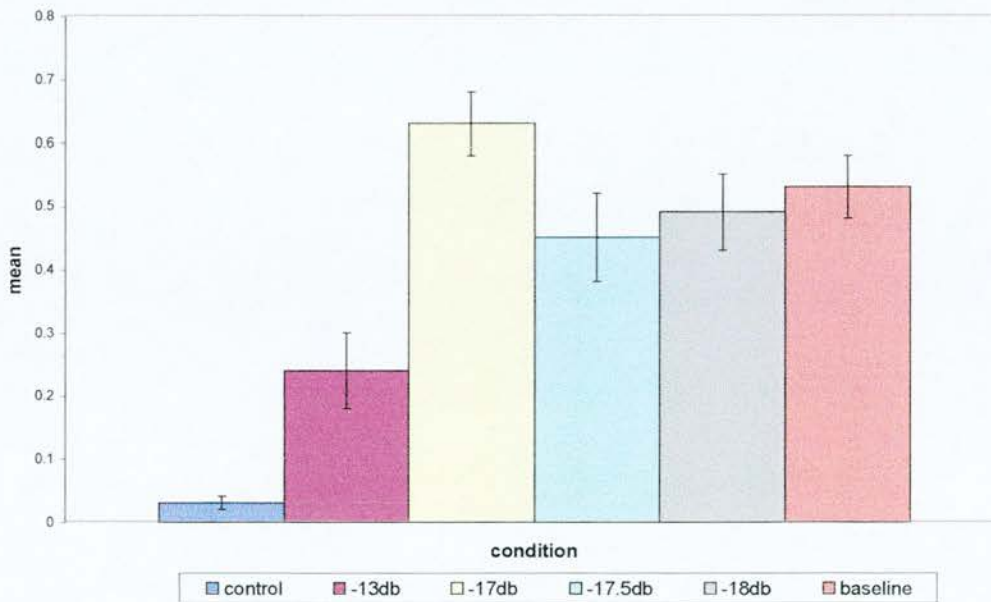
It was hypothesised that the ratio in which perception without awareness functions most efficiently would show a higher proportion of exclusion failures than the baseline condition. In other words, it was hoped that, at a critical ratio, participants would fail to respond to the word they did *not* hear, and would tend to be influenced by the unconscious influence of the masked priming word. This is in comparison with the baseline condition, which is in place to give a measure of the proportion of exclusion failures that occur when there is *no* priming word present at all. If perception without awareness is happening at one of the ratios, then it is expected that the rate of exclusion failure will be significantly higher than the baseline condition.

Pilot Results

The randomisation process resulted in the number of trials in each condition sometimes being different. As a result of this, for each participant a proportion was calculated for each condition based on the number of exclusion failures in each condition divided by the total number of trials in that condition. It was this proportion that the main analysis was conducted on.

Figure 5.4 illustrates the number of exclusion failures at each signal to noise ratio. There appears to be a slight peak at -17db . The control condition indicates that participants were able to follow instructions more or less perfectly, and there was indeed a significant difference between the control condition and the baseline condition ($t(13) = 9.44, p < 0.001$, one-tailed).

Figure 5.4: Mean proportion of exclusion failures at varying db levels (+ error)



An analysis of variance revealed a significant effect ($F(4, 52) = 6.13, p < 0.001$). Further investigation of this effect revealed that it was due to the -13db condition being significantly *below* baseline ($t(13) = 3.42, p = 0.0003$, one tailed). The -13db condition

must also be considered as allowing for conscious awareness of the priming word, due to the high level of exclusion success.

None of the other conditions were significantly different from baseline, as can be seen below.

-17db – $t(13) = 1.39, p = 0.09$, one-tailed.

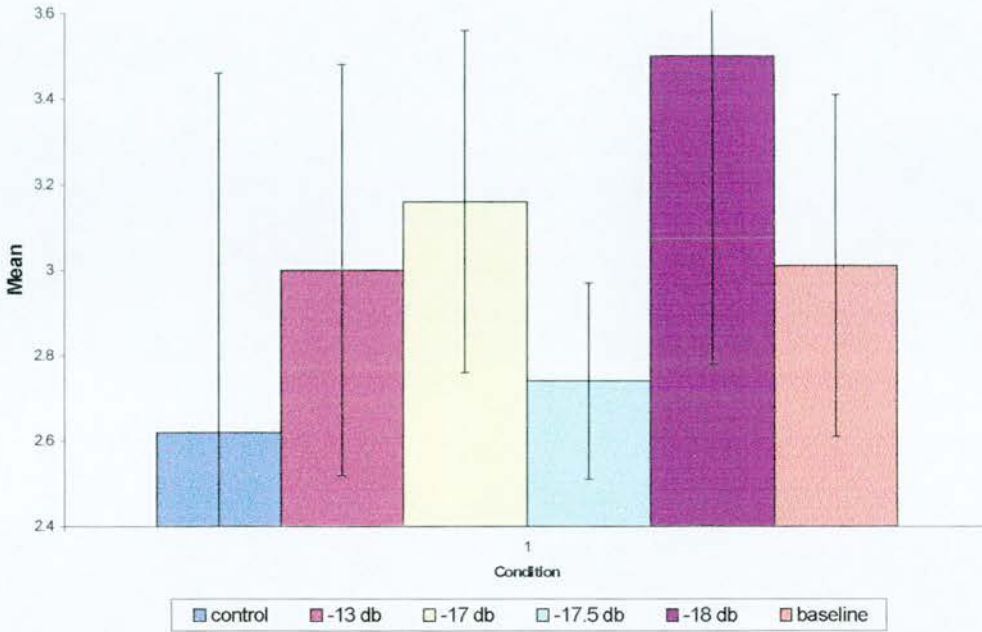
- 17.5db – $t(13) = 1, p = 0.17$, one tailed.

-18db – $t(13) = 0.42, p = 0.34$, one-tailed.

Although the –17db condition suggests a higher number of exclusion failures than baseline, this is not significant and there are no substantial differences between the baseline condition and the –17db, -17.5db and the 18db conditions.

Figure 5.5 represents the reaction times in each condition. It appears that participants were fastest to respond when the priming words were masked by 17.5db of noise, and slowest in the –18db condition. The scale of this chart may have exaggerated this effect.

Figure 5.5 - Mean reaction times at each level (seconds) and error



A repeated measures t-test was conducted on the reaction times in the control and baseline conditions, and no significant difference was found ($t(13) = 0.7, p = 0.25$, one-tailed). A repeated measures ANOVA was then conducted on the reaction time data for the -13db, -17db, -17.5db, -18db and baseline conditions. This did not reveal any significant difference between the groups ($F(4, 52) = 1.1, p = 0.37$).

Main Study

The results from the pilot were inconclusive. There was a suggestion of a very small effect occurring at the -17db ratio, as a peak in mean number of exclusion failures appeared at this level. It was hoped that the increased power in the main experiment would help to determine whether this effect was a real one, or whether it was a random variation. It was also decided to test another signal to noise ratio in order to determine whether any further trends could be established that might give an indication concerning the ratios at which any hypothesised effect may be most likely to appear. The ratio chosen was -19db as inspection of figure 5.4 suggested that there may be an increasing

trend in exclusion failures from -17.5db to 18db . If this was a real trend, then exclusion failures may rise above baseline in the -19db condition. It was also hoped that, by decreasing the number of conditions, and thus increasing the number of trials per condition, the power of the experiment would be increased, making it more sensitive to weak effects. In relation to this, it was decided to place limits on the randomisation process, so that there would be an equal number of trials in each condition.

Participants

The main experiment was conducted with 40 participants (12 males 28 females) ranging in ages from 17 to 57. All participants had normal and corrected to normal vision and did not suffer from hearing deficits or dyslexia. Of these, 5 were excluded from the final analysis as their scores in the control condition indicated that they did not follow the exclusion instructions accurately. A further 3 participants were excluded due to computer failures. The analysis, therefore was conducted on the remaining 32 participants (9 males, 23 females).

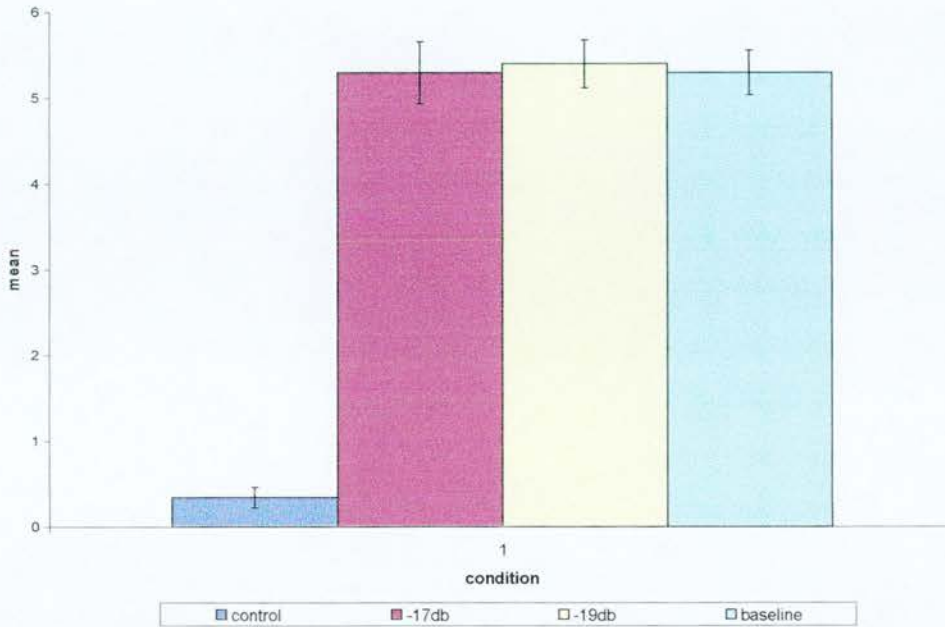
Design and Procedure

The design and procedure were essentially identical to the pilot study. Different signal/noise ratios were used (-17db and -19db) and the reduced number of ratios allowed for increasing the number of trials in each condition. There were 11 trials in each of the 4 conditions (baseline, control, -17db and -19db).

Results

Figure 5.6 illustrates the mean number of exclusion failures in each condition. It can be seen that in the control condition (no noise) there were very few occasions where participants failed to successfully perform the exclusion task.

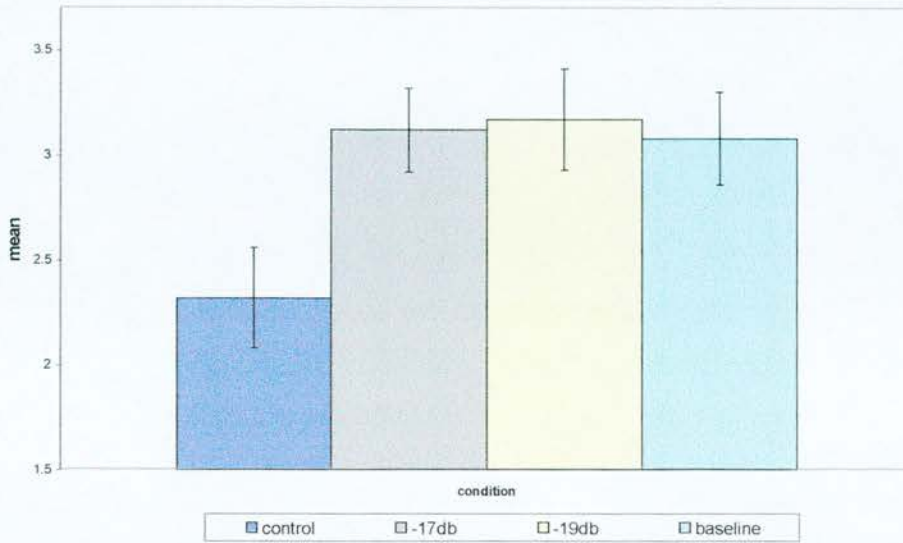
Figure 5.6 - Mean number of exclusion failures at varying db levels



The difference between the control condition and the baseline condition is significant ($t(31) = 19.72, p < 0.001$, one-tailed). A repeated measures ANOVA was then conducted on the -17db, -19db and baseline conditions, and no significant differences were found ($F(2, 60) = 0.05, p = 0.96$). This suggests that, in relation to baseline performance, masking a word with white noise at these levels does not have an influence on the subsequent exclusion task.

Reaction time data was also analysed (fig. 5.7). The difference between the baseline and control condition was again significant ($t(30) = 3.42, p = 0.002$, one-tailed), suggesting that participants made their decisions faster when the priming stimulus was not masked. This is to be expected, as the control condition was completely conscious, and thus the task was easiest in this condition. A repeated measures ANOVA was conducted on the reaction time data for the -17db, -19db and baseline conditions, and this did not reveal any significant difference ($F(2, 60) = 0.24, p = 0.79$).

Figure 5.7 – Mean reaction time at each db level (seconds)+ error



Discussion for experiment 1b

The aim of experiment 1b was to investigate whether an auditory stimulus perceived outside of awareness could influence responses to a homophone. This was based on the assumption that conscious and unconscious processes lead to qualitatively different effects when placed in opposition to each other, in this case, in an exclusion task. It was assumed that, when stimuli are perceived consciously, then responses based on these stimuli will be purposive in nature. Thus, when participants are asked to actively avoid using a stimulus when responding, then this should be easy to do when the stimulus is consciously perceived. If, however, the stimulus is perceived unconsciously, then the ability to actively exclude it from any responses will be diminished. Assuming that unconscious influences lead to automatic responses, it was predicted that when the stimuli were unconsciously perceived, then the automatic response would be to actually include it as a response, due to the fact that that particular response has been primed, but cannot be consciously excluded. Participants were instructed to respond with the first associated word that came to mind if they could not make a decision based on the

masked word. In this situation, it was hypothesised that the masked words would unconsciously influence this decision when presented outside of awareness.

Overall, this experiment failed to find any evidence of perception without awareness using this paradigm. This is interesting in itself, as Ott *et al* (2000) report a significant auditory perception without awareness effect at a ratio of -17.5db . Although our technical methods were slightly different than those of Ott *et al*, it may be argued that the technique employed in the present experiment bettered them in some ways.

These results may be interpreted in a number of ways. It may be the case that perception without awareness simply does not happen when stimuli are masked with noise. This would appear to be in line with some of the visual literature suggesting that “energy” masks do not lend themselves to effects, while “pattern” masks do. Energy masks are masks which consist of relatively random variation, such as white noise, or, in the visual domain, an abstract, unpatterned and random array. Pattern masks, on the other hand, consist of stimuli that have a consistent pattern to them. In the visual domain, this may consist of letters, or of symbols that are non-random in nature. In the auditory domain, a pattern mask might be music, or some other form of patterned acoustic stimulus. Marcel (1983) reported that visual pattern masks were more effective in eliciting PWA than visual energy masks. Marcel (1983) suggests that energy masks may impoverish the stimulus information prior to analysis of contour or figural properties, precluding pattern recognition of any kind. Presumably, for PWA to occur, the stimulus must be initially perceived at some crude level. If an energy mask prevents any kind of initial pattern recognition, then there will be no subsequent effect. However, while it is relatively easy to implement pattern masks in the visual domain, it is difficult to conceive being able to mask acoustic stimuli with a parallel pattern mask (at least not while keeping signal to noise ratio constant). For this reason, it would appear that, if indeed energy masking prevents PWA, then it may be impossible to ever convincingly demonstrate PWA in the auditory domain.

Another interpretation for the results obtained may be made in terms of the task used. This was the first time such an exclusion task had been used in a study of perception without awareness. It was based on a stem completion exclusion task used by Joordens and Merikle (1997) when investigating PWA in the visual domain. However, subtle differences exist between the two tasks, and perhaps these had an influence on the results. For example, in the stem completion task, participants are asked to complete a stem with any word that comes to mind with the exception of the previously presented word. Thus, the range of options open to the participant is reasonably large. In the current task, subjects were asked to respond with the homophone interpretation that they had *not* heard immediately before. However, in this task, a choice of two options were presented, limiting the range of options, and possibly influencing response patterns (e.g. if the word in the white noise *did* have an influence at some level, then this may be dissipated when the participant views the two response alternatives).

Whatever the explanation may be, it is not entirely discouraging that these results were obtained. It must be kept in mind that an entirely new method of masking stimuli was devised for this experiment. In addition to this innovation, a new type of exclusion task was used for the first time. Indeed, this was the first time such a task had ever been used in a study of auditory perception without awareness. Given these advances alone, then this work should be considered worthwhile despite the non-significant nature of the results. It remains to be seen whether a perception without awareness effect can be reliably established in the auditory domain using these methods.

General Discussion and Conclusions

The aim of these two studies was to compare the effects of two forms of weak, unconsciously processed information on word association. Experiment 1a attempted to influence homophone interpretation through psi means. Results suggested that there may have been an effect, especially when minority responses were considered in isolation. Evidence suggestive of an experimenter effect was also found. Despite the possibility that the results were due to an artefactual “stacking effect”, new techniques developed

specially for this study suggested that this was not the case. Obviously, this study requires replication before any firm conclusions can be drawn, although it would seem that word association might be a promising way to assess unconscious psi.

The effects found in experiment 1a were used as a basis for experiment 1b. Based on the assumption that psi processing might be unconscious and similar to PWA, study 1b set out to determine whether a comparable effect could be obtained using a degraded sensory stimulus. The procedure was not identical to experiment 1a however. In experiment 1a, psi was assumed to be inherently unconscious. In PWA studies, however, demonstrating that a stimulus has been perceived unconsciously is a contentious issue. As a result, an exclusion task was used as a way to assess whether a particular set of stimuli were perceived with or without awareness. If the stimuli were perceived outside of awareness, then the observed effect would be comparable to the suggestive psi effect in experiment 1a, in that the response to the homophone would be primed by the weak stimulus. However, the results of experiment 1b did not find any evidence (or even any trends) suggestive of this effect. It is tempting to conclude that the failure to find a comparable effect places the comparison between psi and PWA in doubt, although this may be a premature conclusion. The methods used in experiment 1b were innovative both in terms of the technology used to create the stimulus and the task employed to detect the effect. It is possible that further refinement of these techniques will lead to a clearer picture regarding their viability as methods to demonstrate PWA. It may, however, be argued on the basis of the visual literature (e.g. Marcel, 1983) that the type of masking used in the current study inherently precludes the possibility of finding an effect. Again, this is something that requires further study, and, in particular, replicating the findings of Ott *et al* (2000) is of the utmost importance for researchers interested in auditory PWA.

Taken together, these two experiments failed to shed light on the comparison between psi and PWA, but did raise some interesting questions in themselves, both in terms of the effects psi may have on ongoing associative processes, and the viability of finding a

PWA effect in the auditory domain. Chapter 6 will now look at the comparison between ψ and PWA from a different, although equally valid, angle.

Chapter 6 – Psi, PWA and False Recognition

General Introduction

The experiments described in the previous chapter used a parapsychological effect as a basis for the comparison between psi and PWA. Experiment 1a attempted to replicate the psi effect. Experiment 1b involved a degree of innovation in its design in order to investigate whether a similar effect could be obtained in the field of PWA. The following two experiments reverse this approach. The effect to be studied in experiments 2a and 2b was initially found in PWA research. As such, experiment 2a is an attempted replication of this effect, while study 2b is an innovative psi study aimed at obtaining a comparable effect.

Experiment 2a PWA and False Recognition

Introduction

As mentioned in chapter 3, one way in which PWA researchers have attempted to circumvent the criticisms levelled at their previous methodologies is to find tasks that are differentially affected by conscious and unconscious influences. If single tasks can be found that lead to different effects depending on whether the stimuli are viewed with or without awareness, then this obviates the need for two separate tasks, one indexing conscious awareness and one measuring an unconscious effect.

In 1989, Jacoby and Whitehouse reported a difference in recognition memory that seemed to rely on whether a biasing stimulus was perceived with or without awareness. *False recognition* is the term given to the resulting effect, and it is defined as an “old” response to what is actually a new word, on an “old/new” recognition test.

