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Nonintentional Behavioural Responses to Psi: Hidden Targets and Hidden Observers

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Abstract

Psi is the phenomenon of apparently responding to or receiving information by means other than the recognised senses. Psi information may influence human behaviour, without the individual intending this or even being aware of it. This thesis seeks to investigate nonintentional behavioural responses to psi. We present five empirical studies that investigated nonintentional behavioural responses to psi information. In each study, the psi information was hidden from participants, in that the participants neither had sensory access to it, nor did they know that it existed.

Two different combinations of psi information and a behavioural response were examined. The first was the influence of hidden psi information on psychological task performance. The second was the influence of covert, remote observation by hidden observers on the social facilitation effect. In all the studies, the effects of individual differences in participants' personalities were also considered.

In Experiment 1 we investigated whether hidden targets influenced participants' judgements of the lengths of lines. There was no overall psi effect, but we found a replication of a response bias effect and a significant correlation between psi and participants' extraversion. In Experiment 2 we investigated whether hidden targets influenced participants' speed on a maths task. There was no overall psi effect and no correlations between personality and psi scores.

We reviewed previous research literature on social facilitation from the novel angle of investigating whether being watched can, in and of itself, lead to the social facilitation effect. Experiments 3, 4, and 5 developed the paradigm of testing for a social facilitation effect from remote observation, investigating whether remote observation leads to the same behavioural changes as knowingly being observed by a physically present person. We compared participants' performance on psychological tasks under different observation conditions: alone, remotely observed by a hidden observer, and observed by a physically present observer. The expected social facilitation effect was not found in these experiments, leading to a series of improvements to the sampling, methodology, and tasks over the course of these experiments. As the social facilitation effect from a physically present observer was not reliably replicated, these experiments were not conclusive tests of whether there is a social facilitation effect from remote observation. However, there was an indication in Experiment 3 that remote observation does not exert a significant behavioural effect.

Considered together, our studies explored novel approaches to examining nonintentional behavioural responses to psi. The significant correlation between participants' extraversion and psi is, to our knowledge, the first time this effect has been found in a nonintentional psi experiment. This, and the replication of the response bias effect, represent important advances in parapsychology. Our experiments are also the first to test the assumption, inherent in many research designs, that covert observation does not affect participants' behaviour. Overall, our findings did not support the psi hypothesis.

Declaration

I have composed this thesis and the work is my own.

Mary-Jane Anderson

Dedication

to Arthur and in memory of Uncle Jack

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1. Introduction

1.1. Overview

This thesis seeks to investigate whether information that cannot be accessed by the recognised senses can affect human behaviour, even without one trying to respond to this *extrasensory* information. There is much more information in existence than could possibly be presented to our senses, and it might be the case that we have *psychic ability* that enables us to respond to information or influences beyond our sensory reach. This thesis takes the working hypothesis that people respond to extrasensory information all the time, without any attempt to make this happen, and usually without awareness that this is happening. Given that, this thesis seeks to investigate aspects of *nonintentional* behavioural responses to purportedly psychic information.

In the studies presented in this thesis, two kinds of extrasensory information or influence are tested. Both kinds of extrasensory information and influence are *hidden* from the participants, i.e., participants have no sensory access to them and do not even know that they exist. In these studies, we investigate the extent to which hidden information and hidden influences might affect participants' behaviour. The first combination of hidden information and a behavioural response explores whether hidden information affects participants' performance on psychological experiment tasks. In Experiments 1 and 2, the hidden information is a set of specifications for how participants might perform their tasks, relating to the answers they might give and the time they might take. In addition, in Experiments 1 and 2, we also investigate how psychic influences on behaviour might be influenced by participants' *personality*. Personality and psychic ability has been well researched in the context of experiments in which one is trying to use psychic ability (see Chapter 2), but has

not been examined as fully in experiments in nonintentional psychic ability. It is hoped that the effects of personality might reveal aspects of how psychic ability functions.

The second combination of a hidden influence and a behavioural response introduces a social element. The hidden influence relates to participants being watched unawares, under circumstances that preclude any sensory way of knowing that someone is watching, such as not seeing the observer or being told about him or her. People's ability psychically to detect being watched: *remote observation*, has previously been researched (see Chapter 6). This remote observation influence is paired with a well researched effect of observation on task performance: the *social facilitation effect* (also reviewed in Chapter 6), bringing together a psychic influence and a behavioural response in a novel combination. The social facilitation effect indicates that in psychological tasks, participants' behaviour is affected by knowingly being watched by a physically present observer. In Experiments 3, 4, and 5, we ask whether observation that one can only be aware of or influenced by *psychically* leads to the same behavioural changes as being observed by someone known to be physically present. If so, this might have important implications for experimental psychology research that relies on covert observation, and might shed light on other forms of unintentional communication and influence in experimental situations.

This chapter introduces the basis for the experiments presented in this thesis. Firstly, we outline what *parapsychology* is, explain why it is worthy of further study, and define the terms used in parapsychology. We then explain the methods of experimental parapsychology research, along with a brief synopsis of the evidence that has been found so far, and the theories that support the experiments presented in this thesis. This chapter concludes with an outline of the subsequent chapters in the thesis.

1.2. What is Parapsychology, and Why Should it be Studied?

Human experience includes phenomena that do not seem to be explained by sensory knowledge, delusions, or logical induction. These include, but are not limited to, apparently knowing that something will happen before it does, thinking or feeling another person's thoughts or feelings, or being able to influence events in the outside world through the power of the mind. These events appear to involve a direct interaction between the mind and the external environment that is not mediated by normal sensory or motor means. These events and experiences have been called *paranormal*, literally meaning 'beyond normal', or *psychic*, literally meaning 'of the mind' (G. A. Miller, 2006). Both these terms refer to phenomena that are theoretically impossible, and beyond the currently accepted range of human or animal capabilities (Thalbourne, 2003). This is a *negative definition*: It sets the boundaries of what paranormal and psychic are not, rather than what they are. Ostensibly paranormal events are commonly experienced: Surveys have found that up to three-quarters of the general population believe that they have had such experiences (Broughton, 1991). These events therefore warrant investigation as they are part of human experience and are not yet fully understood. It is also important to conduct research to discern whether such experiences are genuinely paranormal or psychic, or whether a normal explanation is possible. Research is also important because ostensibly psychic experiences are controversial, and evoke extreme emotional reactions of both belief and scepticism (Broughton, 1991; Freedman, 2005). Impartial and open minded research is therefore vital. This research is parapsychology: the scientific study of "apparent anomalies of behaviour and experience that exist apart from currently known explanatory mechanisms that account for organism-environment and organism-organism information and influence flow" (Parapsychological Association 1989, pp. 394 – 395). Although this is also a negative definition, it is more delimiting than that given for 'paranormal' above, as it specifies that parapsychology does not aim to investigate every phenomenon that is outside of current scientific understanding, but concerns itself with specific areas

relating to anomalous information transfer and influence. These areas are collectively called *psi phenomena*.

1.3. Psi Phenomena and Terminology

The term *psi* is used to denote the unknown factor in anomalous experiences. It has been defined as “a correspondence between the cognitive or physiological activity of an organism in its external environment that is anomalous to generally accepted *basic limiting principles* of nature” (Palmer, 1986, p. 139). This is again a negative definition, setting the boundaries of what *psi* is not. This definition can be criticised because it does not include any reference to behavioural effects of *psi*, and behavioural effects are relevant (as discussed below). *Psi* is also negatively operationalized: It is only when all normal methods of information transfer or influence are excluded that we can attribute anomalous events to *psi*. *Psi* was originally proposed as an atheoretical neutral term that did not presuppose a mechanism for psychic events (Thouless, 1942). It is important that the term *psi* does not imply how psychic events might happen, because there is no agreed-upon theory (Irwin, 1999). Although it is common to speak of *psi* as though it is a mechanism or a process, it is merely a construct for that which is not, or not yet, understood in anomalous events (Broughton, 1991). Furthermore, the existence of *psi* has not been conclusively established; the existence of *psi* is taken as a working hypothesis, referred to as the *psi hypothesis*. We use the term *psi* in this thesis with the caveat that the existence of *psi* is, as yet, uncertain.