Participants in a PWA/false recognition study are first presented with a long list of words and are then presented with a recognition test. This test consists of a context word followed by a test word (e.g., *arena* followed by *tribe*). Participants must decide whether the test word was in the original memory list or not (i.e., old or new). Context words are presented in two conditions: matched (when the context word is the same as the test word, e.g., *tribe* followed by *tribe*) and nonmatched (when the context word is different from the test word, e.g., *photo* followed by *tribe*). The important effect reported by Jacoby and Whitehouse (1989) was that the matched and nonmatched contexts had qualitatively different effects depending on whether they were viewed with or without awareness. When the context words were presented for a short duration (e.g., 50 ms), a new test word was *more* likely to be judged as being an old word in the matched context than in the nonmatched context. However, this effect was reversed when context words were presented for a longer duration, assumed to allow conscious awareness. In this situation, context words were presented for 200 ms, and it was found that a new test word was *less* likely to be judged as being old in the matched context than in the nonmatched context (see Table 6.1).

Table 6.1: Probability of responding “old” to a new test word in Jacoby and Whitehouse’s Experiment 1a (adapted from Jacoby and Whitehouse, 1989)

Condition	Match	Nonmatch	Control
200 ms (aware)	0.24	0.29	0.23
50 ms (unaware)	0.36	0.19	0.26

Jacoby and Whitehouse (1989) explained this effect in terms of perceptual fluency. Perceptual fluency is an heuristic whereby the easier (fluent) a stimulus is to process, then the more familiar it will appear in a memory task. Thus, when participants are presented with a context word, this increases the perceptual fluency of that word when it

is presented as a test word. When the context word is presented outside of awareness (i.e., at 50 ms), the participant is not aware of where the sense of familiarity originates from and assumes that it must be due to having seen the word in the original study list (thus responding “old” to new words). When the context word is presented at a level that allows conscious awareness, participants tend to attribute *too much* of the fluency of the test word to the previously seen context word. This leads to a reversal of the previous effect, in that participants will be *less* inclined to respond “old” in the matched condition than in the nonmatched condition (because every time they feel a sense of familiarity, they are attributing it to the context word).

The PWA/false recognition effect implies that a stimulus outside of awareness can influence short-term memory. Based on the assumption that psi may be processed in a similar manner to weak sensory stimuli, and thus may have comparable effects, what evidence exists that psi can influence memory?

Much of the early experimental work on psi and memory focused on finding correlations between psi performance and memory performance (see, e.g., Feather, 1967; Kanthamani and Rao, 1974, 1975; Rao, 1978). Studies attempting to influence memory performance through psi means are rare. Stanford (1970), however, reported a study in which participants’ memory for a story was apparently influenced by nonintentional ESP.

As previously stated, Roll (1966) proposed a theory of ESP in which ESP responses are actually revived memory traces, activated by an external ESP stimulus. In this account, memory images are seen as “the sense data of ESP” (p. 158). Again, however, the focus of experimental research into this model has been on finding correlations between memory performance and psi performance. The difference between this position and the current hypothesis is that the present experiment is looking to see whether a psi stimulus can subsequently influence performance on a memory task that immediately follows it. The role of pre-existing memories in the psi process is not taken into account. Whereas

Roll posited a central role for memory in psi processing, the goals of the current study were more modest. The aim was simply to establish whether psi could influence performance on a memory task in a similar way to how a stimulus perceived without awareness might. The bigger questions concerning the role existing memories might play in psi functioning are not considered.

The purpose of the following experiments was twofold. First, a replication of the PWA false recognition effect was carried out at the KPU. This was conducted to establish whether an effect could be obtained and to give a reference point for the second study. A second experiment investigated the hypothesis that psi might have the same effect on recognition memory as a weak sensory stimulus.

Experiment 2a was an attempt to replicate the PWA false recognition effect using a different population sample from Merikle and Joordens's (1997) Study 2a, which itself was an updated version of the original study by Jacoby and Whitehouse (1989). It is hypothesised that, when context words are presented for a short duration (e.g., 50 ms), new test words will be significantly more likely to be judged as being an old word in the matched context than in the nonmatched context. Furthermore, when context words are presented for a longer duration (114 ms), new test words are expected to be less likely to be judged as being old in the matched context than in the nonmatched context.

Method

Overview

Participants were presented with a list of five-letter nouns. They were then presented with an old/new recognition test. This test consisted of a masked biasing word followed by a test word. Their task was to decide whether the test word had been in the original list ("old") or was a new word ("new"). The biasing word was presented for one of two durations (50 ms or 114 ms – these times were chosen on the basis of Joordens and Merikle's 1997 replication of Jacoby and Whitehouse's original study).

Participants

Participants were 29 undergraduate and postgraduate students at the University of Edinburgh, Edinburgh, Scotland. Ages ranged from 17 to 39 (mean age = 20 years). Participants were recruited through tutorial groups and through e-mail appeals for participants. All of the participants had normal or corrected-to-normal vision, and English was their first language. They were told they were taking part in a study of memory.

Materials and apparatus

A total of 540 five-letter nouns were compiled from the Kucera and Francis (1967) tables of word frequency. Word frequency ranged from 3 to 60 occurrences per million, which is standard for this type of experiment. One hundred and twenty six words were randomly selected from the pool for the study list, and another 126 were randomly selected by the computer as the new words on the old/new recognition task. A further 84 words were selected to be used as the context words on the nonmatch trials of the old/new task.

A Sony monitor (Model GDM 200PST) with a vertical refresh rate of 160 Hz was used to display the stimuli. The graphics card driving the monitor was an ATI XPERT XL (Rage Pro chipset) with 32 Mb video RAM. The E-Prime® experiment generator program was used to create and run the program. The display settings were adjusted to use 256 colours, and a screen area of 640 × 480 pixels was set.

Procedure.¹

Participants were met by the experimenter and were then taken to the experimental room. This room was essentially a computer lab, consisting of a number of approximately twelve computers. For the duration of the testing period, the lab was empty, with the exception of the participant and the experimenter, who either sat in the lab in a position where the participant could not see him or, on less frequent occasions where it was necessary to leave the lab during the course of the session, waited outside the lab. Participants were told to sit down and make themselves comfortable, and they were requested to adjust the height of the seat so that their eye level was at the centre of the computer screen. The experimenter then verbally described the task, giving the participant an opportunity to ask any questions he or she might have had. The experimenter then started the program running. At this point, the experimenter retired, either to a position in the lab where the participant could not see him or, less frequently, outside the lab. The participant was presented with written instructions on the screen describing the task. The main experiment was in two parts. The first part involved presenting a study list of 126 words, at a rate of 1 per second, with each word presented for 500 ms followed by a 500-ms blank field. Participants were told to silently read each

¹ Prior to the creation of the program, a characterisation of the monitor was conducted. This used a BBW21 silicon photodiode, which has sensitivity similar to the human eye. A Tektronic 5000 series oscilloscope was used to measure the number of screen refreshes detected by the photodiode. A photodiode was attached to the monitor to measure light output. A variety of stimulus presentation times were then tested. It was found that, at extremely fast presentations, the actual output differed from what was requested from the program. However, the true output followed the screen refresh rate of the monitor. As the screen refresh rate was 12.5 ms, then the actual presentation times were a multiple of this. The characterisation allowed me to determine for exactly how long any particular stimulus was presented. Thus, it could be claimed with confidence that the timing of the stimulus presentation in this study was accurate.

word to themselves and that their memory for these words would be tested in the second part of the experiment.

The second part of the experiment consisted of the old/new recognition task. Each trial had the following structure: (a) masking stimulus (££££££££) presented for 500 ms, (b) a context word presented for either 50 ms or 114 ms, (c) masking stimulus presented again for 500 ms, and (d) a test word presented until a response was made. All stimuli were size 18 point and presented at a location in the centre of the screen.

Participants were asked to read the context word to themselves if possible and then to respond “old” or “new” to the test word with respect to the initial study list. Recognition memory was tested in three contexts: (a) match (context word and test word are identical; e.g., *arena* – *arena*); (b) nonmatch (context word and test word are different, and the context word is a new word; e.g., *tribe* – *arena*); and (c) baseline (context word was the letter string *xoxoxox*). For all three of these contexts, the test words were old on half of the trials and new on the remaining trials. This was determined randomly by the E-Prime program.

The old/new recognition test consisted of 12 practice trials followed by 240 experimental trials. Practice trials had four exemplars of each of the three contexts. The six old test words used for the practice trials were the first and last three words of the study list. The experimental trials consisted of 120 trials in which the context word was displayed for 50 ms and 120 trials in which the context word was presented for 114 ms. There were never more than 3 consecutive trials when the same duration was used.

Once testing was complete, the experimenter informed the participant that the experiment was over and answered any questions the participant wished to ask. This often took the form of explaining to the participant the purpose of the experiment. At no point did any participant inform the experimenter that he or she had worked out what the purpose of the experiment was during the testing period, although several participants did mention what they thought the experiment was testing. This was usually the result of

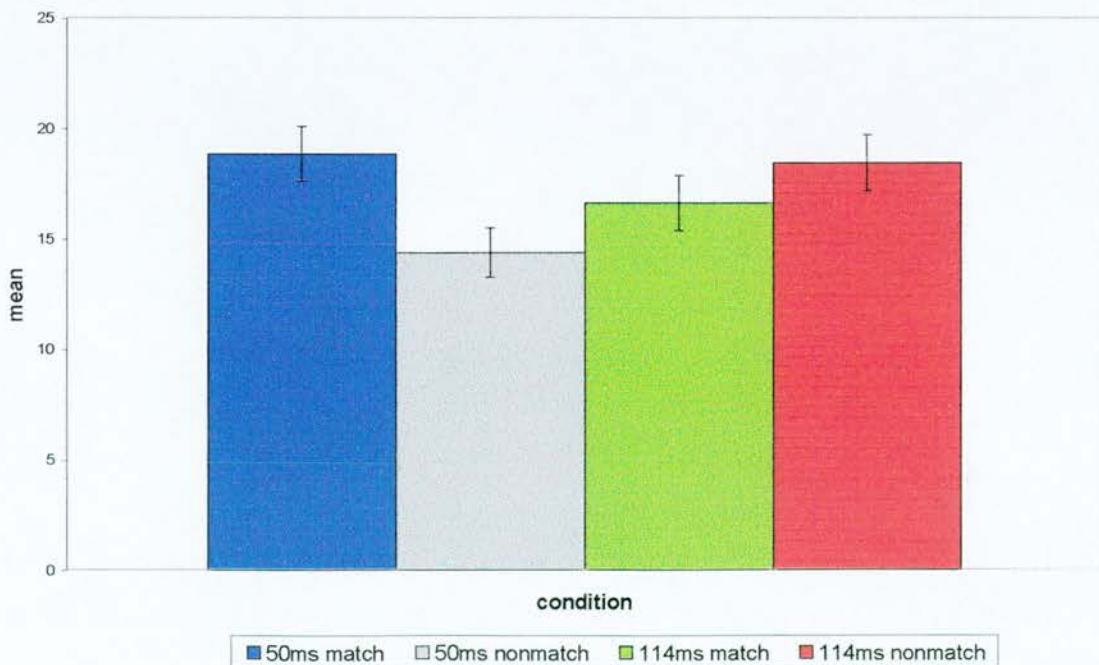
two or more related words appearing in close proximity to each other during the testing period. This, of course, was unintentional, and the result of the random selection of words.

Results

A 3×2 repeated measures analysis of variance (ANOVA) revealed a significant main effect of context, $F(2, 56) = 3.50, p = 0.04$, and a significant interaction between duration and context, $F(2, 56) = 14.51, p < 0.001$. Further analysis of this interaction revealed that in the 50-ms condition, there were significantly more “old” responses to new words in the matched context than in the nonmatched context, $t(28) = 4.78, p < 0.0001$, one-tailed.

In the 114-ms condition, this result was reversed, with significantly more “old” responses in the nonmatch condition than in the match condition, $t(28) = 1.706, p = 0.05$, one-tailed. Figure 6.1 demonstrates the nature of the interaction.

Figure 6.1; Mean Responses of ‘Old’ responses in 4 conditions (+ error)



Discussion for Experiment 2a

These results confirm the hypotheses and are in accordance with previous studies (e.g., Jacoby and Whitehouse, 1989; Merikle and Joordens, 1997). They demonstrate a qualitative difference in recognition memory depending on whether the context stimulus was viewed with or without awareness. The author concurs with the original explanation offered by Jacoby and Whitehouse (1989) that this is likely due to perceptual fluency, caused by unconscious exposure to the context word. When the participant feels the context word is familiar, he is more likely to attribute that feeling of familiarity to having seen the word in the study list when it has been presented “unconsciously”.

Given that the experiment described above suggests that an unconsciously perceived stimulus can have an influence on recognition memory, the purpose of the following experiment was to investigate whether a comparable effect could be obtained by means of a psi-mediated stimulus. This is in line with the central theme of the thesis, which holds that psi and PWA are similar in the way they are processed, and thus must have similar effects on cognitive processes (in this case, recognition memory).

Experiment 2b – Psi and False Recognition

Introduction

Having confirmed that recognition memory can be influenced when a biasing stimulus is perceived outside of awareness, the aim of Experiment 1b was to obtain a similar false recognition effect using psi as a biasing stimulus. In this experiment, the biasing stimulus was presented to a sender who was isolated from the participant and who was attempting to influence the participant's responses on selected trials. If information obtained through psi is subsequently processed in a similar way as PWA, then it would be logical to expect it to have similar influences on cognitive processes, in this case recognition memory.

The present experiment is, as far as the author is aware, the first study looking at the effect of psi on recognition memory. The main hypothesis was that there would be more "old" responses to new words when a sender had "sent" the target word in the 5 s immediately prior to the trial. A nondirectional exploratory hypothesis concerning reaction time was also included, stating that a difference would be observed in reaction times between the sending and nonsending conditions.

Pilot Study

A pilot study was carried out with 21 participants (see main experiment for more detailed description of method). Participants were presented with a list of 240 words, followed by an 'old/new' recognition test. This test consisted of 60 trials, 30 of which were 'old' trials and 30 of which were 'new' trials. On each trial, the participant viewed a black spot on the screen for 5 seconds, before seeing the test word. On half of the 'new' trials, a sender (the author) viewed the word that the participant was about to see and attempted to influence participant to say 'old' (i.e. to falsely recognise the word). A comparison was made between 'old' responses to send and non-sent words. The results were non-significant overall ($t(20) = 0.802$, $p = 0.734$). Effect size for this study was 0.18. Power for this test was <0.15 .

Exploratory analysis of this data revealed some interesting gender effects. Males and females were compared on their performance on non-sent items ($t(19) = 0.345$, n.s., effect size 0.15) When males and females were compared on sent items the results were considerably better, with females showing more 'old' responses to sent items than males ($t(19) = 1.521$, $p = 0.145$). Effect size for this test was 0.747, which is considerably larger than either the overall results or the test comparing responses to non-sent items, although this may be a result of the small N used.

On the basis of this pilot study, we decided to proceed with the main experiment, making some slight changes to the procedure in an attempt to improve the effect. The number of "old/new" recognition trials was increased from 60 to 80. This meant that the number of "psi" trials in the main experiment was 40. It was hoped that this increased number of trials would increase the power of the study to detect any psi effect that may have been present.

Additionally, the pilot test suggested that there might be a gender effect in this task. This is something that previously has been found in parapsychology, using different kinds of ESP tasks (see, e.g., Dalton and Utts, 1995; Freeman, 1967). In accordance with this pilot data, it was hypothesised that female participants would display more false recognition in the sending than in the nonsending condition.

Method

Participants

Forty participants took part, ranging in age from 18 to 35 years old (mean age = 22 years). There were 25 female and 15 male participants, which approximates the respective male/female ratio in the population used (i.e. primarily psychology students). All of the participants had normal or corrected-to-normal vision, were native English speakers, and had not taken part in Experiment 1a. They were recruited in the same

manner as Experiment 1a. Again, they were told that they were taking part in a study on memory.

Materials and apparatus

From the initial 540 word pool used in Experiment 1a, 120 words were selected randomly to serve as the study list, and a further 100 were selected to serve as potential new words in the old/new recognition test.

A button box was used to register responses. This was different from the one used before (due to the different software used in the creation of the experiment). This button box consisted of two buttons, labelled “old” and “new,” and was small enough to be comfortably held in one hand during the testing procedure.

Procedure

The setup was different from that of Experiment 1a because the experiment was carried out in a different room. As the present experiment required a sender, the experimental suite at the Koestler Parapsychology Unit was used (see experiment 1a for details about this set-up).

Participants were met by the experimenter, taken into the experimental room, and asked to sit down and make themselves comfortable. The experimenter then described the general procedure. Participants were told that they would initially see a list of words and that their memory for these words would be tested. They were told that they would be given an old/new recognition task, consisting of a black spot followed by a word. Participants were told to watch the black spot and relax for the duration it was on the screen. They were told to respond to the word, indicating whether they thought it was old or new. If they were unsure, participants were instructed to go with their “gut instinct.” They were then asked if they had any questions. After this, the participant was given the button box to hold. It was then explained that the experiment took a total of 10 min after which the experimenter would re-enter the room and debrief the participant.

The experimenter then started the program in the experimenter's room and retreated to the sender's room while the participant viewed the initial word list. The experimenter's room was locked during this period, and the key remained with the experimenter. Thus, the participant did not have any normal access to what was being sent during the experiment. The study list was presented in the centre of the screen, at a rate of one word per second with each word presented for 500 ms with a 500-ms blank field.

The second part of the experiment consisted of the old/new recognition test. On each trial, participants viewed a black fixation spot for 5 s, before a test word appeared. The task was to decide whether the test word was in the original list (new) or not (old), and responses were made on a two-button box. The difference between this and the old/new task in Experiment 1a was that no context word was presented in the present task.

There were 80 trials in total, consisting of 40 "old" trials and 40 "new" trials, randomly selected by computer. Half of the new trials were selected by the computer as being psi trials (i.e., 20 psi trials). On these trials, the word and a visual image related to the test word was shown to the sender during the 5 s *before* the participant viewed that word. The sender attempted to "send" this word, and thus create a sense of false recognition in the participant. (The author was always the sender in this experiment. I have had limited experience as a sender, conducting one previous experiment in which the results were mixed.)

Results

Table 6.2 shows mean number of false recognition responses in the sending and nonsending conditions and mean reaction times in the respective conditions. There is a slightly higher mean in the sending condition, but this difference is extremely small. Likewise, reaction times are slightly faster in the sending condition, but again this difference is small.

Table 6.2: Overall mean “old” responses to new words, reaction times and standard deviations.

Measure	Sending		Nonsending		N
	mean	S.D.	mean	S.D.	
“Old” responses to new words	5.53	3.04	5.3	3.07	40
Reaction time (in seconds)	2.04	1.17	2.17	0.95	40

Table 6.3 shows the mean number of false recognition responses for male and female participants in each condition. Whereas female participants appear to show slightly more instances of false recognition in the sending condition, the opposite is true of male participants.

Table 6.3: Differences between male and female participants in mean number of “old” responses to new items.

Mean no. of “old” responses to new words					
Gender	Sending		Nonsending		N
	mean	s.d.	mean	s.d.	
Male	5.08	3.4	5.76	3.66	15
Female	5.74	2.89	5.07	2.79	25

Table 6.4 displays the reaction times in each condition for male and female participants. Female participants appear to be faster in the sending condition, whereas the opposite is true for male participants. Again, however, these differences are extremely small.

Table 6.4: Mean reaction time data for male and female participants in each condition.

Reaction times (in seconds)					
	Sending		Nonsending		
Gender	mean	s.d.	mean	s.d.	N
Male	2.45	2.36	2.31	1.04	13
Female	1.85	1.04	2.11	0.91	27

Although the hypotheses in this study were directional, all tests were two-tailed. This was to detect any potential psi-missing that may have been present in the data.