Psi phenomena can be categorised by whether they refer to ostensibly anomalous information transfer: *extrasensory perception* (ESP), or ostensibly anomalous influence: *psychokinesis* (PK). ESP is the main focus of this thesis.

ESP is defined as: the apparent ability of a human being to acquire information without using the ordinary senses of the body and without depending on logical inference. The extra simply means “outside” of the

sensory channels (at least as we understand them today); perception can refer to anything from vividly “seeing” or dreaming an event to having a vague hunch or even obtaining information that never reaches one’s consciousness but in some way affects his or her behaviour. (Broughton, 1991, p. 33).

This is a comprehensive definition that covers the different levels of awareness of psi information that the recipient may have; this is a key concept for this thesis which we will return to below. This definition can be criticised, however, for omitting that ESP can have an effect on the physiology of the recipient organism (e.g., Radin, 2004). ESP can be further subdivided by the perceived source of the psi information. If this is apparently from another organism, such as ostensibly knowing another person’s thoughts, this is *telepathy*. If the source of the psi information is apparently an object or event, such as seemingly locating a lost object through psi, this is *clairvoyance*. In experimental situations it is extremely difficult to differentiate between telepathy and clairvoyance, and the term *general extrasensory perception* (GESP) is used to indicate that both telepathy and clairvoyance might be involved. ESP can also appear to occur backwards in time. This is *precognition*, defined as awareness of a future event that cannot be deduced from present knowledge (Thalbourne, 2003). PK, while not the main focus of the thesis, is an important concept in understanding research presented later herein. Psychokinesis literally means ‘mind movement’, and is defined as “the apparent ability of a human being to affect objects, events, or even people around him or her without using the usual intervention by the muscular system. ...often described today as the direct influence of consciousness on physical systems.” (Broughton, 1991, p. 35). The difference between ESP and PK is in the perceived direction of information flow or influence. In ESP, information apparently comes into someone’s mind; in PK an influence is apparently sent out. In practice it is difficult to distinguish between ESP and PK, just as it is difficult to differentiate between telepathy and clairvoyance. Consider, for example, the case of a man who becomes aware that he is being looked at by an unseen person, without having received any normal sensory information relating to being watched: *remote observation detection*. Is this telepathy, anomalous awareness of what the person

looking at him is thinking? Or is it clairvoyance, anomalous awareness of the event of being looked at? Alternatively, it could be PK, if the observer anomalously influenced him to become aware of being seen. These distinctions therefore break down under certain situations, but are still the main classifications of psi phenomena (Irwin, 1999).

1.4. Methods of Investigating Psi

This section describes the main types of research used to investigate psi. In the early days of parapsychology, researchers collected case studies and anecdotal accounts of spontaneous ostensible psi occurrences (Broughton, 1991). Spontaneous cases are important in illustrating the role of psi and its processes in real life, unrestricted by experimental conditions (L. E. Rhine, 1978), but are at risk of artefacts that prevent them counting as proof for psi. These include selective memory (i.e., forgetting or exaggerating aspects of the memory); self-deceptions (whether intentional or a product of interpretation); and other unknown confounding factors that cannot be controlled for (Morris, 1978). Researchers who used case studies relied on the honesty of the contributors, and selected only cases that had no apparent normal explanation (L. E. Rhine, 1978). This cannot eliminate instances of intentionally fraudulent reporting. Spontaneous case collections are also at risk of reporting biases. For example, people must first interpret an experience as being a psi event in order to report it. Secondly, as people are likely to report only instances when, for example, they experienced a precognitive impression that turned out to be true, spontaneous case collections will lack the many instances when such impressions were subsequently found to be false. Thus, we cannot rule out chance as an explanation for these spontaneous events. Lastly, many people would not have any opportunity to report their experiences to a parapsychologist, and so the sample of respondents might not be representative of the population as a whole (Morris, 1978). As spontaneous events occur in uncontrolled conditions it is impossible to rule out normal explanations for these events. Spontaneous cases therefore do not posit proof

for psi, but they are useful for observing patterns across large numbers of reported psi experiences, which can then inform experimental hypotheses.

Most present day parapsychological research is experimental (Broughton, 1991; Wolman, 1977). Experimental studies are able to tackle two main problems in testing for psi: firstly whether normal explanations for the phenomenon have been ruled out, and, secondly, whether or not an apparent psi effect is a chance occurrence. It is important to rule out all possible normal sensory explanations, because, as noted above, psi is negatively operationalised. It is only when all sensory means of information transfer or influence are prevented that an effect can be attributed to psi. If there is any chance that an experimental outcome is caused by something other than psi, then the psi hypothesis can neither be supported nor rejected. One problem inherent in eliminating *sensory leakage*, the accidental transfer of target information to the participant by non-psi means, is that people's behaviour can be affected by sensory stimuli that they are not consciously aware of, such as extremely quiet sounds (Nash, 1986; Roney-Dougal, 1986; Wilson, 2002a, 2002b). Such *subliminal* influences can affect people's behaviour in a similar way to psi (Roney-Dougal, 1987). Therefore, leakage of any sensory information, including stimuli too weak to be consciously perceived, must be rigorously prevented to be able to claim that there was a psi effect. Sensory leakage can be obviated by physical and sensory separation of the recipient and the source of psi, (for a more in-depth consideration of these issues see Dalton, Morris, Delanoy, Radin, & Wiseman, 1996). Unintentional cueing of the participant can be prevented by keeping the experimenter blind to the psi manipulation during his or her contact time with the participant.

Regarding ruling out chance as an explanation of experimental outcomes, it is possible to construct experimental situations in which the probability of an event occurring can be estimated. This is usually achieved in GESP tests by having both a *psi target* (the stimulus the participant tries to identify) and a *pool* of other potential targets. One example of such targets are the Zener cards: five geometric shapes

(circle, cross, wavy lines, square, and star) (J. B. Rhine, 1934; J. B. Rhine & Pratt, 1962). Participants try to select the target. A *hit* is achieved when participants select the actual target, and a *miss* occurs when they select one of the other items in the target pool. The overall hit rate is statistically compared to *mean chance expectation* (MCE): the expected hit rate if there were no psi effect and if chance were the only influence. One important consideration in establishing MCE is that targets are adequately randomised (see Chapter 2).

Within experimental parapsychology there are two main approaches to gathering evidence for psi. Much of the early research was *proof-oriented*, and aimed to find evidence for the existence of psi (Thalbourne, 2003). *Process-oriented* research, which has predominantly superseded proof-oriented research (Broughton, 1991), aims to determine how psi is related to other psychological variables (Thalbourne, 2003). Through process-oriented research the field of parapsychology hopes to gain more understanding about psi, and validate the psi hypothesis by linking psi to other known psychological phenomena (Rao, Kanthamani, & Palmer, 1990; Schmeidler, 1988), by grounding the unknown (psi) in the known (such as personality variables). Process-oriented research asks whether, if psi exists, certain testable consequences can be found. The experiments in this thesis, in common with other process-oriented research, are carried out despite the caveat that the existence of psi is indeterminate, but aim to test the psi hypothesis further.

1.5. Evidence for Psi

Although the existence of psi is not certain, in this section we review experimental evidence for psi, using as an example the Ganzfeld studies: a high-profile and controversial paradigm with considerable research evidence. Participants in a Ganzfeld study are exposed to homogenous, un-patterned sensory stimulation while in a state of relaxation that aims to create a psi-conducive altered state of consciousness (Honorton, 1977). In contrast to the Zener card example cited above,

in which participants know the potential target identities (the geometric shapes) and choose between them: *forced-choice* design, in a Ganzfeld experiment participants do not know the potential target identities. Instead, participants free associate out loud: *free-response* design. The correspondences between what the participant says and each of the potential targets in the pool are rated by someone blind to the identity of the target. If the participant's description most closely matches the target, a hit is scored. The Ganzfeld studies appear to demonstrate a strong psi effect, for example a hit rate of 47% was found in a creative sample, where 25% would be expected by chance (Dalton, 1997). Bem and Honorton (1994) contended that the Ganzfeld database provided substantial enough evidence for parapsychology to be recognised in general psychology.