A 2×2 mixed ANOVA was carried out on the false recognition data, with condition (sending vs. nonsending) and gender as the variables. This revealed no main effect for condition, $F(1, 38) = 0.001$, $p = .98$ (effect size $\eta^2 = 0$; power = 0.05); no main effect for gender, $F(1, 38) = 0.00$, $p = .99$ (effect size $\eta^2 = 0$; power = 0.05); and no interaction, $F(1, 38) = 2.65$, $p = .11$ (effect size $\eta^2 = 0.07$; power = 0.35).

To test the reaction time data, a 2×2 mixed ANOVA was conducted to test the two-tailed hypothesis that reaction time might be influenced according to the sending/nonsending conditions. Again, the results revealed no main effect for condition, $F(1, 38) = 0.24$, $p = .63$ (effect size $\eta^2 = 0.006$; power = 0.076); no main effect for gender, $F(1, 38) = 1.43$, $p = .24$ (effect size $\eta^2 = 0.036$; power = 0.215; and no interaction effects, $F(1, 38) = 2.74$, $p = .11$ (effect size $\eta^2 = 0.067$; power = 0.365)

All of these results are in accordance with the null hypothesis.

Discussion for Experiment 2b

The results of Experiment 2b were not as predicted. Experiment 2b was based on the premise that, once in the system, psi may work in a similar way to PWA and thus may have an influence on the same processes that PWA has been found to affect. The false recognition effect in PWA is fairly robust, as demonstrated by Experiment 2a. To my knowledge, this was the first time that recognition memory had been used as a possible mediator for psi. The overall results were nonsignificant. The first explanation for this pattern of results is that there was no psi happening at all. Although this may seem like an obvious conclusion, it is important to point out. It is also possible that, if indeed there was a psi effect present, then it was too weak to be detected.

One other possible reason for the nonsignificant results is the small number of false recognition responses elicited during each session. In Experiment 2a, the overall mean number of “old” responses was 16.65, whereas in Experiment 2b the mean was 5.58. This is a considerable difference and most likely reflects the reduced number of trials in Experiment 1b.

Another, related, explanation for the nonsignificant results in Experiment 2b is that the participants’ good performance may have served to obscure any psi that may have been happening, through a ceiling effect. Participants made only a few mistakes per session, so the quality of their memory performance may have overridden any other (i.e., psi) factor. Experiment 2b consisted of fewer trials than Experiment 2a (240 trials in Experiment 1a, 80 trials in Experiment 1b—20 of these being psi trials), due to the time required for “sending.” This may have been an important factor as it may not have been a sufficient number of trials to allow enough mistakes or uncertainties that might have given rise to the effect. For example, if there are many trials, then it is likely that a participant will be unsure of the correct response more often. When this is the case, then they are more likely to use secondary cues (e.g., a feeling of familiarity) to be able to respond. This may be where any false recognition effect comes to the surface. If, however, there are too few trials, then memory performance may be good, and the need

to use secondary cues is substantially reduced, thus reducing any effect. It is, therefore, possible that the test itself was not powerful enough to pick up any psi that may have been present.

An interesting pattern of results emerged when gender effects were analysed. A short pilot version of Experiment 1b seemed to suggest that women displayed more psi than men. While this may seem an odd finding, it has some precedent. Dalton and Utts (1995) reported that in the PRL (Psychophysical Research Laboratories) ganzfeld database, it was found that male/female, sender/receiver pairs performed best, followed by female/female pairs, with male/male pairs performing worst. As the author always took on the role of sender in Experiment 2b, then it may be possible to speculate that the apparent gender difference observed in the current experiment is in line with these previous findings. This will be discussed further in the final chapter. Another aspect of Experiment 2b was that this pattern of results held true when the reaction time data was analysed. It may be of note that there was, again, a small (nonsignificant) interaction between male and female participants. Male participants performed faster in the nonsent condition, whereas female participants tended to respond faster to sent items. Again, this is nonsignificant. It is difficult to speculate about why reaction time might differ between conditions and between genders. There was no real a priori reason to expect reaction times to differ in a particular direction. On the one hand, it could be argued that reaction time should be quicker when the sender is sending, due to the “helping hand” given to the decision. This is in accordance to what one would expect if the psi information is unconscious in the same sense that other weak perceptual stimuli are. In these cases, the response to an unconscious stimulus is automatic, and thus faster than any response in which conscious perception mediates the response. On the other hand, it may be argued that the reaction time may be slower in the sent condition, due to the fact that the sender is sending erroneous information (i.e., trying to influence the participant to say “old” to a new word). This may slow down the decision as the participant struggles with conflicting information.

The above is, it must be noted, purely hypothetical as none of the gender interactions were significant. The effect sizes for both interaction effects were small, and the power for both interactions was only .35 and .36, respectively, suggesting that a more powerful test of these interactions is required. However, the fact that gender and psi interactions persisted in two different measures of psi is interesting in itself, as is the fact that the effect sizes of each interaction are so similar.

Any further work looking at the effect of psi on recognition memory should be aware of the difficulty in maximising power. If psi exists and can influence this type of task, then it is likely to be a weak effect and difficult to detect. This requires that power be maximised in psi studies, by either increasing the number of trials or increasing the number of participants. In the present study, it may not have been productive to increase the number of trials, as this would lead to extremely long, arduous sessions. Many participants in Experiment 2b commented on the difficulty that they experienced performing the task continuously. Having a break during the session was considered but was not implemented as it was felt that this may have an adverse effect on the memory task. Another issue to be considered is the time interval between trials, which was 5 s in Experiment 2b. The reason for this was to give the sender time to send. Reducing the amount of time could increase the number of trials, and thus increase the power. However, if the sending time is reduced, then it may have a detrimental effect on the sender, who is attempting to influence the participant during this period. Indeed, an argument could be made for increasing this time for sending purposes. This is an issue that deserves further investigation.

Summary

To summarise, while a replication of the PWA false recognition effect succeeded, an attempt at obtaining a parallel effect using psi as a biasing stimulus failed. This may have been due to methodological issues such as the reduced number of trials in experiment 2b or the task itself may not have been psi-conducive. There were some interactions suggestive of a gender interaction, although these were not significant.

As in experiments 1a and 1b, the experiments described in this chapter did not suggest that psi and PWA have comparable influences on cognitive processes. There may, however, have been other reasons for these findings, and these will be considered in chapter 8. The purpose of the following chapter (7), however, is to take the most promising aspects of the previous experiments, and combine them. The word association psi task from experiment 1a and the PWA false recognition task from experiment 2a were the most successful protocols, and it was decided to make a direct comparison between performance on both types of task (psi and PWA). Chapter 7 describes the final study, aiming to bring together psi and PWA in a repeated measures experiment, while also incorporating a number of personality variables in order to determine what other variables may potentially mediate performance on both types of task.

Chapter 7 – Study 3 - Directly Comparing Psi and PWA

Introduction

The aim of study 3 was to combine the most promising aspects of the previous studies in one, repeated measures design. The results from experiment 1a (psi influence on homophone interpretation) were interesting, and suggested that there might be a weak main effect of psi on the homophone word association task. Additionally, the response bias effect showed promise as a further measure of psi influence. Given the encouraging results from experiment 1a, and the fact that this procedure required replication, it was decided that the final study would use this technique from experiment 2a as a test of unconscious psi. In addition to this, it was also decided to use the PWA false recognition technique as a measure of PWA. This effect appeared to be reliable, and should offer a robust demonstration of PWA, which can then be compared to psi performance.

Although the previous studies had sought to make a comparison between psi and PWA by attempting to elicit similar effects in each respective condition, they were not sensitive to any possible relationship between performance on each task. This is because each experiment was run separately using different participants. An important question that the previous studies did not address was whether there might be a correlation between performance on each type of task. Do people who achieve high psi scores also show higher levels of perception without awareness? If this were indeed the case, then it would suggest that there might be certain individuals who are more open to unconscious influence. If this effect was found, then it would lend considerable support to the idea of psi information being processed unconsciously, which is a central theme of the current thesis. Additionally, if a correlation between performance on the two tasks did exist, it would lead to questions concerning what might underlie such an effect. This is an important question, as it is possible that there are various factors that may have an influence on the way people perform on

the respective tasks. The present study, therefore, incorporated a number of personality variables so as to gain a fuller picture of what factors may play a role in psi/PWA performance.

What factors might be involved if certain people are found to be open to weak, unconscious stimuli? There are a number of possibilities. Firstly, there has been some precedent in parapsychology for believing that belief in whether or not psi exists might actually underlie psi performance (Schmeidler and McConnell, 1958; Lawrence, 1993)¹. In an unpublished finding, Roney-Dougal found that believers in PWA show a similar effect, with believers in PWA displaying more actual PWA than disbelievers (Roney-Dougal, 2002, personal communication). Based on the findings in parapsychology, study 3 incorporated two measures of belief: one looking at belief in psi, and one measuring belief in PWA. If psi and PWA are comparable, then factors influencing performance on one should also influence performance on the other in a similar way. The effect in parapsychology whereby believers in psi outperform disbelievers serves as the basis for comparing psi and PWA in this way. If it was found that individuals who believe in PWA show a higher degree of PWA performance, then this would suggest that psi and PWA are comparable, and may share commonalities in the way they are processed and the factors underlying this processing.

Comparing psi and PWA involves looking for factors that are common to both phenomena. One such factor might be the fact that they can both be associated with the process of “intuition”. Intuition is a concept that is difficult to define adequately, although Bastick (1982) offers a number of suggestions. It can be generally described as knowledge (or at least the feeling of knowledge) that arises without any conscious mental steps.

¹ This effect was briefly introduced in chapter 4 as the “sheep/goat” effect. In this chapter, the terms “believer” and “disbeliever” will be used instead of “sheep” and “goat”, due to the comparative neutrality of the former terms.

It might be posited that people who are adept at utilising unconscious information (be it psi or PWA information) are more intuitive than those who are not. Much has been said about the concept of intuition, and it is not the purpose of the current thesis to cover this issue. However, most commentators would agree that there is an unconscious element to intuition (see, e.g. Bastick, 1982). One way of envisaging the intuitive process is to posit that it is the result of the individual perceiving information without awareness and then acting on this information. In doing so, the individual may benefit from the information that was unconsciously perceived. This would look like what is commonly known as “intuition”. It is, therefore, important to identify what unconscious factors may play a part in the intuitive process. As previously stated, if psi is an unconscious process, then considering it alongside other unconscious phenomena should lead us to discover factors common to both psi and other unconscious phenomena. If there is an unconscious element to intuition, and if there are factors that can be shown to underlie these unconscious factors, then it follows that the same factors may also apply to psi processing.

Recent research has suggested that there might be individual differences in the intuitive process, although this is an issue which has been disputed.

Some commentators have held intuition as being an ability exclusive to a creative population (e.g. Polanyi, 1958). This view holds that intuition is relatively rare, and available to a select few. This would seem unlikely, given that most people benefit from some form of intuition during their lives. More recently, Reber, Walkenfeld and Hernstadt (1991) have suggested that intuition (as measured by performance on an implicit learning task) is equally distributed throughout the population. Reber (1992) holds that the unconscious processes necessary for intuition are evolutionarily older than explicit, conscious processes. This led Reber (1992) to postulate that there would be fewer individual differences in these unconscious processes. This contrasts with Westcott (1968) and Bowers, Regehr, Balthazatd and Parker (1990) who hold that there are individual differences in intuition.

Woolhouse and Bayne (2000) have recently found that people who are classed as intuitive on the Myers-Briggs Type Indicator are more successful at using

unconscious information as measured by a test of implicit learning. Taking Woolhouse and Bayne as a precedent, it was decided that study 3 would use the MBTI as a way of measuring whether people considered themselves intuitive. Given that Woolhouse and Bayne (2000) found that individuals classified as intuitive seemed to perform better on a test of implicit learning, it was of interest to investigate whether this finding could be extended to other measures of unconscious processing: psi and PWA. Once again, if it could be demonstrated that factors such as this personality factor relate to performance on both types of task in a similar way, then it would lend support to the hypothesis that they are meaningfully comparable.

Additionally, a further factor that may be important in comparing psi and PWA are factors concerning the way in which people process information. Epstein (1990, 1991, 1993, 1994) has developed a theory of personality which incorporates an “intuitive” information processing style. This measure was also used in the present study. The reasoning behind the inclusion of the REI is much the same as that for using the MBTI. As the REI measures the way in which people process information, then it may be possible to establish whether there is any association between susceptibility to unconscious stimulation and processing style. Additionally, the REI incorporates a concept of “intuition” which, again, relates to the phenomena we are studying.

What follows is a brief description of the personality variables used in the present study.

MBTI

The Myers-Briggs Type Indicator was first published in 1962 (Myers, 1962) as a means to validate C.G. Jung’s theory of psychological types (Jung, 1921/1971) and put Jung’s theory to practical use. The MBTI consists of four scales: Extraversion/Introversion (EI), Judging/Perceiving (JP), Sensing/Intuition (SN) and Thinking/Feeling (TF). The reliability and validity of the MBTI has been the subject of many studies over the years, and the MBTI manual (Myers, McCaulley, Quenk and Hammer, 1998) presents results from a large databank indicating reliabilities of

above 0.8 for each scale. The validity of the scales has also been extensively documented (e.g. Bayne, 1995). The MBTI appears to show strong relationships with four of the five scales in the five-factor model of personality as measured by the NEO-PI (McCrae and Costa, 1989). Indeed, as Bayne (1995) points out, this fact allows the MBTI to benefit from the extensive validity work conducted on the five-factor model. The scale that we are currently concerned with is the SN scale, and, in particular, the “intuitive” dimension. According to Jung, intuition was characterised as perception by way of the unconscious. In describing intuition, Myers *et al* (1998) state that “[i]ntuition permits perception beyond what is visible to the senses” (p.24). This is in contrast to the sensing dimension, which places emphasis on information that is directly available to the senses. Based on this, it would appear that the intuitive dimension might be associated with perception without awareness, if “visible to the senses” can be interpreted as relating to that which can be consciously perceived. It may be possible that participants who are adept at using unconscious information (be it psi or PWA information) might be the same people who class themselves as intuitive on the MBTI (after Woolhouse and Bayne, 2000).

The MBTI has been used in parapsychology before. Honorton (1997) reports that participants who are classed as being “feeling” (on the thinking/feeling dimension) and “perceiving” (on the judging/perceiving dimension) are also those who seem to score above chance in the Ganzfeld. Similarly, Parker, Grams and Peterson (1998) report that above chance performance in the Ganzfeld was associated with participants who were classed as “feeling” on the MBTI. Parker *et al* (1998) suggest that this finding may be linked with a concurrent finding suggesting that the emotional content of the targets also plays an important role in Ganzfeld performance.

Honorton, Ferrari and Bem (1998) report a significant association between *extraversion* and above chance performance on a Ganzfeld ESP test.

The difference between these studies and the current approach is that most previous studies looking at the MBTI in relation to ESP have used some form of intentional

ESP test. Intentional ESP tests may be substantially different from nonintentional tests, and as such, the personality variables involved in both may not be comparable. In intentional ESP tests, factors such as self-consciousness may come into play as participants are explicitly told that they are taking part in an ESP experiment. This pressure to “be psychic” might influence different people in different ways, and it may be this that that the observed correlations between certain personality variables and successful performance is indexing. Nonintentional psi tasks do not place any such pressures on the participant (although admittedly there may be different kinds of pressures involved). Unconscious, nonintentional psi, if it does indeed exist, would seem like intuition in everyday life. For example, a person might get a “hunch” or a “gut-feeling” that might be psi mediated, and then act on this to his/her advantage. Given that the information is unconscious, then the individual would be likely to attribute the occurrence to his intuitive abilities, rather than psi. This is in direct contrast to intentional psi, which many parapsychological studies focus on. Real world instances of intentional psi may actually be rather rare, and, if they were to occur, the individual would probably make attributions based on psychic ability rather than intuition. Thus, in the current study, the sensing/intuitive dimension was utilised in the current study. This was based on the assumption that psi (or, at least, nonintentional psi) might be unconscious and similar to other forms of unconscious psychological abilities, and that individuals classed as “intuitive” may show more sensitivity to unconscious influences.

REI

Another personality measure that was used in this final study was the rational-experiential inventory (REI). This derives from a global theory of personality known as cognitive-experiential-self-theory (CEST; Epstein, 1990, 1991, 1993, 1994). CEST holds that there are two parallel interactive information-processing systems. These are a rational system and an experiential system. According to the theory, the rational system operates primarily at the conscious level and has several attributes. It is assumed to be intentional/effortful, analytical, mediated by conscious appraisals and slow. In contrast, the experiential system is thought to be automatic/effortless, associationistic, unconscious and fast. Behaviour and conscious thought are a joint

function of the two systems, although they sometimes conflict, and in certain situations one mode of processing may take precedence. It would appear that the experiential dimension shares many characteristics with attributes of PWA (e.g. automaticity), so it was decided to include this in the present study as a further measure that could conceivably correlate with unconscious processes (i.e. psi and PWA).

Epstein *et al* (1996) constructed a questionnaire intended to measure individual differences in intuitive (experiential) and analytic (rational) thinking styles. To measure analytic-rational processing, they adopted the need for cognition (NFC) scale, previously introduced by Cacioppo and Petty (1982). As no appropriate scale was available for measuring the intuitive thinking style, Epstein *et al* (1991) developed what became known as the “faith in intuition” (FI) scale (see Epstein *et al*, 1996 for validity and reliability issues). Together, the FI and the NFC scales make up the rational-experiential inventory (REI). The REI differentiates respondents into four categories:

1. High on FI and high on NFC (complementary thinking style)
2. Low in FI and low in NFC (poor thinking style)
3. High on FI and low on NFC (intuitive thinking style)
4. Low on FI and high on NFC (rational thinking style)

Although it has previously been used as a possible predictor of paranormal belief (see Wolfradt, Oubaid, Straube, Bischoff and Mischo, 1999), the current study was the first to look at the REI in relation to performance on a psi or PWA task (see below for related hypotheses).

Belief measures

The measure of psi belief was taken from the participant information form used at the Koestler Parapsychology Unit, University of Edinburgh (see appendix 2). This is composed of 12 questions relating to belief in psi phenomena, although only 11 questions were used (the 12th question related to a prediction concerning

performance in a subsequent psi task, and was inappropriate as the questionnaire was always administered after the task - see below).

Additionally, a short three-item belief in PWA questionnaire was created, based on the questions asked in the psi questionnaire (see appendix 3). This was intended to measure the degree to which participants believed in the possibility of being unconsciously influenced. The PWA questionnaire consisted of a short description of what PWA is, and explained that it has been a controversial topic in psychology. This was followed by three questions asking participants to rate whether they believed in the possibility of being influenced by stimuli outside of their awareness; whether they had ever felt as if they had been influenced by stimuli outside of their awareness and whether they had heard or read of any experiences which suggested PWA could take place.

In addition to investigating the relationship between the above personality variables and psi/PWA performance, it was also possible to make predictions concerning how they might relate to each other. As the MBTI and the REI both purport to be measuring factors that could be related to “intuition”, it was predicted that there would be an association between the intuitive dimension of the MBTI, and individuals classed as “high faith in intuition/low need for cognition” in the REI. Indeed, Pacini and Epstein (1999) state the importance of relating the REI scales to other scales, placing emphasis on the MBTI.

It was also predicted that belief in psi and belief in PWA would be correlated with each other (after Roney-Dougal, 1987, see above).

Although predictions based on the personality variables and psi/PWA performance were easy to make, it became less clear what to expect when considering the relationship between *belief* in psi/PWA and the REI/MBTI. Various studies have suggested that believers in psi have different cognitive abilities than nonbelievers (see, e.g. French, 1992). For example, Blackmore and Troscianko (1985) found that believers in psi were worse than disbelievers when asked to make probability

judgements, while Wierzbicki (1985) found that believers made more errors on a test of syllogistic reasoning than disbelievers.