To assess whether an effect is genuine, meta-analyses are used to combine the results of multiple studies and estimate the overall effect size. However, there is a certain element of subjectivity in meta-analytic procedures, such as study selection and weighting. Bem and Honorton's (1994) meta-analysis of Ganzfeld studies found a mean hit rate of 35%, significantly above MCE of 25%, and a medium effect size. These Ganzfeld studies were strongly criticised by a sceptic (Hyman, 1985). Following a critical defence of the Ganzfeld findings, which found support for the psi hypothesis in the better controlled Ganzfeld studies (Honorton, 1985), guidelines for improvements to the Ganzfeld methodology were agreed between one of the lead Ganzfeld researchers and the sceptic (Hyman & Honorton, 1986). Data from Ganzfeld studies conducted in accordance with those guidelines found a hit rate significantly above MCE (Bem & Honorton, 1994; Honorton, Berger et al., 1990). Bem and Honorton concluded that this was strong support for the psi hypothesis and the efficacy of the Ganzfeld methodology. This was challenged by a subsequent meta-analysis of 30 more recent studies; this found a null effect size (Milton & Wiseman, 1999). Milton and Wiseman discussed whether the previous meta-analysis findings were spurious, or whether some of the previous studies had been conducted under more psi-conducive circumstances, but concluded that the Ganzfeld database

was not strong evidence for psi. Milton and Wiseman's (1999b) meta-analysis was strongly criticised for unwarranted exclusion of significant studies (Storm & Ertel, 2001). Storm and Ertel conducted another meta-analysis based on 79 Ganzfeld studies; this supported the psi hypothesis. Storm and Ertel's meta-analysis was itself criticised for including studies with poor methodological quality (Milton & Wiseman, 2001). A subsequent meta-analysis that incorporated the 30 studies in Milton and Wiseman (1999b) and added 10 more recent well-controlled studies to the analysis found a small, but significant, effect size and an overall hit rate of 30.1% (Bem, Palmer, & Broughton, 2001). Thus, despite controversy and fierce debate, there is considerable support for the psi hypothesis.

1.6. Theories of Psi

Given that there is support for the psi hypothesis, we now consider theories of how psi might function. These theories concern the processes of receiving, responding to, and becoming aware of psi information, and what the role of psi might be. We consider theories that propose that people respond to psi information all the time, nonintentionally, and frequently without awareness. Lastly, we advocate the importance of behavioural responses to psi, in theories and experimentation.

Inherent in the following theories is the idea that psi is an *unconscious* process (Broughton, 1988, 1991, 2006a, 2006b; Eisenbud, 1966-67; Stanford, 1974; Tyrell, 1947). Although the term unconscious has been used to refer to different aspects of psi function, and will be further disambiguated below, it can be clarified by first defining *conscious*. Conscious is defined as a subjective state of awareness, including awareness of dreams, usually operationalised by people being able to report their mental contents (Vandenbos, 2007). In contrast, unconscious is defined as mental processes and structures outside of subjective awareness that can nonetheless influence conscious experience, thought, and behaviour (Kihlstrom, 1987). This is distinct from *preconscious* information, which is not in current awareness but can be

recalled at will (Agnes, 2008), or the psychoanalytic concept of the unconscious as containing repressed ideas (Freud, 1977).

Tyrell (1949) proposed a two-stage model for ESP. The first stage is the process of psi information acquisition, which Tyrell argued occurs in the “subliminal self” at an unconscious level. This unconscious level is conjectured to be beyond time and causality, in which minds are interconnected so that information can be accessed from one mind by another (Price, 1949; Radin & Rebman, 1996; Tyrell, 1947). This leads to the problem that there is an infinite amount of information accessible through psi, requiring a filtering mechanism to restrict what is acquired (Broad, 1953). The second stage in Tyrell’s model is psi information coming to consciousness via “mediating vehicles”. Mediating vehicles include hallucinations, dreams, and impulses and occur if there is a “motive”, such as a need for the recipient to receive the psi information. The mediating vehicles are not themselves paranormal, and also mediate other forms of unconscious knowledge. Tyrell emphasised that people can only be aware of the mediating vehicle, not the process of psi information transfer. This, therefore, is one way in which psi is an unconscious process. The same, however, can be said of memory and sensory perceptions. For instance, we are not aware of the process by which our visual cortex processes light (Crick & Koch, 1995), but we are aware of seeing. It is not known how the first stage of Tyrell’s model might occur, but several suggestions have been made for the second stage. For instance, the first stage of psi might trigger memories the recipient already has, and the information in the memories then forms the conscious psi impressions (Irwin, 1999; Roll, 1966). Irwin advocated the memory model as it does not require perceptual information arriving without a sensory signal, which he thought was counter-intuitive, especially in precognition. Alternatively, Rao (1961) suggested that the second stage involved imagination. L.E. Rhine (1978) also considered Tyrells’ two-stage model of psi valid, and used it in interpreting spontaneous case reports. For example, she interpreted instances of apparent incomplete information transfer, such as a recipient knowing that a disaster had

befallen someone, but not knowing what exactly had happened, as a failure for all of the psi information to enter consciousness. Implicit in L.E. Rhine's reasoning, and in Tyrrell's model, is the idea that the end product of psi is conscious awareness. If, however, not all psi information reaches consciousness, there might be much more psi information available at unconscious levels, and the conscious portion might be the "tip of the iceberg".

There is support for the unconscious first stage of Tyrrell's two-stage theory of psi from similarities between psi and other unconscious processes. For example, *perception without awareness*, which occurs when subliminally presented sensory information enters the cognitive system and influences the recipient without conscious awareness (Dixon, 1981; Nash, 1986; Roney-Dougal, 1986; Schmeidler, 1986; Stanford, 1990). There is support for the second stage of Tyrrell's theory involving a transfer of information from the unconscious to the conscious mind from research comparing defensiveness to subliminal information and psi ability. For instance, people with higher thresholds against consciously perceiving threatening stimuli also demonstrate less psi ability (Haraldsson & Houtkooper, 1992, 1995; Watt, 1993; Watt & Morris, 1995).

Eisenbud (1966-67) considered the role of psi, specifically asking why people do not use psychic ability to gain great wealth and power. Eisenbud concluded that psi function is unconscious, in that it is not under voluntary control. Tyrrell (1949) also alluded to this when he stated that conscious intention is not required for telepathy to occur. Eisenbud also asserted that psi serves people's unconscious *needs* and *goals*, which may be self-defeating. In addition, he proposed that psi also fulfils evolutionary goals, which might be contrary to individuals' consciously held goals. For example, if psi served population control needs it might necessitate that an individual die; this would be contrary to the individual's desire to survive. He postulated that psi is a widely distributed function in all organisms, and might unconsciously play a part in all actions and decisions.

Considering the ideas of Tyrrell and Eisenbud combined, psi can be considered to be unconscious in several ways. For instance, people are unaware of the process by which psi occurs, they are sometimes unaware of psi information, and psi occurs without conscious effort. However, it should be noted that psi can also be consciously intended (Rao, 1961). To disambiguate the use of the word unconscious, we will use the term *nonintentional*, coined by Lewis and Schmeidler (1971), to denote psi occurring without conscious intent.

One of the implications of psi occurring nonintentionally is that the methods of testing for psi described above, such as the forced-choice guessing of Zener cards and the free-response guessing of Ganzfeld psi targets, might be very different from how psi functions in real life. Firstly, participants are intending to use psi. Secondly, participants are attempting to gain conscious awareness of psi information. While psi information can apparently enter conscious awareness, it arguably does not always do so, and therefore conscious guess measures might miss some psi effects.