If it is indeed the case that believers and disbelievers have different cognitive abilities, then it may be possible to find relationships between believers/disbelievers and the MBTI and REI (especially the REI as it is specifically designed to measure individual differences in information processing). It was expected that there would be an association between belief scores (psi and PWA) and REI scores. This was essentially an exploratory hypothesis for a number of reasons. It could be argued that “believers” would be more likely to be “low faith in intuition/low need for cognition” (poor thinking style) on the REI. This prediction is based on the “cognitive deficits” hypothesis. This term was coined by Irwin (1993), and suggests that “believers” and “disbelievers” are cognitively different. Typically, this difference takes the form of “believers” being cognitively inferior to “disbelievers” (see, e.g. French, 1992; Irwin, 1993; Blackmore, 1994; and Roe, 1999). If this is the case, then respondents who are classed as having a “poor” thinking style (low faith in intuition/low rational) will be more likely to be believers. However, it might also be argued that people who are “intuitive” (high faith in intuition/low need for cognition) will believe more in psi due to their reliance on experience, intuition and associationistic connections. This is contrasted with the “rational” (low faith in intuition/high need for cognition) individuals who have a tendency to rely on logical connections and rationality, and are more likely to be “disbelievers”. Wolfradt *et al* (1999) did indeed find that “rational” individuals scored lowest on a measure of paranormal belief and experience. However, these authors also report that the group scoring highest on these measures was, in fact, the “complementary” (high faith in intuition/high need for cognition) group. It would seem that there are a number of possibilities when considering the relationship between belief in psi/PWA and the REI. As such, the investigation into the REI and belief in psi/PWA is an exploratory one.

Predictions concerning the relationship between the dimensions on the MBTI and belief in psi/PWA were similarly difficult. Although specific hypotheses were made about the sensing/intuitive dimension and *performance* on the psi/PWA tasks, it was

decided that, in studying how the MBTI might be associated with belief, all dimensions would be considered. This is because there is not as strong an argument for expecting any particular dimension to be related to *belief* as there is for expecting the intuitive/sensing dimension to be related to nonintentional psi performance. In the latter instance, it is the unconscious nature of nonintentional psi and the association between this and the MBTI's conception of "intuition" as potentially being related to unconscious processes, that is guiding the hypothesis. When considering the question of belief, the notion of the unconscious becomes less important, and as such, all MBTI dimensions will be included.

In addition to investigating the relationship between psi and PWA, and personality factors that may underlie it, the present study also allowed for a replication of the initial homophone/word association psi task used in experiment 1a. There were various interesting patterns of data found in experiment 1a. The current study aimed to replicate these, using a more advanced methodology. During the period between conducting studies 1a and 3, the experimental equipment had been upgraded. This meant that true randomisation procedures could be implemented. This would address the issue of any possible artefacts that might arise through the inferior randomisation process employed in experiment 1a (although see chapter 5 for an argument against this). Unfortunately, one aspect of study 1a that the current study was unable to follow-up was the issue of an experimenter effect. Eric Pronto was no longer available to act as a co-experimenter. Also, due to the automation of the procedure, and the fact that strict time-constraints were in place, it was decided that it would not be practical to use two experimenters due to the difficulties in scheduling participants and two experimenters. The experimenter/sender in this final study was always SW. The automation of the procedure and the fact that only one individual executed the sessions also meant that the procedure for determining "hits" and "misses" was different in the current study (see below for details).

Hypotheses

As the study was multivariate, a number of hypotheses were formulated.

Primary Hypotheses

1. There will be an overall psi effect (two-tailed).
2. Least favoured homophone responses will show more psi 'hitting' than responses that are more frequent (based on experiment 1a, again two-tailed to detect psi-missing).
3. There will be an overall PWA effect (one-tailed based on experiment 2a and previous research).
4. There will be a positive correlation between psi performance and PWA performance (two-tailed).
5. Believers in psi will perform better in the psi task than disbelievers (one-tailed).
6. Believers in PWA will perform better in the PWA task than disbelievers (one-tailed).

Exploratory Hypotheses

7. There will be gender differences in both the PWA task and the psi task. This was primarily an exploratory hypothesis, based on the suggestion in study 2b that males and females performance on psi tasks may show an interaction effect, with each gender showing an opposing pattern. Females were expected to show more psi "hits" than males in the psi task. No directional hypothesis was predicted for the PWA task.
8. "Intuitive" individuals on the MBTI will perform better at the psi task than "sensing" individuals (one-tailed).
9. "Intuitive" individuals on the MBTI will perform better at the PWA task than "sensing" individuals (one-tailed).

10. People showing High Faith in Intuition/Low Need for Cognition will perform better at the psi task than those in the other three REI groupings (one-tailed).
11. People showing High Faith in Intuition/Low Need for Cognition will perform better at the PWA task (one-tailed).
12. There will be an association between High Faith in Intuition/Low need for Cognition on the REI and the “intuitive” dimension of the MBTI.
13. There will be a positive correlation between belief in psi and belief in PWA (one-tailed).

Additionally, further exploratory analyses were planned on the relationship between belief in both psi and PWA, the four groupings of the REI (two-tailed), and the MBTI dimensions.

The present study consisted of many exploratory elements. It was felt that this was necessary in order to obtain a fuller picture of the potential relationship between psi and PWA. The present thesis is concerned with comparing these two phenomena from various angles. The previous studies set out to establish whether or not these two forms of unconscious perception could influence cognitive processes in similar ways. The current study looks at the comparison from a different angle. Instead of looking for the *same* effects, study 3 utilises the most successful protocols from the previously described experiments. As such, the comparison no longer concerns just similar effects. Instead, a comparison is made within participants. Each participant will complete both tasks. It will then be possible to make a comparison between psi and PWA in terms of the relationship between the two forms of unconscious perception within each individual. Furthermore, various exploratory measures were deemed necessary to shed further light on any possible relationship. Questions were posed concerning the way certain personality variables might relate to performance on each task. Some of these stemmed from research into the relationship between personality and unconscious processing (Woolhouse and Bayne, 2000), while some were taken from parapsychological findings concerning the effect of individual differences on psi performance (i.e. the belief measures). If it could be found that one (or more) personality variable correlated with performance on each task in similar

ways, then this would be a finding of some importance, suggesting that the two phenomena under study share common processing features.

Given the number of statistical analyses planned in the current study it is important to be aware of the potential for “capitalising on chance”. Although this is a problem, it was felt that the potential importance of the exploratory hypotheses justified their inclusion. The implications for having so many analyses are covered in the discussion section.

Method

Overview

This study used the psi word association task used in experiment 1a (the strongest psi effect) and the PWA false recognition task employed in experiment 2a (the strongest PWA effect). The purpose of using these tasks in the current study was to investigate any possible relationship between performance on both tasks, and also to examine whether similar personality variables are associated with performance on each.

The homophone word-association experiment was adapted such that all targets and stimuli were randomly presented to the participant, thus obviating any potential stacking effect. This was achieved due to the new system installed in the experimental rooms of the KPU. The homophones used were identical to those used in study 1a with one exception. Due to a recording error, the homophone “watch” could not be used, and was replaced by “flour/flower”. An additional homophone (“serial/cereal”) was also added. The recordings of the homophones were taken from the session used to record the stimuli in study 1b.

Participants

Fifty participants took part in this study. All were students or employees of the University of Edinburgh and were aged between 18 and 38. There were 14 males and 36 females. They were recruited through word of mouth, through e-mail appeals and through second year tutorial groups.

Materials and Apparatus

The self-scorable MBTI form M was used (see Myers *et al*, 1998). This consisted of 93 items, all of which had two alternative responses. Individuals were categorised in to their personality types as per the instructions in Myers *et al* (1998). This primarily consisted of assigning a score to each response, and using an answer matrix to total the scores, and work out personality type. The short version of the REI (Epstein, 1996, see appendix 4) was also used. The REI consisted of 10 items, five of which measured “faith in intuition”, and five of which measured “need for cognition”. Responses were measured on a five-point Likert scale, ranging from 1 (completely false) to 5 (completely true). Participants were divided up into high/low need for cognition and high/low faith in intuition by means of a median split on scores for each sub-scale. It was then possible to place each individual into one of the four categories described above.

A belief in psi scale was adapted from the one included in the participant information form used by the KPU (see appendix 2). This consisted of 11 items enquiring about belief in and experience of telepathy, clairvoyance, precognition and psychokinesis, as well as two general questions, one asking about belief in the existence of ESP, and one asking for a description of the participant’s psi abilities. These items were scored using a 7-point Likert scale. A score of 1 on each item represented the highest level of belief, while a score of 7 on an item represented the highest degree of disbelief. Believers and disbelievers were categorised by means of a median split. A comparable PWA scale was created based on the psi belief questionnaire. After a brief explanation of what PWA was, three questions asked about belief in and experience of being influenced outside of awareness (see appendix 3). Again, a median split was used to categorise believers and disbelievers.

Psi experiment

Homophones were recorded using the voice of Sandie Cleland, a PhD student in the psychology department. Words were recorded onto DAT (digital audiotape; Sony DTC A8 DAT recorder). The words were then transferred onto computer, in the form of .wav files. Further editing on the computer resulted in each word being preceded by 15 seconds of white noise.

Visual cues relating to each meaning of the homophones were obtained from the Internet (via the image search at www.altavista.com), and were transformed into 'jpeg' files consisting of the image, and the word it related to. The images could either be illustrations or actual photographs (see figure 5.1, chapter 5 for examples).

A computer program was written in Visual Basic to present stimuli and record responses. Auditory stimuli were presented to participants via Koss SB30 headphones. The soundcard used was a Creative Soundblaster Live card.

PWA experiment

As in experiment 2a, a total of 540 five-letter nouns were compiled from the Kucera and Francis (1967) tables of word frequency. Word frequency ranged from 3 to 60 occurrences per million, which is standard for this type of experiment. One hundred and twenty six words were randomly selected from the pool for the study list, and another 126 were randomly selected by the computer as the new words on the old/new recognition task. A further 84 words were selected to be used as the context words on the nonmatch trials of the old/new task.

Due to the fact that the study was to be conducted in a different room than study 2a, a slightly different configuration of computer equipment was used. An Iiyama Vision Master Pro 410 with a vertical refresh rate of 160 Hz was used to display the stimuli. The graphics card driving the monitor was an Matrox Millenium G400 with 32 Mb video RAM. The E-Prime® experiment generator program was used to create and run the program. The display settings were adjusted to use 256 colours, and a screen area of 640 × 480 pixels was set.

In order to ensure that the presentation time of the stimulus was accurate, a full characterisation for this equipment was conducted (see experiment 2a for details of the characterisation process).

Procedure

Participants were met by the experimenter at the front door of the psychology department and were taken by the experimenter to the experimental suite. Initial details were taken, and the participant was given the MBTI to complete. After completing this, the experimenter randomly (by tossing a coin) assigned which task the participant was to complete first (psi or PWA – details of each below). After completing the first task, the participant was given the REI to complete. Half of the subjects also completed the belief in PWA questionnaire at this point (allocated randomly).

Participants were then taken into the second experimental room (see appendix 1) where they completed the second test (psi or PWA). After completion of this task, they were given the belief in psi questionnaire. Again, half of the participants also completed the belief in PWA questionnaire at this point. Belief in psi was always administered last. This was due to the fact that the word association task was designed as a nonintentional psi task, and completing a psi questionnaire before participating may have given a cue as to the psi component in the study. Half of the participants completed the belief in PWA questionnaire before completing the PWA task, and half completed it after. This was in order to counterbalance any effect that completing the PWA questionnaire (and thus having potential knowledge of the nature of the experiment) might have had.

Psi task – Participants were taken into the “receiver’s room” room (see appendix 1) and told to make themselves comfortable. It was then explained to them that they would hear fifteen seconds of white noise followed by a word. They were instructed to relax during the white noise, and then to say aloud, and as clearly as possible, the first associated word that came to mind after hearing the word. The experimenter

then answered any questions before starting the experiment in the experimental room. There was a slight delay in the commencement of the trials in order to give the experimenter time to lock the door to the experimental room, and take up his position in the sender's room.

For each trial, the computer randomly selected a homophone (which had two primary interpretations), and randomly selected one of these interpretations as a "target". This target interpretation and a visual image related to it were displayed to the sender (always SW) during the period in which the participant was listening to the white noise. As the sender was viewing this image, he attempted to influence the participant's response to the homophone in the direction of the target interpretation. After the white noise was over, the computer began recording the participant's response². Once the session was over, the experimenter went back down to the experimental suite, where he informed the participant that this part of the experiment was over.

PWA task – This was essentially identical to that described in study 2a.

Participants were told to sit down and make themselves comfortable, and they were requested to adjust the height of the seat so that their eye level was at the centre of the computer screen. The experimenter then verbally described the task, giving the participant an opportunity to ask any questions he or she might have had. The experimenter then started the program running. At this point, the experimenter retired from the room. The participant was presented with written instructions on the screen describing the task. The main experiment was in two parts. The first part involved presenting a study list of 126 words, at a rate of 1 per second, with each word presented for 500 ms followed by a 500-ms blank field. Participants were told to

² As a safeguard, a tape recorder was also present in the participant's room in order to record the responses that might be lost as a result of a computer failure. On several occasions the computer failed to record responses, and this tape recording was used. No data was lost as a result of computer failure.

silently read each word to themselves and that their memory for these words would be tested in the second part of the experiment.

The second part of the experiment consisted of the old/new recognition task. Each trial had the following structure: (a) masking stimulus (££££££££) presented for 500 ms, (b) a context word presented for either 50 ms or 114 ms, (c) masking stimulus presented again for 500 ms, and (d) a test word presented until a response was made. All stimuli were size 18 point and presented at a location in the centre of the screen. Participants were asked to read the context word to themselves if possible and then to respond “old” or “new” to the test word with respect to the initial study list. Recognition memory was tested in three contexts: (a) match (context word and test word are identical; e.g., *arena* – *arena*); (b) nonmatch (context word and test word are different, and the context word is a new word; e.g., *tribe* – *arena*); and (c) baseline (context word was the letter string *xoxoxox*). For all three of these contexts, the test words were old on half of the trials and new on the remaining trials. This was determined randomly by the computer.

The old/new recognition test consisted of 12 practice trials followed by 240 experimental trials. Practice trials had four exemplars of each of the three contexts. The six old test words used for the practice trials were the first and last three words of the study list. The experimental trials consisted of 120 trials in which the context word was displayed for 50 ms and 120 trials in which the context word was presented for 114 ms. There were never more than 3 consecutive trials when the same duration was used.

Once testing was complete, the experimenter informed the participant that this part of the experiment was over.

After completing both tasks and all the questionnaires, the participant was given the chance to ask the experimenter any questions. These mainly took the form of queries about the nature of the study, which the experimenter answered as best as he could. No participant indicated that he/she had worked out what either experiment was

studying. Although some participants were surprised at the psi component, none of these participants expressed any negativity about having taken part in a psi study. After completion of the study, all participants were e-mailed and fully debriefed about the nature of the study, and were given feedback on their MBTI scores.

Classifying responses

It was important that the classification of responses into “hits” and “misses” was carried out blind. This meant that the experimenter could not take part in this process. Two judges were used to carry this out. The first judge listened to all the responses and noted them down (with no prior knowledge of either the homophone or the target interpretation). Any ambiguous responses were given to the second judge, who gave her opinion on what the word was. This was usually enough to come to a consensus, but in two cases the response was too unintelligible to make out, and was discarded.

The two judges were then given details of what the targets and what the homophones were for each session. Based on this, each judge had to class each response depending upon whether it was a “hit” (response matches target interpretation), a “miss” (response matches the non-target interpretation) or “neither” (response does not match either interpretation). Although initial agreement was high, it was necessary to get a consensus between the judges. The experimenter then collated the trials on which the judges disagreed, asked each judge for an explanation as to why they had classified as they did and re-presented these to the judges. It became clear at this point that many of the disagreements between the judges were simply due to mistakes made in the judging procedure. There were some instances where a judge’s knowledge caused them to rate an item as a “hit” while the other judge rated the same item as a “neither”. For example, the homophone “been/bean” often elicited the response “pie”. One rater had never heard of a “bean-pie”, so classed these responses as “neither”. However, when the other rater explained what a bean-pie was, then an agreement was reached. Such instances were clarified during this procedure, and a general consensus was reached. After this, there were only eight instances where the

judges still disagreed. These were subsequently given to a third party who made a final decision.

Results

Overall Psi scores

As the target interpretation in each trial was selected randomly by the computer, the probability of obtaining a hit by chance is 0.5. It should be noted that this is true regardless of any biases that might exist towards or away from particular responses.

As in experiment 1a, the analysis was conducted on all the “appropriate” responses (i.e. responses that related to one of the two prescribed meanings of the homophone). Overall, there were 905 appropriate responses, of which there were 454 hits and 451 misses. This results in a z-score of 0.1, which is not significant ($p = 0.92$, two tailed).

Response Bias

As in experiment 1a, the responses that were favoured least within our sample were isolated and analysed. Overall there were 185 minority responses, of which 93 were hits and 92 were misses. This is almost exactly as chance would predict, and is not significant ($z = 0.07$ $p = 0.94$, two tailed).

A “psi-proportion” was then calculated for each participant by dividing the number of hits they achieved by the total number of appropriate responses they gave (i.e. hits + misses).

Gender differences on Psi task

Table 7.1 suggests that females score more hits on the psi task than males. An independent samples t-test was carried out to determine whether the difference between males and females was significant. This hypothesis (number 7) was directional, due to the trends observed in the previous studies. The t-test revealed that the difference was marginally significant ($t(48) = 1.64$, $p = 0.053$, one-tailed).

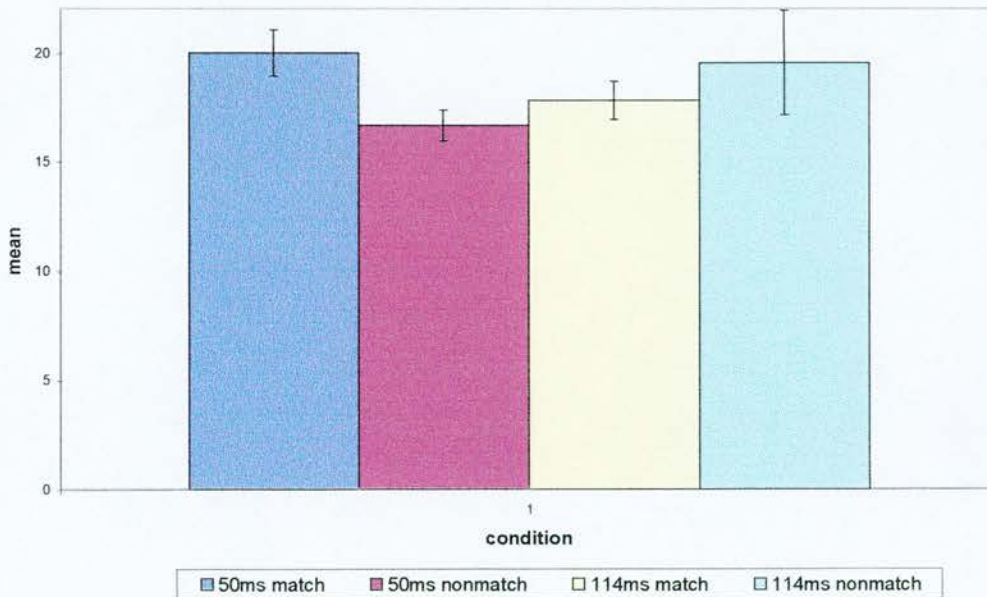
Table 7.1 Psi proportions and standard deviation for males and females.

	Mean Psi Score (hits/hits + misses)	Standard deviation
Males (N = 14)	0.46	0.12
Females (N = 36)	0.51	0.11

PWA False Recognition.

Figure 7.1 suggests that a similar interaction to that observed in experiment 2a was present in this data.