The psi-mediated instrumental response (PMIR) model was developed in reaction to the previous ESP testing paradigm that relied on conscious guess measures, and to the underlying idea that the end state of psi information was to be consciously known (Stanford, 1990). It was intended to bridge the gap between laboratory ESP testing and real life psi experiences, and to develop ways of researching psi that would make the findings more generalisable to real-life situations. Stanford noted that, by definition, spontaneous psi events occur without conscious intent; therefore, he focussed on nonintentional psi paradigms. The PMIR model was revised several times (Stanford, 1974, 1977, 1982, 1990). This review will refer by default to the most recent formulation from 1990. The PMIR model involves many stages and propositions; this review will focus on those which are important to the experiments in this thesis. Stanford postulated that psi is goal oriented, and responds to the needs or dispositions of the recipient. The fundamental assumption of the PMIR model is that:

Through psi the organism (i.e., the living behaving individual) is able to respond to circumstances in its environment with which it has no sensory contact if those circumstances are of a kind to which it would respond if it had sensory knowledge of them. (Stanford, 1990, p. 62)

Although Stanford did not categorically state that the psi-mediated response would be the same response that people would make given sensory knowledge of identical circumstances, it is a reasonable assumption that in many instances the responses would be the same. We can illustrate this with an example cited by Stanford (1974). A gentleman wanted to visit friends across town, and made his journey by train. If he had known that his friends were not at home, but in fact near an interim train station, he would have alighted there. In actual fact he did not know that his friends were not at home, but alighted at the interim station absentmindedly, and met them, perhaps not as planned, but as intended. Stanford interpreted this as an example of a psi-mediated instrumental response; it is presented here as an example of the psi-mediated response paralleling the response that one would knowingly make. In some circumstances, unconscious knowledge leads to a different response to conscious awareness of the same information (see, e.g., Debner & Jacoby, 1994), but such situations are rare and difficult to contrive (Merikle & Daneman, 1998). Thus, the PMIR model proposed that psi enables people to respond to events and circumstances that are beyond sensory reach and not consciously known, as if they had conscious awareness of them. In addition to the psi-mediated behavioural responses, Stanford also proposed that PMIR leads to changes in people's arousal, attentional focus, and emotions; for example, feeling inexplicably nervous prior to an unexpected visit from one's manager. Psi information leads to these responses by triggering behaviours, feelings, images, associations, desires, or memories that are already part of the recipient's repertoire. The fact that these responses are already habitual to the recipient is probably one of the reasons why this process can go unnoticed, and also a means by which psi acts in the most economical way. It is not, for example, necessary to have a detailed, conscious, psi-mediated impression of an avoidable disaster, if one can avoid it through a small change in behaviour. Psi-

mediated responses can also occur through unconscious changes in timing, memory, mistakes, associations between thoughts, and conscious cognitions. Stanford asserted that PMIR can, but does not necessarily, happen without any conscious effort to use psi, conscious effort to fulfil one's needs, awareness of the situation that one is responding to, or awareness that anything extraordinary is happening. Although PMIR is a model for nonintentional psi, it also underpins conscious awareness of psi.

There are similarities between the models of Tyrell (1949), Eisenbud (1966-67), and Stanford (1974, 1977, 1982, 1990). The concepts of needs and motives are similar and obviate the need for a pre-conscious filtering mechanism to prevent psi information from all minds, places, and times from entering conscious awareness, because only the relevant information is drawn in. These models propose that the psi process is unconscious, and that psi information is primarily unconscious. The difference between these models is the end product of the psi process. Tyrell (1949) focussed on psi information coming to consciousness. Stanford (1990) emphasised the importance of behavioural responses, and suggested that conscious awareness of psi information might occur only in the event of adaptive behavioural responses failing to happen. Both Eisenbud and Stanford argued that psi events are likely to be much more common than is apparent, but that they occur without awareness, and so go unnoticed.

One criticism of these models described above is the concept of psi responding to needs. The idea that psi responds to needs was proposed by Stanford (1974), based on his appraisal of a large collection of spontaneous case reports. However, spontaneous case reports might be biased towards need-relevant psi events, because people might not notice psi events that bore no relation to their own needs. L.E. Rhine (1964) noted in her collection of spontaneous case reports that, while many reports concerned important circumstances, many conversely concerned trivial events that the recipients apparently did not need to know about. As noted above, Eisenbud (1966-67) argued that psi responded to people's unconscious needs and

evolutionary needs, which might be contrary to their consciously experienced needs and desires. Stanford (1974, 1977, 1982, 1990) also incorporated this concept, by suggesting that certain factors lead to people nonintentionally using psi against their best interests. These factors included neuroticism, negative self-concept, low self-esteem, guilt, and other conflicts. Therefore, if needs can be fulfilled by psi appearing both to serve and not to serve people's needs, need-relevance becomes an irrefutable proposition, and consequently not a useful hypothesis. As a further illustration of how two different outcomes can both be interpreted as supporting the need-serving nature of psi, Eisenbud gave the example of the conflict between a mouse's need to survive, and a cat's need to eat the mouse. Stanford did, however, propose some testable hypotheses relating to whether psi is need-serving. For example, that people with stronger needs are more likely to use psi nonintentionally to fulfil them. The issue of whether psi serves needs, and whose needs it serves, raises the question of what psi is for.

Broughton (1988) argued that psi, as a human ability, developed as a result of natural selection, and therefore must convey an evolutionary survival advantage. In a synthesis and extension of the models proposed above, Broughton suggested that the function of psi might be to bias thoughts, actions, and feelings unconsciously to lead to adaptive behaviour, such as avoiding danger. In this, psi might manifest as intuition or luck. He argued that psi might function better in this regard if it were unconscious and nonintentional, as overt, intentional use of psi might be counter-productive. For example, it is not long since witchcraft was punishable by death (Broughton, 1991). Perhaps to avoid individual responsibility for psi, many cultures attribute ostensible psi events to external agencies such as spirits (Broughton, 1988).

Broughton (2006) concurred with Tyrell's (1947) two-stage theory of psi, but proposed a new mechanism for the second stage: the *emotional system*. The emotional system functions unconsciously, and is geared towards survival. It quickly and automatically biases thought, behaviour, and feelings according to emotional

memories. Emotional memories are feelings and physical states (e.g., sweaty palms and elevated heart rate for fear) and these emotional memories play a major role in decision making (Bechara, Damasio, Tranel, & Damasio, 1997; Bechara, Tranel, Damasio, & Damasio, 1996; Damasio, 1995). The emotional system may unconsciously reduce the range of options for any decision by biasing the choice of memories and images that come to consciousness; this process would feel like intuition. Broughton (2006) argued that Stanford's assertion that psi: fulfils organisms' needs; occurs unconsciously; and functions through triggering behaviours, memories, feelings, images, and other mechanisms already in the organism's repertoire is fulfilled by the emotional system. The emotional system also operates independently of conscious intention, thereby fulfilling the nonintentional aspect of the PMIR model. There is support for the emotional system responding to psi influences in *presentiment* research, which investigates whether emotional response (measured by palmar sweating) increases prior to people being exposed to shocking stimuli (Bierman, 1997; Bierman & Radin, 1997; Radin, 2004a). Overall, Broughton concluded that the chief function of psi is to provide information, whether conscious or unconscious, to guide behaviour.

In summary of the theories of psi presented above, psi is widely thought to occur at an unconscious level, and to a much greater extent than is usually recognised. As Eisenbud (1966-67) suggested, this indicates that psi ability is widespread in the population and so *unselected* participants from the general public, rather than just selected psychics, can justifiably be used in experiments (Radin, 1997; Targ & Puthoff, 1974). While people can become consciously aware of psi information, it is likely that this is the tip of the iceberg, and more psi information might be available at an unconscious level.

The implications of this for parapsychology experiments are that measures of unconscious responses to psi might be more successful in capturing a psi effect, if there is one. In addition to the presentiment example noted above, there has been a

recent rise in psychophysiological measurements of psi, enabled by recent technological advances. A thorough review of psychophysiological correlates of psi-mediated remote observation detection is discussed in Chapter 6. Additionally, electroencephalograms have recently been used to investigate brain activity during psi experiments (Baker & Stevens, 2008; Kittenis, Caryl, & Stevens, 2004). As well as being unconsciously processed, psi is thought frequently to occur nonintentionally. Therefore, rather than using the traditional intentional psi-testing paradigms (e.g., participants trying to guess psi targets), psi might be better tested by creating situations in which participants' behaviour can respond to psi without the participants trying to make this happen. These nonintentional psi experiments will be further described in Chapter 2, but in brief, they consist of experimental situations in which participants are not informed that they are in a psi test. However, the outcome of the experiment depends upon participants' nonintentional behavioural responses to target information or influences. As participants do not know that they are in a psi experiment, they do not know that there are psi targets; thus are called *hidden targets*, as their existence is hidden from the participants. Furthermore, if the purpose of psi is to guide behaviour, it follows that it is important to research whether or not psi affects behaviour.