Figure 7.1. Mean Responses of 'Old' responses in 4 conditions (+ error)



A 3 x 2 repeated measures ANOVA revealed a significant main effect of context ($F(2, 98) = 5.14, p < 0.01$) and a significant interaction between duration and context ($F(2, 98) = 3.61, p < 0.05$). Further analysis of this interaction revealed that in the 50ms condition, there were significantly more 'old' responses to new words in the matched context than in the non-matched context ($t(49) = 4.637, p < 0.001$, one-tailed). This

replicates the previous findings in experiment 2a and demonstrates the predicted false recognition effect.

PWA Indices

Due to the fact that PWA in this study is assessed by an interaction, then there is no absolute measure of PWA performance that can be compared to the psi-score. However, it is possible to create a measure in a number of different ways. Given the fact that the interaction is significant, then it can be safely argued that PWA was present in the data. If it was present, then we must decide where it is most likely to show up. There are a number of possibilities, and so three separate measures of PWA performance were calculated.

Index 1 - The first involved dividing the total number of “old” responses in the 50ms condition (both match and nonmatch) by the number of “old” responses to new words in the 50ms/match condition. This was based on the fact that the strict definition of PWA in this study was the number of “old” responses to new words in the match/50ms condition. This method takes gives a measure that takes into account this strict definition in relation to the total number of old responses in the 50ms condition.

Index 2 - However, it could be argued that we need to also consider the number of “old” responses to new words in the 50ms condition as a function of the total number of “old” responses in *all* conditions. The second measure we created did this, by dividing the number of old responses in the match/50ms condition by the total number of old responses. This measure, therefore, took into account the *total* number of old responses throughout the session, and the created an index based on the number of old responses in the 50ms (i.e. outside of awareness) trials. It does not, however, take into account the match and nonmatch conditions.

Index 3 - The final measure of PWA performance that was calculated takes into consideration the number of match/50ms trials that the participant completed. Thus,

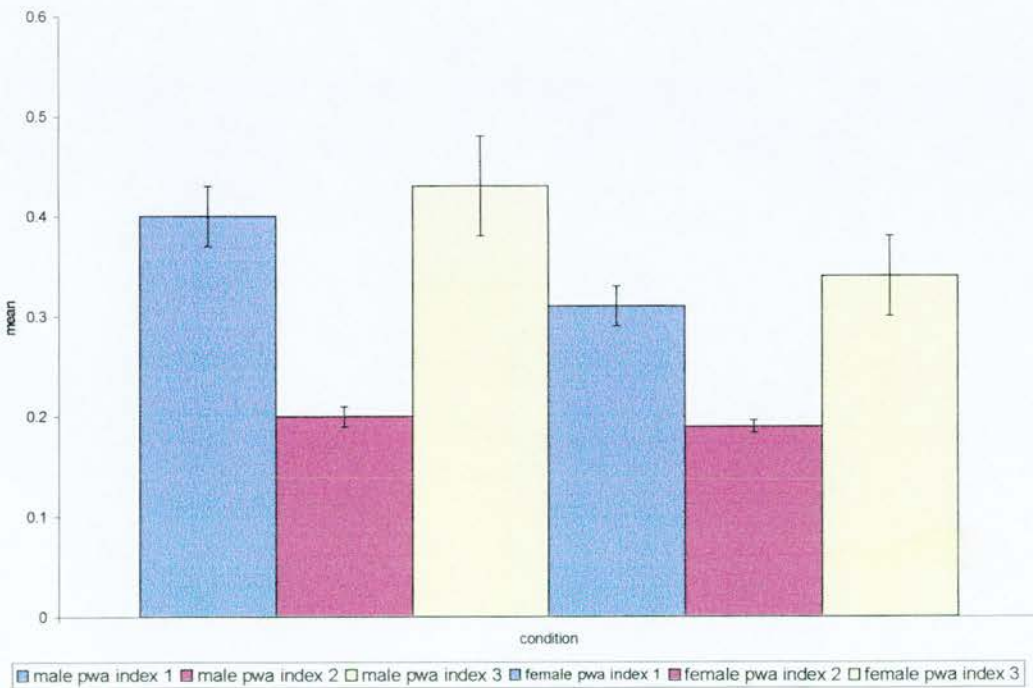
this measure took the number of old responses to new words in the 50ms condition, and divided this by the total number of trials in this condition (i.e. 20). This measure was based on the trials in which false recognition would be hypothesised to occur, i.e. when the context word and test word matched, and when the context word was presented for 50ms. As there were 20 of these trials in the session, the third measure creates an index based on the proportion of these 20 trials in which an “old” response was made.

All of the PWA indices correlated positively with each other, and these correlations were significant at beyond the 0.01 level.

Gender effects on PWA

Using these indices, it was possible to analyse the effect of gender on PWA performance. Figure 7.2 shows that males tended to show higher levels of PWA than females.

Figure. 7.2. Bar graph showing levels of PWA in males and females for each of three PWA indices (+ error).



A multivariate analysis of variance was conducted on each index. This revealed a significant gender effect on the first PWA index, but not on any of the others. On all of the indices, females were shown to display a higher mean than males.

PWA index 1: $F(1, 48) = 4.57, p = 0.04$.

PWA index 2: $F(1, 48) = 0.78, p = 0.38$.

PWA index 3: $F(1, 48) = 2.11, p = 0.15$.

When the effect of gender on PWA index 1 was explored further, a significant difference was found between males and females in their PWA scores as measured by this index ($t(48) = 2.14, p = 0.04$, two-tailed). This suggested that males scored significantly higher on this measure of PWA than females.

Correlation between psi and PWA.

The indices were then used to determine whether any of the PWA measures correlated with psi performance. None of the subsequent correlations were significant, although all of the correlations were negative, suggesting that, the higher the psi score, the lower the PWA score.

PWA index 1: Pearson's $r = -0.11, p = 0.45$, two-tailed.

PWA index 2: Pearson's $r = -0.09, p = 0.56$, two-tailed.

PWA index 2: Pearson's $r = -0.04, p = 0.76$, two-tailed.

Gender differences in psi/PWA correlations.

When males ($N = 14$) were considered separately, it was revealed that the second and third indices were significantly negatively correlated with psi scores.

PWA index 1: Pearson's $r = -0.488, p = 0.08$, two tailed.

PWA index 2: Pearson's $r = -0.534, p = 0.05$, two-tailed

PWA index 3: Pearson's $r = -0.596, p = 0.02$, two-tailed.

When females ($N = 36$) were considered separately, none of the psi/PWA correlations were significant (though they were all positive).

PWA index 1: Pearson's $r = 0.08$, $p = 0.64$, two-tailed.

PWA index 2: Pearson's $r = 0.12$, $p = 0.51$, two-tailed.

PWA index 3: Pearson's $r = 0.23$, $p = 0.18$, two-tailed

Personality Variables

Although there was no evidence of psi in the overall data, it is still useful to look at how personality variables might relate to psi performance. This is due to the possibility that the overall non-significant psi results were caused by the tendency of certain personality types to score well in the psi task, while others may cancel out this psi-hitting by scoring *below* chance (i.e. psi missing).

Believers/Disbelievers Scores

Believers and Disbelievers on both the psi and PWA questionnaires were determined by means of a median split. Those scoring below the median were classed as disbelievers, while those scoring on or above the median were classed as believers. The mean scores of psi believers and disbelievers on the belief questionnaire can be seen in table 7.2 below. The mean scores of PWA believers and disbelievers are shown in table 7.3. For both measures of belief a lower score indicates a higher degree of belief.

Table 7.2 – Psi believers and disbelievers mean scores on psi belief questionnaire (and standard deviations).

	Mean psi belief score	N	s.d.
Psi Believers	35.8	25	6.42
Psi Disbelievers	59.4	25	6.39

Table 7.3 – PWA believers and disbelievers mean scores on PWA belief questionnaire (and standard deviations).

	Mean PWA belief score	N	s.d.
PWA believers	5.24	25	1.56
PWA disbelievers	10.24	25	3.17

Psi Believers/Disbelievers' psi performance

Table 7.4 shows there is little difference in the psi scores for believers and disbelievers. An analysis of variance revealed no main effect when believers and disbelievers were compared for psi performance ($F(1, 46) = 1.72, p = 0.19$). There was also no gender x believers/disbelievers interaction ($F(1, 46) = 1.92, p = 0.16$).

Table 7.4 – Mean psi scores for psi believers and psi disbelievers (and standard deviations).

	Mean psi score	N	s.d.
Believers	0.51	25	0.12
Disbelievers	0.48	25	0.12

PWA believers/disbelievers' PWA scores

Again, classification into believers and disbelievers categories for the PWA belief questionnaire was achieved by a median split.

Inspection of the means in table 7.5 below reveals that the means for all three PWA indices for PWA believers and disbelievers were very similar.

Table 7.5: PWA believers and disbelievers performance on the PWA task measured by three PWA indices.

		PWA index1	PWA index 2	PWA index 3
PWA believers	<i>Mean</i>	0.33	0.19	0.36
	N	25	25	25
	s.d.	0.13	0.04	0.20
PWA disbelievers	<i>Mean</i>	0.33	0.19	0.37
	N	25	25	25
	s.d.	0.15	0.03	0.22

Analysis of variance revealed no significant effect of belief on any of the three PWA indices.

PWA index 1: $F(1, 48) = 0.02, p = 0.62$.

PWA index 2: $F(1, 48) = 1.96, p = 0.16$.

PWA index 3: $F(1, 48) = 1.5, p = 0.22$.

There were also no gender x PWA belief interactions in any of the indices.

PWA index 1: $F(1, 48) = 2.66, p = 0.08$.

PWA index 2: $F(1, 48) = 2.10, p = 0.13$.

PWA index 3: $F(1, 48) = 1.80, p = 0.17$.

In summary, there was no indication that believers psi/PWA differed on performance in the respective tasks.

MBTI

Although the main MBTI factor that was of interest was the Sensing/Intuitive dimension, it was important to determine whether any of the other dimensions were associated with the SN scale in such a way that they might be moderating variables. Chi-square tests were conducted, and it was found that the only other scale that was associated with SN was the judging-perceiving (JP) scale ($\phi = 0.287, p = 0.04$). Thus, in all the subsequent analyses using the SN scale, the JP scale was factored in as a controlling variable. If any of the analyses revealed a significant interaction between SN and JP, then it would mean that the JP scale was a moderating variable in the relationship between SN and the dependent variable under study (e.g. psi score). Gender was also factored into the analyses.

MBTI and psi-score

It was hypothesised that people who viewed themselves as intuitive would perform better at performing the non-intentional unconscious psi task. Table 7.6. shows that there are no substantial differences between the sensing and intuitive groups.

Table 7.6: Mean psi scores for sensing and intuitive types (and standard deviations)

	Mean	s.d.	N
Intuitive	4.97	0.13	37
Sensing	5.03	0.09	13

An analysis of variance was carried out, with SN, JP, and gender as factors. This revealed no main effect of the SN dimension ($F(1, 44) = 0.22, p = 0.37$). The interaction between SN and JP was also nonsignificant, indicating that JP was not a

moderating variable ($F(1, 44) = 0.083, p = 0.775$). Additionally, the SN x JP x gender interaction was also found to be nonsignificant ($F(1, 44) = 2.278, p = 0.12$).

MBTI and PWA score

It was hypothesised that intuitive types on the MBTI would show a higher degree of sensitivity to the PWA stimuli, leading to higher PWA scores (as measured by the three previously described indexes).

A multivariate analysis of variance was conducted. This did not reveal any differences between sensing and intuitive individuals on any of the indices.

PWA index 1: $F(1, 44) = 2.67, p = 0.11$.

PWA index 2: $F(1, 44) = 1.583, p = 0.23$.

PWA index 3: $F(1, 44) = 2.2, p = 0.15$.

No SN x JP interaction was found on any of the indices:

PWA index 1: $F(1, 44) = 1.73, p = 0.19$.

PWA index 2: $F(1, 44) = 0.25, p = 0.78$.

PWA index 3: $F(1, 44) = 0.28, p = 0.76$.

Finally, no SN x JP x gender interaction was found on any of the indices.

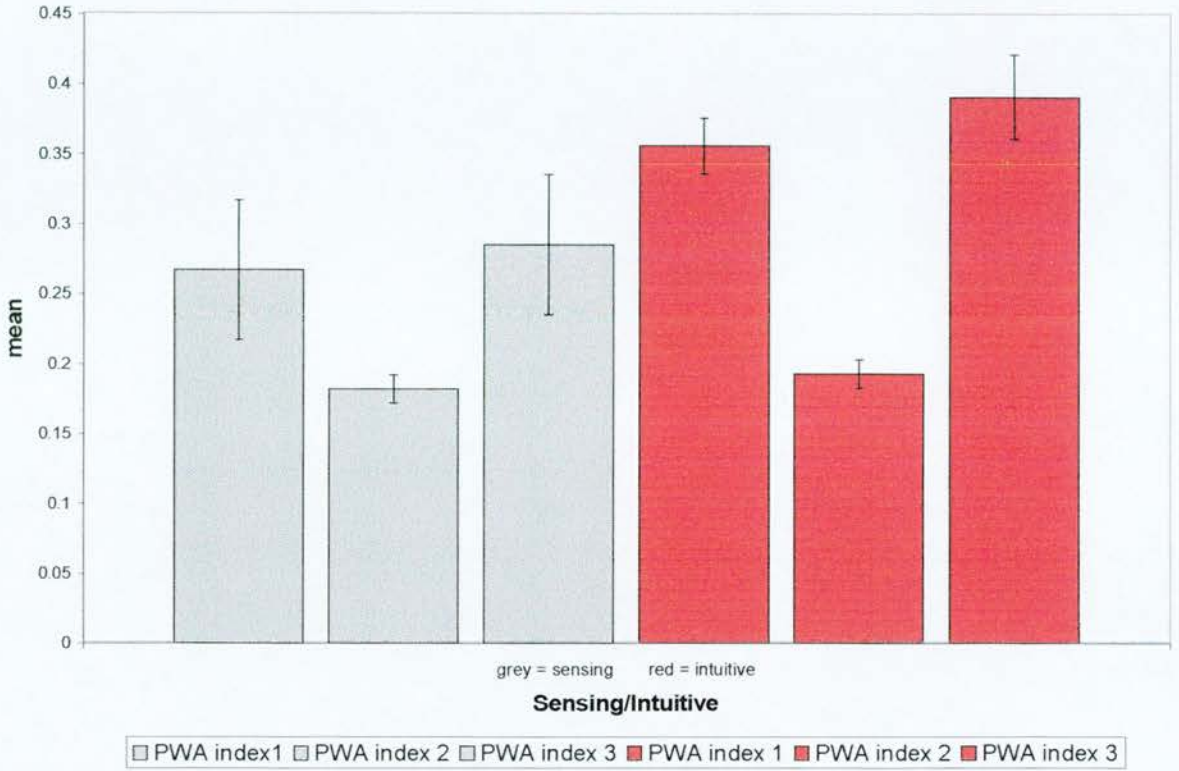
PWA index 1: $F(1, 44) = 1.07, p = 0.35$.

PWA index 2: $F(1, 44) = 2.75, p = 0.08$.

PWA index 3: $F(1, 44) = 0.99, p = 0.38$.

As can be seen from figure 7.3, intuitive individuals show higher PWA scores on all the indices than sensing individuals.

Fig 7.3: Mean PWA scores for sensing and intuitive individuals on each PWA index.



In summary, there did not appear to be any relationship between the sensing/intuitive dimension of the MBTI and either psi or PWA performance.

REI

The internal consistency (α) coefficients of the 5-item versions of the need for cognition (NFC) and faith in intuition (FI) scales were 0.73 and 0.82 respectively. In the latter case, this is considerably higher than Epstein (1996), while in the former it is identical. A principal components factor analysis with varimax rotation was also conducted, revealing two factors. The NFC items loaded highly on the first factor, accounting for 32.2% of the variance, while the FI items loaded highly on the second factor, accounting for 24.85% of the variance. Table 7.7 displays the factor loadings

for each item. This confirms the finding by Epstein et al (1996) suggesting that the 10-item REI is a valid measure of FI and NFC, and they are distinct factors.

Table 7.7 – Factor loadings of the REI items (values in bold type indicate highest factor loading for each item) (r) = reverse scored.

REI scale and item	Factor	
	1	2
<i>Need for Cognition</i>		
I don't like to have to do a lot of thinking (r)	0.53	0.16
I try to avoid situations that require thinking in depth about something (r)	0.67	0.42
I prefer to do something that challenges my thinking abilities rather than something that requires little thought	0.80	-0.12
I prefer complex to simple problems	0.68	-0.17
Thinking hard and for a long time about something gives me little satisfaction (r)	0.78	-0.06
<i>Faith in Intuition</i>		
I trust my initial feelings about people	0.04	0.84
I believe in trusting my hunches	0.09	0.79
My initial impressions of people are almost always right	0.01	0.75
When it comes to trusting people, I can usually rely on my "gut feelings"	0.01	0.81
I can usually feel when a person is right or wrong even if I can't explain how I know	-0.015	0.66

No gender differences were found on either of the scales ($t(48) = 0.97$, $p = 0.34$; $t(48) = 0.79$, $p = 0.43$ for the FI and NFC scales respectively, two-tailed tests).

It was hypothesised that individuals showing high faith in intuition would show higher degrees of PWA and psi. Further, it was hypothesised that individuals who were high faith in intuition/low need for cognition would show this effect more prominently.

REI grouping

Each individual's REI score was determined in the following way. Raw faith in intuition (FI) and need for cognition (NFC) scores were calculated. A median split was then performed and, on each scale, those scoring above the median were classified as "high" and those below the median were classified as "low". Thus, each individual could either be high or low on each of the dimensions (FI and NFC). Each participant was then assigned to one of four groups

1. High NFC, High FI
2. High NFC, Low FI
3. Low NFC, High FI
4. Low NFC, Low FI.

The mean scores for each of these groups on the FI and NFC scales can be seen in table 7.8

Table 7.8 Mean scores on FI and NFC scales for each of the four REI groupings.

REI grouping		FI score	NFC score
High FI/High NFC	Mean	20.33	18.45
	s.d.	1.56	1.55
	N	24	
Low FI/High NFC	Mean	14.4	18.1
	s.d.	3.43	1.20
	N	10	
Low FI/Low NFC	Mean	16.00	14.88
	s.d.	1.31	1.36
	N	8	
High FI/Low NFC	Mean	21.25	15.25
	s.d.	1.91	1.04
	N	8	

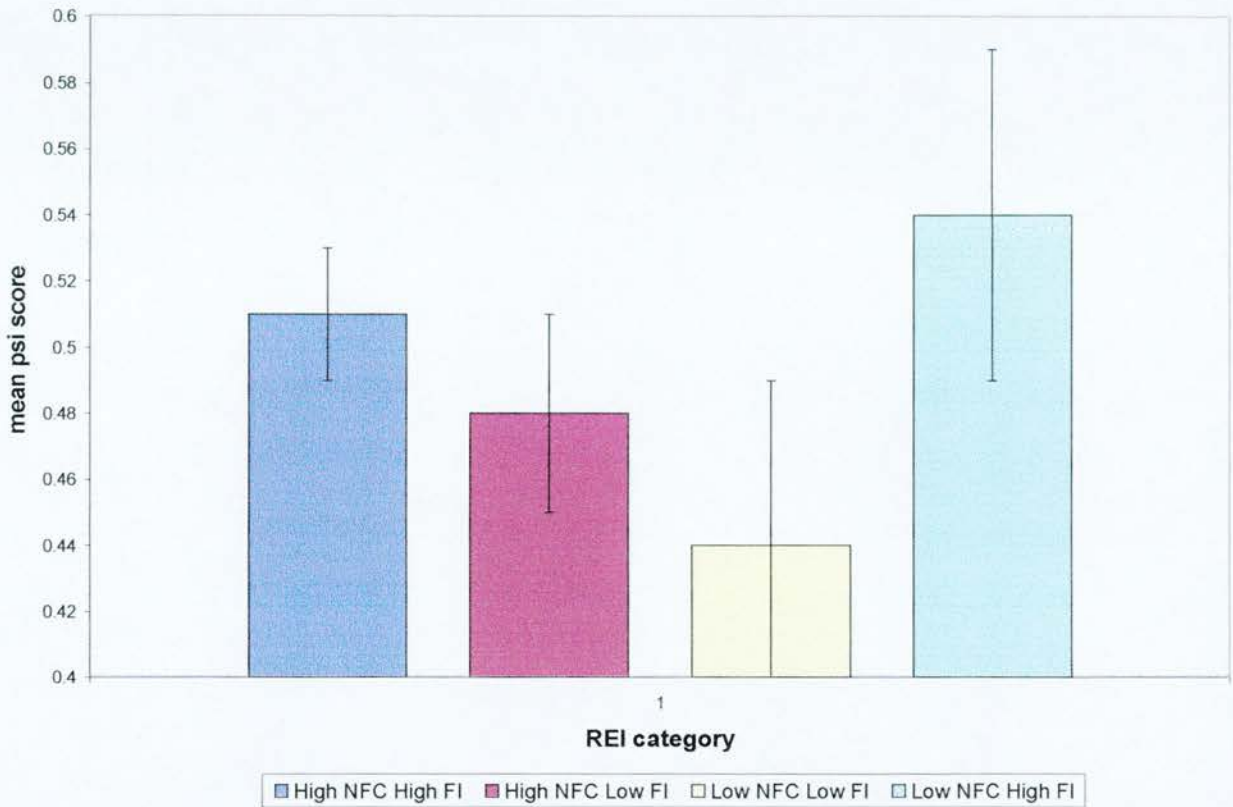
REI and psi

It was hypothesised that individuals who were low in NFC but high in FI would show higher psi scores than the other three groups.