Before considering the ways that nonintentional behavioural responses to psi have been researched, we will outline three ways psi can affect behaviour, depending on the level of awareness people have of the psi information. Spontaneous case reports will be presented as illustrations, but do not posit proof that these events were psi-mediated. The first way is that the recipient could become aware of psi information, and may even be aware that it arrived via anomalous means. This leads to *declarative knowledge*, which is knowing that something is the case, and is knowledge that can be verbalised. The recipient can then choose whether or not to act on the psi information. An example of this was related by Stanford (1982). In brief, while driving home at night Stanford felt a strong urge not to proceed along the main road, but to visit a nearby beach where he frequently watched birds. As it was night time,

this impulse seemed irrational, and Stanford concluded that it might be a psi-mediated warning that there was danger on the road that he would otherwise have taken. To test this theory, he proceeded slowly and cautiously along the main road, and discovered that there had been an accident, and the road was partially blocked. Had he been travelling at normal speed, this would have been a dangerous situation for himself, his passenger, and possibly for others.

The second way is that psi information, without the recipient gaining declarative knowledge of it, could produce an intuitive feeling or a hunch that could influence behaviour. The recipient would be aware of the action they took, but would not know why they were taking it. In this case psi leads to *procedural knowledge*, which is knowing how to do something, and is non-verbalisable. Kihlstrom (1987) stated that procedural knowledge is unconscious, in the strictest sense of the word, although this arguably a grey area. Kihlstrom included skills that have become automatic and the rules of grammar as unconscious too, but it is possible to introspect on these, and it is the capacity to introspect on mental activity that Kihlstrom used to classify conscious mental states. One example of this pathway was reported by a student nurse, who had felt a sudden urge to enter an obstetric ward, which was out of bounds to her. She did not know why she felt this urge, but on arriving there found a baby bleeding profusely, whose life she saved by this action (L. E. Rhine, 1962).

Lastly, psi information, without the recipient being aware of it, could bias behaviour that the recipient is not aware of doing. One example of this is mistakes. An example from Stanford's (1974) records was a girl who mis-dialled her friend's phone number, and instead reached an elderly lady having a heart attack. The girl was able to have the number traced and send help. Therefore, whichever level of awareness recipients have of psi information, the response can be a change in their behaviour. Thus, behavioural responses can be used to indicate a psi effect, irrespective of the level of awareness people have of psi information. In all the examples described above, the recipient had no conscious intention to access the psi information.

In the experiments presented in this thesis, in line with the examples and the models of psi presented above, we examined nonintentional behavioural responses to hidden psi influences or information. In these experiments, it was not essential that participants never gained conscious awareness of the psi information (and therefore responded with unconscious behaviour). It was, however, important that participants were not informed that we were testing for psi.

1.7. Outline of Thesis and Contributions

The structure for the remainder of the thesis is as follows. Chapter 2 presents a literature review of nonintentional psi experiments. In this, we describe the previous research methods, issues, and findings in nonintentional psi and PMIR experiments. In addition, we consider personality characteristics that have been found to influence psi performance.

Chapters 3 and 4 present two empirical studies that aimed to discover whether hidden targets can influence participants' behaviour in psychological tests. These studies also investigated the relationship between personality characteristics and nonintentional psi. Experiment 1 (Chapter 3) investigated participants' performance on a nonintentional psi task disguised as a line-length judgement task. This aimed to create a psi influence analogous to the social pressure to conform, which reliably influenced people's line-length judgements (Asch, 1952). In our study, we investigated whether psi information in hidden targets could exert a similar effect on line-length judgement. Experiment 1 found a null overall effect, but there was an indirect indication of a psi effect in two ways: in a *response bias effect* and in a correlation between participants' extraversion and hit rates (see Chapter 2). However, Experiment 1 did not incorporate any incentive for participants to respond in accordance with their hidden targets, and an incentive might be important in eliciting psi (see, e.g., Stanford, 1974). Therefore, in Experiment 2 (Chapter 4), we created an incentive by using a PMIR paradigm of a reward or punishment

contingent upon performance in a nonintentional psi task. The task itself was a conceptual replication of previous research by Stanford (1976,a,b) into the possibility that unconscious changes in timing would nonintentionally respond to psi. Experiment 2 found an overall null effect, and also null correlations between personality variables and psi score. These studies contribute a thorough investigation of a range of personality characteristics in nonintentional psi. They also extend the previous body of research by considering two new nonintentional psi tasks.

Chapter 5 presents a general discussion of Experiments 1 and 2, relating their findings to the previous literature and showing how they extend it. We consider reasons for the predominantly null effects, and suggest improvements for future research, considering the optimal conditions for investigating psi. We suggest that research into nonintentional behavioural responses to psi might benefit from direct comparison with behavioural responses to analogous non-psi stimuli, which we had not included in Experiments 1 and 2. Thus, we juxtaposed a proven social influence (social facilitation) and a conceptually related psi influence (remote observation) in the experiments in the second half of this thesis. This novel combination seeks to answer the original question of whether psi-mediated observation influences behaviour similarly to observation from a physically present observer. It is important to discover whether covert observation routinely used to assess performance in psychology experiments, and in general surveillance technology, has any effect on behaviour in and of itself.

Chapter 6 presents a review of the literature and a detailed description of both social facilitation and remote observation. In this review, we consider the backgrounds, issues, and findings of both fields, and present a case for our novel investigation of these phenomena in combination. Chapter 7 presents two empirical studies (Experiments 3 and 4). Experiment 3 explored the paradigm of testing for a social facilitation effect from remote observation. In Experiment 3 we compared participants' performance on covert tasks while they were alone, observed (by a

physically present observer), or remotely observed (by a *hidden observer*). This experiment found an overall null effect, and Experiment 4 aimed to improve upon the methodology involved, more closely replicating the most suitable previous research designs from the literature. Experiment 4 compared participants' task performance while alone or observed (by a physically present observer), and again found overall null results. The expected social facilitation effect (of simple task facilitation and complex task inhibition) was not found in either Experiment 3 or 4, but the results were suggestive of an overall facilitatory effect from a physically present observer. For this reason, different tasks were used in Experiment 5, intended to capitalise upon this effect. In this experiment (Chapter 8) we compared participants' task performance while they were alone, observed (by a physically present observer), or remotely observed (by a hidden observer). Experiment 5 found null results.

Chapter 9 presents a general discussion of Experiments 3, 4, and 5; suggestions for how this novel paradigm could be extended in the future; and the implications of the predominantly null findings for both fields. Chapter 10 presents the final discussion and conclusions, relating the findings of all the experiments as a whole back to the original aim of investigating nonintentional behavioural responses to psi, and summarising the contributions of these studies to current parapsychological knowledge.

2. Literature Review of Nonintentional Psi and Personality

2.1. Overview

This chapter presents a review of previous experimental literature investigating nonintentional psi in general extrasensory perception (GESP) paradigms. Firstly, we explain several fundamental aspects of nonintentional psi-testing paradigms and how they differ from intentional psi testing. Secondly, we consider the importance of randomisation in psi experiments, which is relevant to the studies subsequently discussed. Then we consider nonintentional psi in the following tasks: standard psychological tests, timing in word association tasks, affective judgement tasks, and classroom exams. In accordance with the methodology of Experiments 1 and 2, this review focuses on experiments with either hidden targets or forced-choice testing methods. Within this chapter, we also review personality theory, because linking personality characteristics and psi is an important part of parapsychological process-oriented research (i.e., investigating relationships between known psychological variables and psi). Following an overview of relevant personality characteristics, we review findings relating to personality and psi, both in the previous parapsychology literature in general, and in the nonintentional psi literature specifically. We conclude this chapter with a brief motivation of the studies in the following chapters (Experiments 1 and 2).

2.2. Intentional and Nonintentional Psi-Testing Paradigms

In intentional psi experiments, participants know that there is a target (i.e., information to be acquired through psi), or a sender (i.e., a person who attempts to transmit that information), but not what the target information is. Nonintentional psi experiments, in contrast, create a situation in which participants are not aware that they are being tested for psi, and consequently do not intentionally use psi; nonetheless, psi apparently manifests in their behaviour (Schechter, 1977). In

nonintentional psi experiments the fact that there is a target is hidden from the participants; thus, these are called hidden targets. Likewise, if there is a sender in a nonintentional psi study, this is also concealed from the participants, and these can be called *hidden senders*. In nonintentional psi experiments, participants perform a task which is, unbeknown to them, a *vehicle* for nonintentional psi, i.e., a task in which psi is expected to manifest.

2.3. Randomisation

The concept of randomness is important in designing psychology experiments. Random means without pattern. Randomness obviates biases, such the potential bias from an experimenter choosing which participants to allocate to experimental or control groups. Randomisation of target lists reduces the risk of a spurious effect caused by participants having a similar response pattern to a pattern in a non-random list. It is also preferable to create randomised target lists individually for each participant. This avoids the *stacking effect*: a spuriously low or high hit rate resulting from shared response biases between participants coinciding with, or deviating from, a shared target sequence (Thalbourne, 2003). Randomisation is particularly important in parapsychology because of the controversial claims that are made. If there is any reason other than psi that could explain an experimental effect, such as a pattern in non-random target sequences, then the psi hypothesis has to be rejected.

Random number tables are the best source of random sequences because they have been checked for randomness. Random entry into the table of random digits by using a non-determinable source, such as the weather forecast or a calculator's random digit function to indicate starting page, line, and column is the method usually used by parapsychologists (e.g., Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). Stanford (1990) argued for the careful use of random entry into random number tables, to avoid potential problems caused by *experimenter psi*. Experimenter psi refers to the possibility that the experimenter, intentionally or nonintentionally, could

use psi to influence the outcome of the experiment. For example, if a participant guessed above chance on a sequence of randomly created targets, it could be argued that the participant did not use psi, but that the experimenter precognitively created the target list that the participant would respond with. This theory is counter-intuitive, and explains one psi event with another. Experimenter psi has, however, been raised as a counter-possibility for many psi events, and should be considered until it has been disproved. Dice and card shuffling are non-optimal because they could be affected by nonintentional biases from the person rolling the die or shuffling the cards. One random entry into a random number table is the preferred method. Computer generated random number sequences can also be used (e.g., Luke, Delanoy, & Sherwood, 2008; Luke, Roe, & Davison, 2008). The experiments reviewed below used one of these optimal methods of randomisation, unless otherwise stated.

2.4. Nonintentional Psi in Psychological Tests

It is important to consider whether psi can nonintentionally affect standard psychological tests, for several reasons. If psi allows for nonintentional information transfer or influence, such as clairvoyance from the answers to a test, this means that psi could “contaminate” test results (White, 1976). If psi affects mental functions, such as memory, in standard tests, this would show that psi interacts with known psychological processes. Lastly, experimenter effects have been found in parapsychology: Some experimenters typically find evidence for psi and others do not (Palmer, 1997; Smith & Savva, 2004). If information from the experimenter can nonintentionally affect participants’ performance, this could partially explain the experimenter effect (Kreitler & Kreitler, 1972).

Stanford (1970) investigated whether memory of a story could be influenced by psi. Participants listened to a story, and later answered a series of unexpected multiple choice questions, each with four options. The questions referred either to specific

details in the story, to details which could have been inferred from the story, or to details which were never specified (such as asking the colour of a coat only described as ‘bright’). For each participant, one answer to each question was selected as the target after the questions were answered. Answers that matched the selected targets were scored as hits. For details that had not been specified, the hit rate of 31.2% was significantly higher than MCE of 25%. A greater proportion of hits were achieved in answers that contradicted the information in the story (36.2%), especially when those details had been specifically mentioned in the story (46.9%). It was rare that participants gave responses that contradicted specific details in the story, but when they did so, these answers had the highest hit rate. This pattern of rarely made responses being more likely to be hits when they are made is called the response bias effect, and as it occurs in many of the experiments subsequently presented we describe it below.

The response bias effect is a proportionally higher hit rate on responses that are not favoured by any other (non-psi) biases (Stanford, 1967). Stanford (1967, 1973) suggested that people are more likely to notice, or act upon, information that might be psi-mediated if it is somehow out of the ordinary or incongruous. For example, if someone has the thought “something terrible has happened to John”, many factors determine whether they will think that this is due to psi, and many factors determine whether they will act upon it. One of these is how frequently they think that something terrible might have happened to John; if this is an infrequent thought, it is more likely to be noticed and acted upon (Stanford, 1967). The response bias effect requires a *response bias* to exist in the first place. A response bias is a tendency to give one response more frequently than others; for example, responding ‘A’ more than ‘B’, ‘C’, or ‘D’ on a multiple choice exam. The response bias effect might occur because the non-psi bias adds noise to the responses favoured by the bias, diluting the proportion of hits. Following the multiple choice analogy, this would mean that when a student did not know the answer, he would answer ‘A’. ‘A’ would therefore contain a lower proportion of correct answers, as it also includes all the guesses.

Stanford (1970) proposed that the response not favoured by the bias is unlikely to be chosen when it is not the target, thus reducing the false alarm rate. This response bias was first investigated by Stanford (1967) in an intentional, clairvoyance-style psi task. He found that the targets with fewer responses had a higher proportion of hits. The response bias effect is a relative effect; it is not the case that there would be a greater number of hits in the responses that are rarely made, but a greater proportion. This response bias effect was found in the following series of experiments investigating nonintentional psi in psychological tests.

Kreitler and Kreitler (1972) aimed to discover whether psi influences could facilitate the recognition of subliminally presented letters. Participants were ostensibly recruited for a perception study, and were randomly assigned to act as sender or receiver. Sender and receiver did not meet, and the receiver did not know of the existence of the sender. The receiver vocally identified capital letters projected tachistoscopically for 10 milliseconds onto a wall. Luminosity was individually adjusted so that receiver guessed $\frac{1}{4}$ of the letters correctly. The experimenter was blind to the order of the letters, and did not watch the projections. The sender was located in a non-adjointing, sound-proofed room, and concentrated on sending psi targets to the receiver. Each letter was presented to the receiver twice, once with the sender concurrently concentrating on the same letter, and once with the sender concentrating on a control picture that was not associated with any letters. The experimenters and participants did not leave their separate rooms during the experiment, ensuring that there was no risk of sensory leakage or cueing. Significantly more letters were correctly identified when the same letter was being sent than during control trials. This effect was restricted to letters that were recognised less than 33% of the time in the control condition. The recognition of letters that were more easily detected was not improved. This experiment therefore supported the existence of nonintentional psi. It also replicated the response bias effect (Stanford, 1970) in that only responses with a low probability of success without psi were improved. Both the significant psi effect and the response bias

effect were independently replicated using a very similar procedure (Lubke & Rohr, 1975).

Kreitler and Kreitler's (1972) second study into the effect of psi in standard psychological tests investigated the effect of a sender on the perception of autokinetic motion. Autokinetic motion is the apparent movement of a stationary point of light in an otherwise dark and featureless space. Participants, ostensibly recruited for a perception study, were randomly assigned to act as sender or receiver. Sender and receiver did not meet, and the receiver did not know of the existence of the sender. The receiver sat in a dark room, continuously describing the apparent movement of a spot of light as being static, or as moving in one of eight directions (up, down, left, right and the four diagonals). The experimental time was divided into four blocks. In the third block, a sender, in a non-adjointing, sound-proofed room, concentrated on one of the directions, pre-selected at random. There was no overall psi effect, but participants with a below-median calling rate of a direction in the control blocks showed significantly increased calling of that direction when it was the psi target: a response bias effect. The last experiment in this series is not considered in this review as it neither investigated the effect of hidden targets, nor was it a forced-choice task.

The strongest evidence for psi in the three experiments reviewed above had been found in the letter recognition task, which combined a subliminal stimulus and a psi stimulus. To further investigate how subliminal and psi influences interact they were directly compared in a follow-up experiment (Kreitler & Kreitler, 1973). Specifically, they tested whether a hidden sender would influence a receiver's perception of the relative size of geometric shapes. They also sought further to investigate the response bias effect, that psi had manifested in their previous experiments only in the responses which otherwise had a low occurrence (Kreitler & Kreitler, 1972). Participants, ostensibly recruited for a perceptual judgement task, were randomly assigned the role of sender or receiver, and were taken to non-

adjoining, sound-proofed rooms by different experimenters. The receivers knew neither that there were senders, nor that there were psi targets. The stimuli presented to the receiver were a combination of supraliminal and subliminal images. The supraliminal images were either two lines of identical length, or two circles of identical size, presented at full luminosity through a tachistoscope for eight seconds. In some trials there were additional subliminal images, which made visual illusions, such as the classic Müller-Lyer illusion (see Figure 1), by adding subliminally presented arrowheads to the supraliminally presented lines.