As can be seen from figure 7.4, the hypothesis looks to have been supported to a certain extent, with low NFC/high FI individuals showing high psi scores. The lowest scores were in the low NFC/low FI group.

An analysis of variance revealed that the main effect of REI grouping was not significant ($F(3, 42) = 2.05, p = 0.12$). No interaction between gender and REI grouping was observed ($F(3, 42) = 1.23, p = 0.31$).

Figure 7.4: Psi scores for each of 4 REI groupings.



REI and PWA

Similar hypotheses were made concerning the REI/PWA relationships as were made for the REI/psi relationships. Table 7.9 shows the means of the PWA scores in each index for the four groupings of the REI. There does not seem to be much difference between the REI groups on any of the indices. An analysis of variance revealed no main effects for REI group on any of the three PWA indices.

PWA index 1: $F(3, 42) = 0.38, p = 0.77$.

PWA index 2: $F(3, 42) = 0.70, p = 0.56$.

PWA index 3: $F(3, 42) = 0.61, p = 0.61$.

Table 7.9 REI groupings and PWA scores (with standard deviations)

REI Grouping		PWA index1	PWA index2	PWA index3
High FI/High NFC	Mean	0.32	0.19	0.36
	s.d.	0.12	0.04	0.21
	N	24	24	24
Low FI/High NFC	Mean	0.35	0.20	0.34
	s.d.	0.12	0.03	0.18
	N	10	10	10
Low FI/Low NFC	Mean	0.35	0.20	0.41
	s.d.	0.18	0.06	0.27
	N	8	8	8
High FI/Low NFC	Mean	0.33	0.17	0.34
	s.d.	0.18	0.05	0.27
	N	8	8	8

There was also no interaction between REI grouping and gender on any of the three PWA indices.

PWA index 1: $F(3, 42) = 1.55, p = 0.21$.

PWA index 2: $F(3, 42) = 0.51, p = 0.73$.

PWA index 3: $F(3, 42) = 0.85, p = 0.5$.

Relationship between psi belief and PWA belief

Table 7.10 indicates that there is not much association between psi belief and belief in PWA. A chi-square was conducted, and this revealed no significant association (chi-square (1) = 0.8, $p = 0.39$)

Table 7.10: Numbers of believers/disbelievers on both psi and PWA questionnaires

		Psi belief		
		Believers	Disbelievers	Total
<i>PWA belief</i>	<i>Believers</i>	13	12	25
	<i>Disbelievers</i>	12	13	25
	<i>Total</i>	25	25	50

A correlation was also performed on the raw scores in each of the belief questionnaires, and, again, this did not reveal a significant association between psi belief and PWA belief (Pearson's $r = 0.16$, $p = 0.28$, two-tailed)

Relationship between psi belief and REI grouping

These were exploratory hypotheses. Table 7.11 demonstrates that there are no striking trends between the REI groupings and belief in psi. There are slightly more believers in the low NFC/ high FI group and there were slightly more disbelievers in the high NFC/low FI group. A chi-square reveals that these associations are not significant (chi-square (3) = 4.27, $p = 0.23$).

Table 7.11: Contingency table showing association between belief in psi and REI grouping.

		<u>Psi belief</u>		<u>Total</u>
		Believers	Disbelievers	
<u>REI grouping</u>	HiNFC/HiFI	11	13	24
	HiNFC/LoFI	3	7	10
	LoNFC/LoFI	5	3	8
	LoNFC/HiFI	6	2	8
<u>Total</u>		25	25	50

Relationship between PWA belief and REI grouping

Inspection of table 7.12 does not suggest any relationship between belief in PWA and REI grouping. This is confirmed by a chi-square (chi-square (3) = 4.27, p = 0.23).

Table 7.12: Belief in PWA and REI grouping.

		<u>PWA belief</u>		<u>Total</u>
		Believers	Disbelievers	
<u>REI Grouping</u>	Hi NFC/Hi FI	11	13	24
	Hi NFC/Lo FI	7	3	10
	Lo NFC/Lo FI	5	3	8
	Lo NFC/Hi FI	2	6	8
<u>Total</u>		25	25	50

Relationship between MBTI and psi belief.

Again, these were exploratory hypotheses, and no specific predictions were made. Table 7.13 suggests that there is no real relationship between the thinking and feeling scale of the MBTI and belief in psi. This is confirmed by a chi-square (chi-square (1) = 0.85, p = 0.77).

Table 7.13: Relationship between belief in psi and TF scale of MBTI.

	<u>Belief in psi</u>			<u>Total</u>
		Believers	Disbelievers	
MBTI dimension	Thinking	9	10	19
	Feeling	16	15	31
Total		25	25	50

Again, table 7.14 does not suggest a relationship between judging and perceiving and belief in psi. This is supported by a chi-square (chi-square (1) = 0.76, p = 0.38).

Table 7.14: Relationship between belief in psi and JP scale of MBTI.

	<u>Belief in psi</u>			<u>Total</u>
		Believers	Disbelievers	
MBTI dimension	Judging	8	11	19
	Perceiving	17	14	31
Total		25	25	50

Table 7.15 suggests that is no relationship between extroversion/introversion and belief in psi. This is confirmed by a chi-square (chi-square (1) = 0.37, p = 0.54).

Table 7.15: Relationship between belief in psi and EI scale of MBTI.

	<u>Belief in psi</u>			<u>Total</u>
		Believers	Disbelievers	
MBTI dimension	Extroverted	18	16	34
	Introverted	7	9	16
Total		25	25	50

Table 7.16 does not reveal any relationship between the SN scale of the MBTI and belief in psi (chi-square (1) = 0.10, p = 0.74)

Table 7.16: Relationship between belief in psi and SN scale of MBTI.

	<u>Belief in psi</u>			<u>Total</u>
		Believers	Disbelievers	
MBTI dimension	Sensing	6	7	13
	Intuitive	19	18	37
Total		25	25	50

Relationship Between MBTI and PWA Belief

All analyses concerning MBTI and belief in PWA are two-tailed.

Table 7.17 suggests that there may be an association between belief in PWA and the Thinking/Feeling dimension of the MBTI. This is confirmed by a significant chi-square (chi-square (1) = 4.16 p = 0.04).

Table 7.17: Relationship between belief in PWA and TF scale of MBTI.

	<u>Belief in PWA</u>			<u>Total</u>
		Believers	Disbelievers	
MBTI dimension	Thinking	13	6	19
	Feeling	12	19	31
Total		25	25	50

Table 7.18 indicates that people who are classified as perceiving on the MBTI may be more likely to believe in the reality of PWA. This was not supported by a chi-square (chi-square (1) = 0.76, p = 0.38).

Table 7.18: Relationship between belief in PWA and JP scale of MBTI.

	<u>Belief in PWA</u>			<u>Total</u>
		<u>Believers</u>	Disbelievers	
MBTI dimension	Judging	8	11	19
	Perceiving	17	14	31
Total		25	25	50

Table 7.19 does not suggest a relationship between belief in PWA and extroversion/introversion and this is supported by a nonsignificant chi-square (chi-square (1) = 0.47, p = 0.23).

Table 7.19: Relationship between belief in PWA and EI scale of MBTI.

	<u>Belief in PWA</u>			<u>Total</u>
		<u>Believers</u>	Disbelievers	
MBTI dimension	Extroverted	15	19	34
	Introverted	10	6	16
Total		25	25	50

Table 7.20 suggests that intuitive types may be more likely to be believers than disbelievers. This is not significant (chi-square = 2.6, p = 0.11).

Table 7.20: Relationship between belief in PWA and TF scale of MBTI.

	<u>Belief in PWA</u>			<u>Total</u>
		<u>Believers</u>	Disbelievers	
MBTI dimension	Sensing	4	9	13
	Intuitive	21	16	37
Total		25	25	50

Relationship between SN dimension and REI grouping.

A relationship between the SN dimension of the MBTI and the REI was expected, due to them sharing the concept of “intuition” as a common factor. The table below, however, does not indicate any association between the SN dimension of the MBTI scale and the REI grouping. This is confirmed by a chi-square (chi-square (3) = 3.57, $p = 0.31$).

Table 7.21: Relationship between SN scale of MBTI and REI grouping.

		Sensing or Intuitive		Total
		sensing	intuitive	
REI grouping	Hi NFC/Hi FI	4	20	24
	Hi NFC/Lo FI	3	7	10
	Lo NFC/Lo FI	4	4	8
	Lo NFC/Hi FI	2	6	8
Total		13	37	50

Discussion

Before discussing the results, it is important to note that the number of analyses conducted in the current study increases the possibility of finding significant results by chance alone. It is important to keep this in mind when considering the findings. In total, there were 63 p-values reported in the above analysis. This means that, theoretically, we can expect around three of these to be significant at the 0.05 level by chance alone³. In total, ten of the 63 p-values were significant. Of these ten, three relate to the replication of the PWA false recognition effect. If we assume that this is a valid effect (as it replicates both previous work in the field and experiment 2a of the current thesis), then this still leaves seven significant p-values from a remaining

³ This is a conservative estimate, assuming that all the p values are statistically independent. This may not be the case, although for present purposes we will use this conservative estimate.

total of sixty. It is possible, therefore, that some of these significant effects are the result of “capitalising on chance” and do not reflect real effects. It should be pointed out that, were a more conservative critical p-value adopted (e.g. 0.01) then none of the remaining p-values would reach significance. For these reasons, great caution must be placed on any significant results, particularly those which are marginally significant.

The purpose of experiment 3 was to look at the most promising aspects of the previous studies, investigate how they might relate to each other and look at what factors may underlie any potential relationships between them. The measures used were the homophone/word association psi task and the PWA false recognition task. In addition, this study also served as an opportunity to replicate the PWA effect found in experiment 2a and the psi response bias effect found in experiment 1a, which itself was a post-hoc finding. Several personality variables were included, and hypotheses were made based on these concerning performance on both the psi and PWA tasks. Further hypotheses were formulated concerning the ways in which these personality variables might relate to each other.

Psi, Response Bias and Gender

Overall there was no psi effect and the response bias effect was not evident. It is difficult to speculate on why this may be the case. The design of the current study was an improvement over experiment 1a, with better randomisation procedures implemented. This may have led to the elimination of any spurious effect that was due to the randomisation techniques used in experiment 1a. However, given the analysis conducted on the data from experiment 1a, it is difficult to see where any potential artefact in the initial study would manifest itself, as the lack of true randomisation in experiment 1a did not seem to lead to any consistent patterning of responses. It is, therefore, unclear whether the randomisation in the current study actually did “iron-out” an artefact from experiment 1a. It may be the case that the effects found in experiment 1a were a statistical anomaly (i.e. a type 2 error) that was eliminated in the replication by the increased number of trials.

Another possible explanation for the lack of a psi effect in the replication is the participant pool used. In experiment 1a, all the participants were known to the experimenter (SW). Participants in experiment 3 were recruited through e-mail appeals, and most were not known to the experimenter prior to testing. It is possible that motivation issues played a part, and that the participants in experiment 1a were more motivated to do well. A meta-analysis on early ganzfeld research (Honorton *et al*, 1990) seemed to suggest that sender-receiver pairs who were friends performed better than strangers (although this trend was not observed in a meta-analysis of later ganzfeld work; Bem and Honorton, 1994). It would seem likely that interpersonal factors are important in parapsychological research. This issue deserves further study, with relationships between sender-receiver pairings being more closely scrutinised. One potentially important factor is that the previous work looking at sender-receiver pairings has focused on intentional psi tasks. It is difficult to predict how the relationship between the sender and receiver might influence non-intentional psi, where the receiver is not aware that he/she is taking part in a psi task, or that a sender even exists. One might expect that, in this situation, the relationship between the two would be less critical than in intentional psi tasks, but, again, this is something that might benefit from closer scrutiny.

Related to this issue is the gender effect. There was a slight difference between males and females in the psi task, with females displaying higher psi scores than males. This effect *may* be the result of the multiple analyses problem noted above. However, the possibility of gender differences in a psi task was raised in experiment 2b, in which males and females showed differing response patterns to psi stimulation. So, although treating this finding with caution, it does make some sense in light of the previous experiment. Again, previous research in intentional psi tasks such as the ganzfeld has suggested that male/female sender-receiver pairs tend to be the most successful (see, e.g. Dalton, 1994; Dalton and Utts, 1995). As before, it is difficult to speculate how this effect might be explained in the context of non-intentional psi, although the effect certainly seems worthy of further investigation (see following chapter for a fuller discussion).

The response bias effect, whereby less frequent responses tend to result in more psi “hits”, was not evident in the current study. This is unfortunate, as the pattern observed in experiment 1a seemed to agree with Stanford’s (1973) findings on this issue (although Stanford’s response bias effect was observed using stimuli that were not homophones). As in experiment 1a, the method used to determine “minority responses” may be criticised, as it did not take population norms into account. Instead, a baseline measure was obtained from the participant pool in the study. Stanford (1973) used a measure of the participants’ tendency to respond with pre-defined “minority” responses as a means of testing the response bias hypothesis. The current study did not have any measure of individual dispositions or of predefined minority responses, instead relying on the overall responses of the participant pool to determine which interpretations were less common in that particular population. While this may be a valid approach, future work might include some pilot work aimed at predefining the minority responses in the population for the stimuli to be used.

PWA, gender and the psi/PWA relationship

The PWA false recognition effect was successfully replicated, suggesting that this is a robust effect. There was some suggestion that there may have been a gender difference in this task, although the effect was only apparent on one of the PWA indices and must therefore be treated with caution. Nothing in the recent PWA literature addresses the possibility that males and females may differ in their performance on PWA tasks, although this is not a hypothesis that has been tested.

The correlation between psi and PWA was non-significant, although some interesting trends appeared when the data were split up by gender. When males were considered separately, it was found that the correlation between PWA and psi performance was negatively correlated (this correlation was significant at the 0.05 level on two of the indices). When females were considered separately, all three correlations between psi and PWA were positively correlated, although none of the correlations reached significance. Once again, the problem of multiple analyses places the significance found in question. However, bearing this in mind, it is still interesting that the

direction of the correlations differed for males and females. If this is a real effect, then it is difficult to speculate on what might underlie it.

Personality Factors and Psi/PWA performance

It was predicted that participants who were classed as “intuitive” on the MBTI would show higher levels of both psi and PWA. This prediction was not supported, and the means of the sensing and intuitive types were similar.

It was also predicted that participants classed as high faith in intuition/low need for cognition on the REI would show more psi and PWA, again, based on the assumption that intuitive individuals are more adept at picking up on unconscious influences. This prediction was not supported, however, although high FI/low NFC individuals did show slightly higher scores on the psi task than the other three groups. It should be noted that the high FI/low NFC group suffered from an extremely small N (eight) and this must be taken into account when interpreting these results. It is fair to say that with an N this small, meaningful interpretation is extremely difficult. As a result, these results must be deemed inconclusive until further work investigates any possible relationship using a larger N.

Belief in psi or PWA did not influence performance on the respective tasks. This is, in some ways, surprising, as the believers/disbelievers effect is often cited as being relatively reliable in parapsychology (Lawrence, 1993). The lack of effect in both conditions may be a result of the belief questionnaires used. The psi belief questionnaire was obtained from the participant information form (PIF) administered to participants at the KPU. It may not be as discriminative between believers and disbelievers as other paranormal belief questionnaires might be. Indeed, it is possible for a participant who indicates a high level of belief in psi to still be classified as a disbeliever, based on a lack of reported psi experiences or lack of knowledge of others psi experiences. Based on this, it is possible that a more comprehensive measure of belief is required, which has benefited from extensive validation and reliability studies (e.g. Thalbourne and Delin, 1993). These questionnaires have other drawbacks, however, in that they often cover a wide range of non-psi related

phenomena (such as supernatural entities etc.). The PWA belief questionnaire used in the current study was created for this experiment, and only consisted of three items, all of which were based on items in the psi belief questionnaire used. Again, this may not have been an adequate measure of belief for the reasons given above.

It is, perhaps, not too surprising that many of the individual differences measures failed to discriminate between individuals who performed well in the psi/PWA tasks and those who did not. The psi task was non-intentional, and the traditional finding that believers score higher than disbelievers tends to apply primarily to studies employing an intentional psi task. Thus, it may be argued that the individual differences factors that have been found to co-vary with psi performance may actually be a measure of another mediating factor. For example, if a disbeliever takes part in an intentional psi task, then he/she is being asked to do something they believe is impossible. The conflict involved in this may be enough to obscure any psi effect that might otherwise occur. If, however, a disbeliever is asked to perform a task which, on the surface, has nothing to do with psi, then this factor has essentially been removed. Thus, it is possible to argue that the effects of individual differences factors on non-intentional psi tasks are substantially different to the factors that play a part in intentional psi performance. This is something which future research should address.

Personality variables and belief in psi/PWA

It is interesting that there was no association between belief in psi and belief in PWA. This may be a consequence of the shortcomings of each questionnaire. Belief in PWA did not bear any relation to belief in psi, which was unexpected, as it would make intuitive sense to predict that people who believed in psi would also believe in PWA. There may, however, be an argument for the opposite to be true, with participants who believe in PWA showing higher levels of scepticism about psi phenomena, based on the possibility that PWA can be used in explaining away many paranormal experiences. This argument is weakened, however, by the fact that the administration of the psi and PWA belief questionnaires was randomised, and many participants completed the PWA questionnaire before psi had been mentioned in the session.

There was no association between REI grouping and belief in either psi or PWA. Again, this may be considered surprising, given that Wolfradt, *et al* (1999) found a relationship between the REI and belief in psi, and, again, it may be a consequence of the believers-disbelievers measure employed. Once again, however, the small N in some of the cells must be considered, and these results must be treated with caution. Two of the REI groupings had an N of less than ten, which is probably not enough to find any relationship between grouping and belief. Given the “cognitive deficits” hypothesis and the general interest in the cognitive abilities of believers and disbelievers (see, e.g. Roe, 1999; Musch and Ehrenberg, 2002), then relating the REI to paranormal belief would appear to be an issue of some importance. The REI offers a measurement of cognitive styles that may be related to the way in which believers and disbelievers process information. Many previous studies into this have measured processing styles in terms of actual performance on particular tasks (e.g. Blackmore and Troscianko, 1985; Musch and Ehrenberg, 2002). This is a useful approach, but the REI offers an additional tool in assessing any potential differences in the cognitive abilities of psi believers and psi sceptics. For this reason, it would be worth exploring this issue further, perhaps using a more comprehensive believers-disbelievers measure.

Relationship among personality variables

The incorporation of the various personality variables into study 3 allowed for the investigation into how they might relate to each other. It was hypothesised that there would be an association between belief in psi and belief in PWA, as found by Roney-Dougal (1987). No association was found. This is somewhat surprising, although it was an exploratory hypothesis, and there is no *a priori* reason to expect belief in each to be correlated. Indeed, an argument could be made that those who do not believe in psi may endorse PWA more strongly given that it can explain certain psi-like occurrences.

Exploratory analyses were conducted on any possible relationship between belief in both psi and PWA and REI grouping. No significant associations were found,

although the aforementioned problems with small N's in the REI groupings must be taken into consideration.

Exploratory analyses were also conducted on the dimensions of the MBTI and belief in both psi and PWA. There were no significant associations found when belief in psi was considered, but belief in PWA appeared to be significantly associated with the Thinking/Feeling dimension. There appears to be a difference between the distribution of believers and disbelievers in the thinking and feeling categories, with more believers in the thinking category and more disbelievers in the feeling category. This result may be a consequence of multiple analyses. As such, this result must be treated with caution. If it is real, how are we to interpret it? According to the MBTI manual (Myers *et al*, 1998) thinking individuals are characterised by their reliance on logical connections whereas feeling individuals are more likely to be guided by emotions. As the effect did not appear when belief in psi was considered, then it must be something peculiar to PWA. It might be tempting to suggest that thinking individuals consider PWA to be a useful way of accounting for phenomena such as intuition. Instead of appealing to psi, rational thinkers may see PWA as a logical explanation for psi-like occurrences. The PWA questionnaire used in the current study, however, (see appendix 3) did not mention the possibility of PWA as a possible alternative explanation for psi phenomena. As all participants completed this questionnaire before the psi believers/disbelievers questionnaire, then it is unclear how easy this connection would have been to make. Another possibility is that thinking and feeling individuals had different conceptions about what "unconscious" influence might be. Again, however, due to the possibility that this may not be a real effect, we should perhaps await an independent replication before further speculation.

It was expected that there would be some kind of association between the REI and the sensing/intuitive dimension of the MBTI. This was hypothesised due to them both purporting to measure something akin to "intuition". Indeed, Pacini and Epstein (1999) state the importance in relating the REI to other personality measures, such as the MBTI. Unfortunately, however, there did not appear to be any relationship

between REI grouping and the sensing/intuitive dimension of the MBTI. However, as noted above, analyses on the REI are problematic due to the small N in some of the cells, particularly the two relating to High FI/Low NFC and Low FI/Low NFC. This may render the obtained result meaningless. It can be seen from table 7.7 that there are only eight individuals who fall into the high FI/low NFC category, and of these two were sensing on the MBTI and six were intuitive. It would be interesting to perform this comparison with a larger N, for a finding that there is an association between intuition as measured on both scale would suggest that they both are measuring something similar.

It was unfortunate that factors relating to sample size in certain groupings of the REI prevented adequate assessment of the related hypotheses. However, the fact that the majority of the sample fell into two categories may be interesting in itself. It may be a factor of the population that was used in this study. Most of the participants were students, with the majority of these being psychology students. It may be the case that this population has particular qualities relating to information processing that are not representative of a wider sample. Perhaps this was the reason for the imbalance towards the High FI/High NFC grouping. This in itself would be an interesting finding, as it would suggest students (or at least, psychology students) favour a complementary style of thinking. Epstein *et al* (1996) also used psychology students, but had a substantially higher N (973), although these authors do not report the breakdown of scores in each REI grouping.

Summary of study 3

To sum up, study 3 failed to replicate the psi effect and the subsequent response bias effect that the results of experiment 1a suggested. There was, however, some suggestion that males and females performed differently in this task. A replication of the PWA false recognition effect was successful. Although there was no direct correlation between performance on those two tasks, some interesting patterns emerged when males and females were considered separately. There was some indication that intuitive individuals on the SN scale of the MBTI showed a slightly higher degree of PWA than sensing individuals, although this was nonsignificant. No

relationship was found between belief in psi and belief in PWA. When belief in psi and PWA was associated with MBTI, the only association was between the thinking/feeling dimension and belief in PWA, although this may be due to multiple analyses. Hypotheses relating to the REI were difficult to assess due to a bias away from two of the REI groupings. Further, the number of analyses conducted raises the possibility of obtaining significant effects by chance. It is, therefore, important that any suggestive effects found in this study are replicated independently.

Chapter 8 – Summary, General Discussion and Conclusions

The central aim of this thesis was to make a new comparison between psi processing and perception without awareness. This was based on the model that psi information may be “perceived” outside of conscious awareness, and thus be processed in a similar manner to weak sensory stimuli. If this is the case, then it was expected that both psi and sensory stimuli perceived outside of awareness would have comparable effects on the cognitive system or be related to personality variables in similar ways. The similarity between psi and PWA is a comparison that has been made many times before by parapsychologists (see, e.g. Eisenbud, 1966; Broughton, 1987), and most would agree that psi processing is probably unconscious in nature. This may not be a fundamental property of psi, as there is some evidence that it can be consciously processed in certain conditions and by certain individuals. Conscious psi, however, may be the exception rather than the rule. If psi does exist, then it probably works best, as Broughton (1987) pointed out, when it is not noticed by the individual. Conceptualising psi in this way has led to a great deal of experimental work, attempting to verify its unconscious nature by means of comparing it with apparently similar, “normal” psychological phenomena. These studies had mixed results, but they all suffered from the drawback that, when a PWA stimulus was incorporated, they did not have an adequate definition of what would constitute “awareness”, and as a result of this, the methodologies employed were questionable. As a result of this, there are doubts over the validity of the previous comparisons due to the possibility that the PWA stimuli (or at least part of the stimuli) were actually perceived consciously. Thus, it is extremely difficult to assess the previous work in light of the criticisms of mainstream PWA research, and the subsequent work in PWA to address these criticisms. The current thesis was an attempt to make a new comparison between

the two phenomena. This new comparison took into account the recent developments in PWA, and used current methodologies which address the problem of defining and measuring awareness. In doing so, the current thesis was intended to bring the psi/PWA comparison up to date, and take a new look at it based on these new techniques. Five studies in total were conducted, comparing psi and PWA from varying angles. What follows is a summary of the findings.

Summary of Findings

Experiments 1a and 1b respectively compared the effects of psi and a PWA stimulus on word association. This followed parapsychological work by Stanford (1973), suggesting that psi could “prime” associations to various types of word. Using homophones as stimuli, study 1a found a non-significant psi effect, which, although inconclusive, was encouraging in that it suggested that interpretation of homophones might be a useful test for psi influence. Furthermore, there seemed to be evidence of an experimenter effect, with one experimenter obtaining significant positive psi results, while the other obtained chance results (although it must be noted that the difference between the senders/experimenters was not found to be significant). A further hypothesis emerged during the study, suggesting that “least favoured” responses may lead to more psi hits (after Stanford, 1967, 1973). When “least favoured responses” were isolated and analysed, the effect was found to fall just short of significance, while the sender effect also persisted. Due to the technical limitations, a possible artefact was identified. Known as the “stacking effect”, this artefact arises when participants share a target sequence, leading to the possibility that commonalities in response patterning may lead to spurious psi effects. In order to assess whether this was indeed a problem in experiment 1a, several new techniques to test for it were developed. These failed to find patterns in responding, suggesting that a stacking effect was not present in the data. Despite this, it was important to replicate this study, and one of the aims of study 3 was to do this. This

replication, conducted with randomisation of targets and a larger sample size, failed to replicate either the main psi effect or the response bias effect.

Experiment 1b was intended to be complementary to experiment 1a, investigating whether PWA stimuli could influence the way participants interpreted homophones in a similar way to the experiment 1a suggested psi could. A new technique for creating auditory PWA was developed, and the task used was the first of its kind to be used in a study of auditory PWA. Despite these innovations, no evidence of auditory PWA was found in this study.

While experiments 1a and 1b were based on a psi effect, the following two experiments were based on a PWA effect. These two experiments aimed to show that psi could influence the same cognitive processes that a visual stimulus perceived outside of awareness could.

Experiments 2a and 2b took, as a starting point, an established PWA effect (the false recognition effect; see Jacoby and Whitehouse, 1989). This false recognition effect occurs when a stimulus perceived outside of awareness influences recognition memory in such a way as to make the participant responds “old” to what is actually a new word on a test of recognition. This is due to the perceived familiarity (or perceptual fluency) of the unconsciously perceived word. The source of this sense of familiarity is not known to the participant (as the word was perceived unconsciously) and the participant attributes it to having seen the word previously. This is in direct contrast to when the word is perceived consciously, whereby the effect is reversed, with the participant attributing too much of the familiarity to the consciously perceived word. Experiment 2a successfully replicated this PWA effect, and experiment 2b was designed to investigate whether a psi influence could influence recognition memory in a similar way. The effect of psi on recognition memory was tested, and reaction times were also measured. Based

on a pilot study, gender effects were also considered. No main effects were found, although a non-significant gender interaction was evident, both in the false recognition data and in the reaction time data. This suggested that males and females were displaying opposing trends in the sending and non-sending conditions.

Overall, the above four studies failed to establish any common effects for psi stimuli and PWA stimuli. The final study was designed as a repeated measures procedure. Given that the previous experiments had compared psi and PWA using different samples, study 3 aimed to address important issues concerning the way in which both types of information may be processed within individuals by directly comparing performance on a psi task with performance on a PWA task. To do this, the most promising techniques from the previous studies were combined: the psi word association technique from experiment 1a and the PWA false recognition technique used in experiment 2a. Additionally, study 3 incorporated various personality measures that could conceivably underlie any correlations between the two unconscious phenomena. As mentioned above, study 3 failed to replicate the suggestive psi effects found in experiment 1a. It did, however, successfully replicate the PWA false recognition effect. It was also found that males tended to display slightly higher levels of PWA, although this was not found to be significant.

No overall correlation was found between psi and PWA, although interesting patterns emerged when males and females were considered separately, with males tending to show a negative correlation between psi and PWA, with females showing a positive correlation.

When the personality variables were considered, there were no major findings of note, although there was an indication of an association between the thinking/feeling dimension of the MBTI and belief in PWA. Although the findings were inconclusive,

the current study was the first to look at potential individual differences in PWA performance. As such, further work is needed in order to determine whether the influence of weak, subtle (unconscious) stimuli is pervasive throughout the whole population, or whether there are certain types of individual who are more or less susceptible to them. In general, however, assessing the results of study 3 was made difficult by the amount of analyses conducted.

The major aim of this thesis was to make a new comparison between psi and PWA. Based on the results obtained, the validity of the comparison must now be in doubt. There were, however, many questions arising from the work described, and what follows is a discussion of these issues.

General Discussion

The current thesis makes an important contribution to parapsychological literature in its re-evaluation of the psi/PWA comparison. The studies described represent the first attempt to bring the comparison in line with current methodologies in PWA research. It is important for parapsychologists to be aware of such issues, given the claims they continue to make concerning the unconscious nature of psi. Despite these claims, parapsychologists have shown little sensitivity to developments in mainstream psychology concerning the nature and measurement of unconscious mental processes. It is for this reason that it is necessary to bring such matters to the attention of the parapsychology community, and encourage parapsychologists to look at techniques used in cognitive psychology, and see if they can be applied to the study of psi.

While the previous comparisons were adequate for their time, the methods used do not stand up when placed under close scrutiny. The central issue in PWA research is the definition and measurement of awareness. As was explained in chapter 4, the previous work conducted by parapsychologists utilising PWA stimuli employs definitions and

measurements of awareness that can easily be criticised as being inadequate. As such, it is unclear whether the previous comparisons were valid, and whether their conclusions are justified. The work described in this thesis carefully considered the developments in the PWA field, and attempted to use these in making a new comparison. As previously noted (chapter 4), a “back-to-basics” approach was necessary, the primary aim of which was to assess whether such techniques could be used with success, and if so, whether the comparison between psi and PWA still appeared valid.

When assessing the work described in this thesis, it should be noted that there were many exploratory and innovative elements in many of the studies described. It is possible that this fact contributed to the inconclusive nature of much of the findings. Although the “back to basics” approach was an important step in making a new comparison between psi and PWA, there is the drawback of having to develop and test new methodologies. Whenever this is done, it is inevitable that the first steps will be faltering ones. Refinement of the techniques developed in this thesis may be required to shed light on the psi/PWA comparison in the future. Nonetheless, there have been several interesting outcomes. These will now be discussed, starting with some of the specific findings and observations before moving on to the more central issues.

Gender Differences and the sender-receiver pairing.

Interesting issues to emerge from the studies were those concerning gender differences and the sender-receiver pairing. There was some suggestion of a gender effect in experiment 2b (psi and false recognition), while in study 3, females were found to score higher than males on the psi task, while there was some indication that males scored higher than females on the PWA task. In experiment 1a, a potential experimenter effect was observed, and this raises questions concerning the nature of the sender-receiver pairing. We will now consider these issues, starting with the issues relating to gender.

Males were found to show slightly higher levels of PWA in study 3 than females. This is not a hypothesis which has been readily considered within the PWA field, but it is worth investigating whether there is a gender difference in susceptibility to weak stimuli.

Gender differences in parapsychology have been found in various studies. In experiment 2b, it was found that males and females responded differently to sent and non-sent items. The aim of experiment 2b was to investigate whether a psi influence could cause participants to falsely recognise new words when those words were “sent” by a sender as opposed to when they were not sent. When the results were analysed, it was found that females showed this predicted trend, and displayed more instances of false recognition, while males tended to score in the opposite direction, with the false recognition effect being greater in the non-sent condition than in the sent. When reaction time data was analysed, a similar interaction was found with females responding faster to sent items than non-sent items, and males responding faster to non-sent items than to sent items. It should be noted that both of these interaction effects were nonsignificant, and should be considered appropriately. However, this is an interesting pattern of results, and the fact that the differential scoring between males and females persisted for two dependent variables may suggest that there is something different between the way the two groups approached this task. Obviously, independent replication is needed before this effect can be considered real or not, but it is worth speculating on. There is some evidence in parapsychology of males and females differing on psi tasks.

Freeman conducted a series of psi experiments with children in the 1960s (Freeman, 1963, 1965, 1966, 1967, 1969). One of his findings (Freeman, 1966) was what he called “reversal of scoring” in which, under certain conditions, males score in the opposite direction from females. This difference consisted of males scoring above chance in one condition, but scoring below chance in the other, while females showed the opposite trend. Freeman reasoned that the different patterns of scoring for males and females may

be indicative of underlying psychological differences between the genders. He then proceeded to conduct ESP studies using tests of verbal and spatial abilities as a possible mediating variable for the observed gender differences. Differences between males and females have been debated in psychology for many years, but there do seem to be some differences, particularly in children's cognitive abilities (see Kimura, 1999; Halpern, 2000). Freeman's studies (1968, 1969) offered a degree of support for this hypothesis, suggesting that gender differences in psi performance may be a function of underlying cognitive factors.

Palmer (1978) reviewed the literature on sex differences in ESP scoring, and concluded that, apart from 2 notable exceptions, gender did not play a part in ESP studies "although it occasionally has been shown to interact with other predictor variables" (Palmer, 1978, p. 146.). However, Ramakrishna Rao, Kanthamani and Norwood (1983) identified a number of studies conducted since Palmer's review which appear to contradict this assessment.

Dalton (1994) and Dalton & Utts (1995) have investigated the effect of same or mixed sex sender/receiver pairs in various ganzfeld databases. Dalton (1994) reports that, when she combined her own results with those from two different ganzfeld studies (Schlitz, 1992; Morris, Taylor and McAlpine, 1993; both cited in Dalton, 1994) mixed sex pairings appeared to be more successful (although the individual studies did not show this). In 1995 Dalton and Utts conducted analyses on the PRL autoganzfeld database, which consists of over 10 years worth of ganzfeld data. Again, they were specifically interested in any differences in ESP success which may have been a result of gender differences in the sender-receiver pairing. Overall, they found that mixed sex pairs were more successful than same sex pairs, with male-male pairs producing the lowest scores.

Of course, gender differences such as these are extremely difficult to explain. Although there is some evidence that males and females may differ in terms of cognitive abilities, the social situation into which the participant and sender (and experimenter) are placed may also have an impact. Indeed, it has been found that mixed sex pairs are superior than same sex pairs at a variety of problem solving tasks (see, e.g. Hoffman, 1965). Rumerick, Capasso & Hendrick (1977) note that it may not be the gender differences *per se* that are important in psychological research, but rather the personal interactions between those involved in the study. These factors are particularly important in parapsychological studies, as parapsychologists are specifically encouraging a special kind of interaction between people, and there is no reason to suppose that this type of interaction should be any less affected by interpersonal factors than any other. How this all relates to the findings in the current thesis is unclear, however. As the psi tasks were non-intentional, and the subjects were not aware of a “sender” then this means that the gender effects must have had a basis either in the psychological factors specific to males and females, or in the interactions between the experimenter and participants during the period prior to the psi task.

Experiment 1a suggested that different experimenters/senders may obtain different “hit” rates in an ESP test. This is an effect that has been found before in parapsychology (e.g. Wiseman and Schlitz, 1997; 1999), and these authors offer a number of possible underlying factors for the observed effect. What Wiseman and Schlitz (1997) do not report, however, is any gender effects. Given that the experimenters were a male and a female, it would be extremely interesting to see how this may have interacted with the psi results.

In relation to experiment 1a, then, it is difficult to assess what might have caused the experimenter effect (assuming that the effect was real). It may be the case that interpersonal factors relating to prior knowledge of the experimenter served to influence

scores. All of the participants were known by the author (SW) and recruited by him. This may have led to the increased psi scores obtained when SW was sending. On the other hand, the task was a non-intentional psi task, and participants were not aware that SW would be attempting to psychically influence their responses. Additionally, in the sessions where SW was the sender, he had the most contact with the participants prior to the psi task. When EP (co-experimenter) was the sender, he had the majority of interactions with the participant. Given that the participants were all unknown to him, this may not have had as beneficial an effect on the psi scores as it would have in the sessions where SW was sending.

Additionally, the fact that many participants were recruited from SW's tutorial groups (particularly in experiment 1a) may have had a bearing on the observed effects. There has been some previous research suggesting that teacher-pupil pairings may be successful in psi tasks (see, e.g. Van Bussbach, 1953, 1955, 1956, 1959, 1961). This effect, however, has never been sufficiently replicated and there is evidence that it is only effective with younger children.

The role of the sender and the sender-receiver relationship is of great interest to parapsychologists. It would have been of interest to include a "no-sender" condition, in which the targets were displayed as normal, but no sender was present viewing them and attempting to influence the participant. There is previous research suggesting that the sender may not be necessary in order for psi hitting to occur (see, e.g. Honorton, 1995). Situations in which an individual can psychically obtain information from the environment without the involvement of a sender are instances of "clairvoyance". More recently, this has become known as "remote viewing" (see, e.g. Targ and Puthoff, 1978). It is, of course, of great conceptual importance to the field of parapsychology to determine whether any observed psi effects are due to the influence of a sender, or a

more general ability attributed to individual participants to obtain information from their environment.

Given the gender differences that were found in experiments 1a and 2b, it would be interesting to investigate whether these gender differences persist if there is no sender present, or if a female sender was used on some trials. This would go some way towards clarifying what the basis of this effect may be.