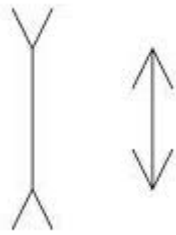


Figure 1: The classic Müller-Lyer illusion

The other subliminal illusions were a variant of the Müller-Lyer illusion with longer or shorter parallel lines beside the supraliminally presented lines and the Delboeuf illusion for the circles, both of which make one shape appear larger than the other. For simplicity, the description below will refer predominantly to the classic Müller-Lyer illusion on the lines. Firstly, just the supraliminal stimuli were shown to the receivers, who were told that the difference between the lines was very small, but to answer *left* or *right* for the line they thought was longest. These trials established participants' response bias baseline: their tendency to call left or right most frequently. Secondly, the subliminal stimuli were added, without a psi influence, to establish that the subliminal stimuli were truly below the level of conscious awareness and did not affect participants' responses. Lastly there were the psi trials. The sender concentrated on images of pairs of lines (or circles) clearly different in size, the order of which was individually randomised for each participant. The sender concentrated on the side on which the longer or larger image was, for example "*the*

left line is longer” (Kreitler & Kreitler, 1973, p. 176). In the first block of trials the sender had been instructed to concentrate intensely on the images, but had not been told that there was a receiver. In the remaining trials the sender was told about the receiver and attempted to send the images to the receiver. Within each block, the three conditions: psi influence alone; concurring psi influence and subliminal illusion (e.g., both the sender and the illusion indicate that the left line is longest); and contrasting psi influence and subliminal illusion (e.g., the illusion would make it appear that the left line is longest, but the sender is looking at a longer right line) were randomly varied. There was a significant interaction between sender mode and influence type, and a positive deviation from MCE, when the sender was “sending” and the psi and subliminal influence contrasted. This occurred for the classic Müller-Lyer and Delboeuf illusions, but not for the other variant of the Müller-Lyer illusion, possibly because it was the weakest of the illusions. Kreitler and Kreitler (1973) interpreted this as meaning that psi signals would normally be subsumed by stronger sensory signals, “arrive” in a usually unattended channel, and are only acted upon when they contrast with other available information. It could also be interpreted as the response bias effect. The psi information only appeared to exert an influence when it contrasted with the subliminal information, the condition in which the psi target answer would be the least likely response.

The strengths of the Kreitlers’ experiments were the controls against sensory leakage and experimenter bias. There was no risk of sensory transfer of information between the participants or experimenters throughout the experiment. The participants were randomly assigned to conditions, the targets were optimally randomised, and the experimenters were blind to the targets. The authors did not act as the experimenters, but recruited others who were kept blind to the full details of the experiment and its aims until all testing was complete. Thus the authors did not meet the participants and could not bias their performance by any form of social interaction. While all the experiments found interesting psi effects in the comparisons between conditions, only the perception of subliminally presented letters showed an overall deviation

from chance. While this could be used to argue against the psi hypothesis, it is important to consider reasons for these findings. There were aspects of these experiments that might not have been psi-conducive. Sceptics were deliberately chosen as experimenters, and were advised that a psi effect was not a desirable outcome. Experimenter belief and attitude has been found to affect psi experiments, (e.g., Wiseman and Schlitz, 1997). It might also have been psychologically difficult for the sender to send to an unknown receiver.

The final pair of experiments we will consider that used a psychological test as a vehicle for nonintentional psi aimed to investigate whether psi could influence the interpretation of homophones (Wilson, 2004). In both experiments, participants were ostensibly recruited for a word association test. Participants individually listened to a tape-recording of homophones and gave the first word association that came to mind. The homophones all had two main alternative interpretations, such as steal/steel, one of which had been predetermined to be the psi target. Just before the participants heard each homophone, a sender in a non-adjacent room focussed on a visual image of the psi target interpretation (e.g., a picture of a burglar for 'steal'). In these experiments the experimenter took the role of the sender. To establish rapport between the participant and the sender, they met before the trials took place; although the participants were not informed that there would be anyone acting as a sender. The overall hit rates for both experiments were non-significantly different from MCE. In the first experiment the response bias effect was found. There was a greater proportion of hits in the less frequently given homophone interpretations. In the second experiment the response bias effect was not found. This failure to replicate could have been because participants' response preferences were determined from the combined responses of all the participants. Wilson recommended that future research use response biases calculated at the individual participant level, which was not possible in his experiments as each homophone was only used once.

The experiments reviewed above, taken together, provide some evidence that psi information in hidden targets or from hidden senders can influence performance on psychological tests. The effect exerted by psi information is not great, and appears to manifest most in the least frequently given responses. One criticism of this style of study is that there was no “need” in these experiments for the participants to perform in accordance with their psi information, which might explain the low overall effects. A main premise of Stanford’s psi-mediated instrumental response (PMIR) model is that psi is need serving and goal oriented (Stanford, 1990). Therefore, people respond nonintentionally to psi information in a way that fulfils their needs. Needs can be utilised by creating a situation, unbeknown to the participants, in which behaviour on a nonintentional psi task will lead to them receiving a reward or a punishment. This reward and punishment contingent upon performance in a nonintentional psi task is the basic set-up for PMIR experiments. The future reward and punishment conditions are thought to act as an incentive to motivate the participants to use psi, although the participants are unaware of the contingency. All PMIR experiments are therefore nonintentional psi experiments, and the following section reviews those that investigated the effect of psi on timing.

2.5. Nonintentional Psi and Timing of Word Association Responses

One of the ways in which behaviour might respond nonintentionally to psi, and bring about a need-relevant outcome, is by a change in timing (Stanford, 1990; Stanford & Thompson, 1974). Stanford (1974) proposed that PMIR would manifest in the most economical way possible, and that the appropriate response would automatically be selected through psi. If, for example, the disaster of driving over a bridge as it collapsed could be averted by simply driving over the bridge a little faster and reaching the other side before it collapsed, then this change in timing might occur through PMIR. This is economical in that the driver would not need to have a conscious premonition of the disaster, would not need to change his intended course

of action, and would not even need to know that psi had influenced him, all of which are aspects of how PMIR is thought to function (Stanford, 1990). We review below PMIR experiments that used a change in timing on word association tasks as the vehicle for nonintentional psi.

Stanford and Thompson (1974) recruited college-age male participants for a precognition task, a test of intentional psi. After completing the precognition task, the participants were then asked to do a word association task, ostensibly to investigate the thought processes associated with precognition. Each participant individually listened to 13 words played on a tape-recorder, the first three of which were a practice. After each presentation of a stimulus word, participants immediately voiced the first word that came to mind. Prior to the experiment, an assistant had randomly selected one trial to be the “key” trial, and also randomly selected whether the target direction for the key trial was fast or slow. The experimenter was blind to the identity and direction of the key trial. If the key trial was *fast* and the participant’s reaction time for the key trial was the fastest, or joint fastest, of their 10 responses, the participant went on to do a reward task; if the key trial was not the fastest he performed a punishment task. The reverse applied for the *slow* targets. The reward task was to rate pictures of attractive women, in various states of undress, on a variety of adjectives. The punishment task was circling certain letters on sheets of random letters. Performance on the nonintentional psi task was non-significantly different from chance. Participants’ scores on the precognition task, however, correlated positively with their performance on the nonintentional psi task, lending partial support for timing responding nonintentionally to psi.