Target Issues

A further issue in the psi studies conducted for this thesis concerns the nature of the targets used. All the targets used were relatively neutral in emotionality. This was partly intentional, in order to eliminate a further potential source of noise, but it was also a characteristic of the stimuli used in the psi tasks. In the homophone/word association experiments, the targets were the interpretations of the homophones, and these tended to be emotionally neutral. Likewise, the targets in experiment 2b were images relating to the medium-frequency five-letter nouns that were selected for the test of false recognition. Again, this target pool tended to consist of neutral items. Targets in both psi experiments did, however, vary slightly in complexity. When a target was required, a visual image was found that related to its meaning. These were usually photographs, but occasionally were graphical illustrations. For some targets, a visual representation was difficult (e.g. "some"), and in these cases, the word itself was used as a target. Although these factors were not considered in the analysis, there is some reason to suggest that emotionality and complexity of targets has an impact on psi scores.

Delanoy (1988) reviews the experimental literature on the nature of psi targets and states that the most consistent finding in her review was that dynamic, multi-sensory targets were the most effective in eliciting ESP. Delanoy acknowledges that this conclusion is based on a limited number of studies, and states that detailed knowledge concerning

what makes a successful target is lacking. While Delanoy focused on the experimental literature on target properties, Watt (1988) takes a theoretical approach. According to Watt, the best ESP targets are meaningful, have emotional impact and contain human interest.

Delanoy's tentative conclusion concerning the apparent superiority of dynamic targets was supported by two meta-analyses. Honorton *et al* (1990) and Bem & Honorton (1994) report meta-analyses of ganzfeld studies, in which there is a suggestion that "dynamic" targets (e.g. video clips) lead to more success in ganzfeld ESP tests than "static" targets (e.g. still pictures).

Bierman (1995) has suggested that emotional targets may lead to more psi hits in a ganzfeld setting, while Parker, Grams and Pettersson (1998) have also reported a similar finding (although objectively classifying what is "emotional" is a contentious issue, as Delanoy, 1988, points out).

Watt (1996) conducted three studies aimed at investigating what properties successful psi targets may have. Watt did not find that emotional targets were more successful than neutral targets. In two of these experiments, there was a nonsignificant trend suggesting complex emotional may be more effective than simple emotional, although this trend was reversed in the third study.

May, Spottiswoode and James (1994) argue that the intrinsic properties of the target should be taken into account when assessing what makes a good psi target. They argue that emotionality is not target specific, as what is emotional can vary between

individuals. May *et al* (1994) identify “shannon entropy¹” as an intrinsic target property, and argue that the degree of entropy within the target positively correlates with target success. However, it would appear that May *et al* (1994) did not pay sufficient attention to the emotionality of their targets, despite claiming that they used affectively “neutral” targets.

The role of the target in parapsychological work is still, in many ways, uncertain. It would be surprising, however, if the target was not in some way intimately related to psi performance, given the number of spontaneous cases which seem to place emphasis on emotional or personally meaningful events. However, the experiments conducted for this thesis did not look at the target properties. Although this was necessary given the nature of the stimuli, and the exploratory nature of the experiments, it would be of great interest to manipulate the target properties in any possible future studies. This may give us an indication on exactly how important the target is in nonintentional psi tasks.

The psi/PWA comparison

As stated in chapter 4, the previous work on psi and PWA has obtained mixed results, but when similarities have been found, they have been fairly impressive (e.g. Roney-Dougal, 1987). However, the previous work suffers from various methodological shortcomings which make it difficult to assess whether or not the comparison is a valid one. The experiments in the current thesis used up-to-date techniques in order to reassess the comparison. This was based on the assumption that, if psi and PWA were indeed similar, then they should influence cognitive processes in similar ways, or be associated with similar personality factors. However, the current thesis did not find any compelling similarities between psi processing and PWA. How might we explain this, given that

¹Shannon entropy derives from information theory. The amount of shannon entropy in a target is related to how much “information” is contained within that target. Thus, a target with high shannon entropy is, generally, less complex than targets with lower shannon entropy.

characterising psi as an unconscious process has been so popular within parapsychology?

The psi controversy revisited

Given the failure to find reliable psi effects in the previously described experiments, we must consider how this may impact on the psi controversy. It is possible that psi is a weak effect, and that the means used in these experiments to detect it were insufficient to detect it. It is also possible that psi itself does not work *at all* in these conditions. While these are possibilities, another explanation for the failure to find a relationship between psi and PWA is that psi is not a real phenomenon. This conclusion may seem obvious, but it is important to state, given that the psi measures in the current study either were unsuccessful (as in experiment 2b) or could not be replicated (as in experiment 1a). Some of the arguments relating to the existence of psi were outlined in chapter 1 and need not be reiterated here. If psi does not exist, then we must look to alternative explanations for the various experiences reported throughout history. In this case, parapsychology becomes the study of anomalous experience, and a variety of alternative hypotheses will be posited relating to the factors that are involved in the “psychic” experiences that will undoubtedly continue to be a part of human existence. The debate concerning the existence of psi will inevitably continue for some time, and the outcome, whatever it may be, will have implications that reach far beyond the current thesis. At this stage it is difficult to see where (or when) the debate may end. What is clear is that parapsychologists must continue to refine their methods in such a way that an answer can, in principle, be obtained on the question of psi’s existence. Some ways in which they can do this are suggested below.

Lessons from PWA

Experiment 1b may serve as an important lesson into the existence and investigation of psi. Auditory PWA is notoriously difficult to demonstrate in a satisfactory manner. Previous attempts to elicit auditory PWA effects in the lab have fallen foul of methodological criticism. Even when the critics' comments have guided research, the phenomenon remains elusive. Does this mean that auditory PWA does not exist? Probably not. It is now generally accepted that, in the visual domain at least, information which is outside of awareness can, nonetheless, have an influence on ongoing cognitive processes. Positing similar effects in the auditory domain would appear to make intuitive sense, and there is nothing that would rule out this possibility *a priori*. However, while PWA researchers in the visual domain have been able to make methodological advances, culminating in convincing evidence for visual PWA, the field of auditory PWA has been left behind, unable, for technical reasons, to make the same advances. This does not rule out the existence of auditory PWA. It just means that, at the moment, it is extremely difficult to study it in a laboratory environment. Perhaps the same could be said of psi. Perhaps we have not yet identified the best methodology (or methodologies) whereby psi can be studied.

Leaving aside, for the moment, the techniques employed in the current thesis, let us consider the methods currently used by parapsychologists in studying psi. Much has been made of the ganzfeld technique, and, admittedly, it is arguably the most successful current protocol in parapsychology. Its roots lie in the idea that reduction of external stimuli will result in any weak extrasensory effects becoming prominent. This seems plausible, but we must ask whether it truly reflects the kinds of psychic experience that are generally reported. It can account for the various psi experiences reported in dream states, in states of meditation, or in altered states of consciousness (see, e.g. Honorton and Harper, 1973), but do these really represent psi as it might work in everyday life? It is likely that cases in which psi occurs in one of these states are exceptional, with the

majority of spontaneous psi experiences being far removed from these instances. It is possible, therefore, that parapsychology has paid a disproportionate amount of attention to these instances, developing techniques which, although apparently successful, may not reflect the way psi commonly works. Instead, other possibilities should be explored.

Parapsychologists are repeatedly stating that there is an unconscious element to psi processing. If they truly believe this, then a primary aim should be to develop new techniques which utilise this property. A lesson can be learned from the PWA research described in chapter 3. When faced with a plethora of methodological criticisms, PWA researchers were themselves forced to take a “back to basics” approach. Early PWA research had incorporated all kinds of esoteric dependent variables which did not necessarily reflect the unconscious properties of the phenomenon under study. Eventually, PWA researchers reasoned that, if they were to be successful, they must fully explore what properties unconscious processes possessed, and find out how these might be exploited when attempting to elicit unconscious effects. One property that proved useful was automaticity. Whereas conscious influences were available to the individual, who could use them to guide his actions, a fundamental property of unconscious influences meant that they result in automatic responses, outside of the individual’s conscious control. This led to a number of testable predictions, and, in time, PWA was demonstrated reliably, and (relatively) without controversy (see chapter 3). If parapsychologists consider some forms of psi processing to be unconscious, then they should follow a similar line and fully understand what this means. Only by exploring the implications of what it means for something to be unconscious, and then developing techniques which can exploit these unconscious properties, will we get a clearer picture of whether psi is unconscious, and, if it is, how it might work. The current thesis offers a step in the right direction for this approach. Although the psi measures used had mixed results, there are a number of other automatic processes that could conceivably be used

in the future as measures of psi influence. It is hoped that this is an issue which will be considered further in the future.

Is the comparison valid?

Another issue which we must consider is the possibility that the comparison between psi and PWA is not a valid one. It may be the case that psi is *not* an unconscious process. This would seem to go against much of what parapsychologists have had to say about it. If this is the case, then it raises various important questions about the nature of psi and the role it might play in the world. For example, many of the spontaneous “psi” experiences in the literature consist of situations in which the psi stimulus enters the system outside of awareness and influences some ongoing process. Stanford (1974) outlines these types of psi experience. Usually, the event is only interpreted as being a “psi” occurrence after the outcome has been assessed by the individual. Given the nature of these experiences, any psi influence would certainly appear to be outside of awareness, but if psi is not an unconscious process, then an alternative explanation for these experiences must be sought. Just as parapsychologists who claim psi processing is unconscious must fully realise the implications of this, if psi is *not* unconscious, then it is the duty of parapsychologists to understand what *this* means. A useful starting point might be to identify what psi is comparable to, if it is not comparable to PWA. It was outlined in the introductory chapter that psi processing is most likely to be closely related to other psychological functions. Finding the psychological processes that are most similar to psi processing will inevitably make it easier to understand the way in which psi may be processed. A further benefit will be the conceptual and methodological work already conducted on whatever process is related to psi. This should help guide parapsychological research in understanding the psychological factors involved in psi processing. This is an extremely important issue. Most modern parapsychology experiments are extremely well designed in terms of controls and safeguards etc. but one cannot help but wonder whether as much consideration has gone

into the nature of the psi phenomena under study as has gone into ruling out extraneous variables. Parapsychologists must think seriously about psi as a psychological process and then ask themselves whether or not their methodologies are conducive to that process.

Certainly, the outcomes of the current thesis place the validity of the comparison into doubt. None of the experiments conducted suggested that psi and PWA work in similar ways. This may be due to the new techniques developed for the current thesis. Experiment 1a revealed suggestive evidence for a psi effect on word association. However, the subsequent PWA experiment aimed at obtaining a similar effect used new methods which were previously untested. Given the innovations required to create stimuli, and the fact that the task itself was being used in this context for the first time, it is perhaps understandable that no effect was obtained. Further work is needed on both the technological side of auditory PWA, and also the nature of the task used to demonstrate an effect.

The results of experiments 2a and 2b did not suggest that psi could influence recognition memory in the same way PWA was found to. In this pair of experiments, it was the psi experiment (2b) that was innovative. This was the first parapsychological study to look at the potential effect of a psi stimulus on recognition memory, and only the second investigating the effect of psi on memory in general (see Stanford, 1970). As such, there was very little precedent for this type of psi task. As previously stated in the discussion for experiment 2b, this experiment suffered due to the lack of trials and the possibility of a ceiling effect, and it would be interesting to devise a similar study addressing these issues.

Study 3 failed to demonstrate that there was any relationship between performance on a PWA task and performance on a psi task. These tasks were, however, very different in

nature, and it would be desirable to conduct a similar repeated-measures study using similar psi and PWA tasks. Indeed, the ideal experiment would be one in which participants could not determine whether they were taking part in a psi trial or a PWA trial. This has been implemented before (e.g. Roney-Dougal, 1987) with some degree of success, although the PWA methodology was questionable. Further work comparing psi and PWA should consider the benefits of having indistinguishable psi and PWA trials, while also adhering to the current techniques in place in PWA research.

Assessing the validity of the psi/PWA comparison on the basis of the current thesis is difficult to do. The work described represents a new approach to the comparison, incorporating methods from PWA research and investigating whether they could be used within a parapsychological context (and, in the case of study 1b, vice versa). Although no direct similarities were observed, this does not necessarily invalidate the comparison. It remains possible that psi *is* an unconscious process, and the methods used in this series of experiments were not suitable as measures of it.

What should be of major interest to parapsychologists is the evolution of the PWA field. PWA research has, for long periods, been constricted by the criticisms it faced. This led researchers to adopt a more conservative approach, aiming to demonstrate that the basic effect was real and worth investigating. This is extremely similar to the field of parapsychology. Now that the PWA field appears to be emerging from what we might call its “proof-oriented” stage, it will be interesting to see how the field evolves, what methodologies are developed and what effects are observed. The current thesis contributes to this evolution by introducing individual differences measures as a means of determining underlying variables in PWA. It may be the case that future developments in the field of PWA have serious implications for parapsychology and the psi/PWA comparison. That is why it remains important for parapsychologists to be aware of the developments, not just in PWA research, but in psychology research in

general. It is in these fields that we may find the key factors that allow us to take parapsychology and the study of psi processes forward.

Conclusions

The research described in this thesis, and, more importantly, the methodological advances that have been made, represent a new way of looking at the relationship between psi processing and perception without awareness. This thesis represents the first attempt to bring the parapsychological work concerned with this comparison into line with current thinking in PWA research. This was needed as, despite many good studies being conducted in the past into psi and PWA, all of them (as far as the author could determine) used outdated methodologies. Parapsychology has much to learn from fields such as PWA, which itself had to display to sceptics that there was indeed an effect worth investigating. Moreover, the way in which PWA researchers approached this was by looking at what it meant for something to be “unconscious” and exploit these properties to reveal unconscious effects. This is something that, unfortunately, has not yet been done by parapsychologists who claim psi is an unconscious process. Again, this is something we can learn from the PWA field.

Although the experiments described did not find direct, convincing comparisons between psi and PWA, this should not be too discouraging. Many of the methods used were new and could possibly be refined in future investigations. There were, however, some interesting things that emerged from the experiments. Results from experiment 1 suggested that word association might be a useful psi vehicle, and raised some interesting questions concerning the sender/receiver relationship. Additionally, analyses of experiment 1 required some innovative techniques in the assessment of potential “stacking effects”, which itself should be of interest to the field as a whole. It is possible that parapsychologists have, in at least some instances, overestimated the influence of the stacking effect on psi results. It may even be possible to use the techniques

introduced in the current thesis to re-analyse cases where stacking effects have been posited.

Experiment 1b was innovative in the technological and methodological advances employed. The fact that no effect was found can be explained in a number of ways, all of which have potential implications for the auditory PWA field. In this sense, experiment 1b was extremely informative and worthwhile.

Experiment 2a replicated a PWA effect, which, given that the field is still emerging from a controversial period, is encouraging. Experiment 2b introduced the first study looking at the potential effect of psi on recognition memory. Although no main effect was found, this should still be a potential area of interest, given the possible ceiling effect and low power. One thing that was of interest in experiment 2b was the apparent (but nonsignificant) interaction between males and females, found both dependent variables. This is similar to what Freeman found in the 1960s, and is certainly worth following up.

Study 3 had many exploratory elements, and included personality variables as further measures of the potential relationship between psi and PWA. The psi effects found in experiment 1a did not replicate, although a gender effect was observed, with females scoring higher than males. The PWA false recognition effect was once again replicated. Interestingly, males and females showed different correlation patterns when their psi and PWA scores were correlated. The fact that the personality variables did not reveal much about how different people respond to each type of weak stimulus was interesting in itself, as it raises questions concerning whether or not personality influences performance on nonintentional psi tasks the way it has been seen to influence intentional psi. This, again, is something that is worth following up on.

Despite not finding the direct comparison between psi and PWA that was expected, the experiments conducted for this thesis have raised a number of important questions. Most importantly, it is hoped that parapsychologists stay aware of developments in other fields, particularly cognitive psychology. There are many issues left to explore, not least those related to the validity of conceptualising psi as unconscious process. Although there is much work to do, it is hoped that the work contained in this thesis represents a step in the right direction.

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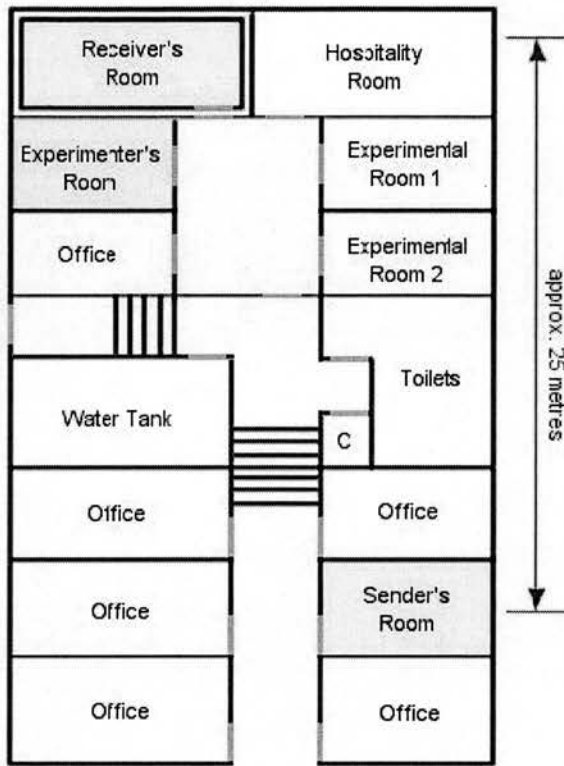
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Appendix 1 – Layout of Experimental Rooms at KPU



Appendix 4 – REI Inventory (short version)

Please rate the following statements on how true they are for you

1. I don't like to have to do a lot of thinking

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Completely false				Completely true

2. I prefer complex to simple problems

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Completely false				Completely true

3. My initial impressions of people are almost always right

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Completely false				Completely true

4. I try to avoid situations that require thinking in depth about something

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Completely false				Completely true

5. I believe in trusting my hunches

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Completely false				Completely true

6. I can usually feel when a person is right or wrong even if I can't explain how

Completely
false

Completely
true

7. Thinking hard and for a long time about something gives me little satisfaction

Completely
false

Completely
true

8. When it comes to trusting people, I can usually rely on my “gut feelings”

Completely
false

Completely
true

9. I prefer to do something that challenges my thinking abilities rather than something that requires little thought

Completely
false

Completely
true

10. I trust my initial feelings about people.

Completely
false

Completely
true

Appendix 5 – Materials

a. Homophones

base/bass	here/hear
bean/been	heal/heel
bear/bare	grown/groan
brake/break	male/mail
deer/dear	meet/meat
duck/duck	no/know
flour/flower	our/hour
plain/plane	sun/son
sell/cell	week/weak
serial/cereal	whole/hole
steel/steal	sum/some

b. 5 letter nouns (experiments 2a, 2b and 3)

paper salad album house
pedal yacht elbow party
bosom slide brothscarf
flush peach ditch easel
night flint nurse elbow
flour shawl dozen swamp
crown bunch union grain
coach match thief heart
dwarf graph depot chair
stove olive dress coast
novel adult sweep troop
ranch slide blade chief
maple bread gypsy spire
paste whale tooth cream
cheek guard spasm swarm
viola lunch broth noose

dough spine shelf radio
flame tiger stamp stall
lodge agony thigh scarf
bacon witch flood bogey
manor horse giant wedge
mouse chick otter beer
porch judge scalp swing
wagon berry altar bosom
panel abode mouth glare
canal human party relic
canoe demon pulse screw
sport cobra daddy clerk
ridge birth arrow baker
uncle flash shirt beard
brick vowel sting craft
alien meter fence stone
geese cloak slope paint
chalk shrub basin table
cargo queen devil brain
store bully track squad
slate beach skull towel
feast purse sweat photo
wrist frame badge floor
brush slice plain aisle
cloth flesh pilot razor
tower rebel buggy brute
black bride sword fairy
world booth alley mayor
ounce wheel atlas hound
shore vicar cliff rival
organ month metal token
stake crane spray widow

shell rabbi rifle drill
hotel sauce grave wound
board voter tutor tulip
crumb rider bench verse
angle tribe brass white
apron ocean dairy drink
movie medal globe curve
opera sugar liver chart
goose flare miner guest
pupil plumb ledge fever
lobby scout arena pitch
plank earth snake candy
trout glass slave saint
juice bible