Using timing in word associations as the vehicle for nonintentional psi, Stanford and Stio (1976) also aimed to investigate two sub-propositions of the PMIR model. Firstly, they tested whether participants with a greater need for the reward more frequently entered the reward condition. The strength of the all-male participants’ need to enter the erotically-toned reward condition was manipulated by playing a

randomly selected half of the participants a banned erotic song prior to the experiment. The other participants listened to it afterwards. Secondly, they tested the proposal that PMIR occurs by the facilitation of readily available responses. On the basis that voicing the first word that comes to mind is a readily available response, Stanford and Stio (1976) hypothesised that it would be more likely to be facilitated (sped up) by psi than inhibited (slowed down). Therefore, they predicted when the target direction was *fast* participants would be more likely to achieve a hit than when it was *slow*. The word association task was conducted as in Stanford and Thompson (1974). The reward condition was a mildly sexually arousing relaxation and free-response ESP task, conducted by a female experimenter. The punishment was a 25-minute forced-choice ESP task. There was no significant effect from the need-strength manipulation, which Stanford and Stio (1976) attributed to the song not producing as much arousal as expected. They recommended that manipulations are checked to avoid this problem in the future. An alternative explanation, if precognition is viable, is that it made no difference presenting the song before or after the experiment, as both could have an effect (Schechter, 1977). Response times for the key trials when the target direction was *fast* were faster significantly more often than chance. When the target direction was *slow* the key trial timings were non-significantly different from chance, supporting the facilitation of ready responses hypothesis. The overall comparison to chance was not reported. It is possible that a stronger effect was produced in this experiment than in Stanford and Thompson (1973), in which the experimenter was male, because the female experimenter increased the participants' need to enter the arousing reward condition.

Stanford and Associates (1976) directly compared the effect of attractive female experimenters and male experimenters. The hypothesis was that the female experimenters would increase the all-male participants' need to enter the erotically-toned reward condition. The word association task was as described above. The reward condition was rating pictures of attractive women, as used in Stanford and Thompson (1973), and the punishment was a 25-minute pursuit rotor task, in which

participants tracked the movement of a light. Participants tested by females scored significantly higher than MCE, confirming the need-strength hypothesis. The overall outcome for both the male and female experimenters combined was not reported. Stanford and Associates (1976) reported evidence of a nonintentional psi effect that supports the need-strength hypothesis of the PMIR theory, but did not report evidence for an overall psi effect.

The experiments discussed so far have concentrated on a reward and punishment condition for the participant him- or herself. This was extended to examine whether participants will nonintentionally and unknowingly put someone else into the reward condition (Stanford & Rust, 1977). Thirty participants did the word association task previously described, with all the psi targets in the faster direction. Each participant was matched to a male *helpee*, who was given a reward task of rating pictures of partially dressed women, or a punishment: the pursuit rotor task. The participants and helpees did not know each other, did not meet in the experiment, and were neither told about each other, nor the relationship between their tasks. Seven helpees went into the reward condition, significantly more than the MCE of three, but there was no overall psi effect found when comparing the key trial and mean response times. This was a failure to replicate Stanford and Stio's (1976) finding that there is a greater psi effect when the target direction is *fast*. This experiment found evidence that people nonintentionally help others through psi, but only when the direct hit measure was considered.

In summary, this series of experiments that used incidental timing of word associations as a vehicle for nonintentional psi found evidence in line with PMIR hypotheses, but their evidence for nonintentional psi per se is limited, as the overall hit rates were not compared to MCE. Additionally, one further study did not support any PMIR hypotheses, or the existence of nonintentional psi, using a very similar paradigm (Stanford & Castello, 1977). One limitation of this group of experiments that they were all conducted by one main experimenter: Stanford. As the

experimenter effect is an issue in parapsychology, independent replication by other researchers is important. These experiments all also used a very similar word association task, and conceptual replication of the timing paradigm with a different task would help to validate or abrogate it. These experiments aimed to create a laboratory analogue of the spontaneous psi experience of coinciding with need-fulfilling circumstances by being faster or slower, relative to other events. In these experiments just reviewed, the task was to make one key trial faster or slower than the control trials. Individual differences in reaction time in the word association trials could have interfered with any psi effect. A measure that relies on overall timing without other confounds might be preferable. In addition to timing, other vehicles for PMIR have been researched; this review now considers more recent research using affective judgement tasks.

2.6. Nonintentional Psi in Affective Judgement Tasks

The three most recent studies into nonintentional psi aimed to test aspects of the PMIR theory with a precognition task, and shared the same basic procedure (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008). Participants chose their favourite of four fractals, pre-matched to be neutrally pleasant, believing this to be preparation for a later psi task. As soon as participants registered their choice by clicking the fractal image onscreen, one fractal was randomly selected by the computer program to be the psi target. If this was the same fractal the participant had chose, it was a hit. If, after the 10 trials were complete, the participants had scored above MCE (2.5 hits), they went on to perform a reward task, for a longer duration the more hits they had achieved. If they scored under MCE they went on to perform a punishment task, for a longer duration the fewer hits they had achieved. The punishment was a vigilance task (indicating when three consecutive odd or even numbers appeared onscreen) that was made more unpleasant by not informing the participants how long it would last. The reward task varied between the experiments, being either rating erotic pictures (Luke, Delanoy et al., 2008) or rating cartoons (Luke, Roe et al.,

2008). In all three experiments the participants achieved more hits than MCE. The mean effect size across all the experiments ($r = .35$) was a medium to large effect size (J. Cohen, 1988). One of the strengths of these experiments was that there was no risk of sensory leakage of the target identity to the participant, as the psi targets were precognitive, and had not been generated at the time the participants rated each set of fractals. The experimenter was also blind to the identity of the future targets, eliminating the risk of inadvertent cueing.

These experiments also aimed to test the PMIR sub-proposition that psi responds to participants' needs, by investigating whether reward and punishment contingencies are necessary. In one experiment, in addition to the reward and punishment conditions, some participants were randomly assigned to a *no-contingent* condition, in which they left at the end of the affective judgment task, and did not do the reward or punishment task, irrespective of their psi performance (Luke, Roe et al., 2008). Contrary to expectation, the participants in the no-contingent condition performed best. This could be interpreted as contradicting the PMIR theory, because the participants without a reward or punishment contingency performed best. However, the participants in the no-contingent condition were able to leave earlier, which might have been a greater incentive than rating cartoons. Therefore, an alternative interpretation is possible: The most psychically gifted participants used psi to enter into the condition in which they were able to leave soonest, and had no risk of getting the punishment. This, once again, demonstrates the need for manipulation checks to verify the validity of the reward and punishment conditions that are used to provide an incentive for participants to use psi. In their follow-up experiment, Luke et al. (2008) checked participants' ratings of the reward and punishment conditions, and found that cartoons were rated as significantly preferable to the vigilance task. However, that experiment did not include a no-contingent condition, so participants' relative preference for it was not assessed.

Other PMIR experiments with affective judgement tasks did not find support for nonintentional psi (Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). In both these studies participants rated 13 images of Japanese Kanji script characters, pre-selected to be neutrally pleasurable, for how pleasant they found them. The first three characters were a practice. Of the remaining 10 characters, one had been randomly pre-selected to be the psi target. Each psi target (the number of the character) was placed in an opaque envelope, with a number on the outside corresponding to each participant's number. During each participant's trials, the experimenter kept the relevant target on her person throughout the whole procedure, while remaining blind to the target identity until after experimentation was concluded. Participants scored a hit if they rated an odd numbered character highest if their target was an odd number, and vice versa for even numbers.

In Watt and Nagtegaal's (2000) experiment participants with a hit could choose their reward between either leaving early without performing another task, listening to relaxing music, or playing an asteroids game on the computer. Participants without a hit performed a computer-based vigilance task as punishment. In Watt and Ravenscroft's (2000) study, a hit (or miss) by the participant performing the nonintentional psi task resulted in a second participant, a helpee in a separate room, entering the reward (or punishment) condition. Neither participant was informed about the other, nor that their tasks were linked in this way. This was a conceptual replication of the PMIR and rewarding others experiment by Stanford and Rust (1977). There was no significant overall psi effect found in either of Watt's experiments. A strength of both experiments was that the participants' ratings of the reward and punishment tasks were compared; the rewards were rated as significantly more preferable than the punishment. The Watt studies used leaving early as a reward condition, whereas Luke et al. (2008) used leaving early as a condition without reward or punishment. Eight of the 40 participants in the reward condition chose to go early, and rated it as 4/6 when asked how much they valued the chance to go early (Watt & Ravenscroft, 2000). This was slightly less than the other two

reward options, which both scored 4.6/6 on pleasantness. This difference, however, might have been in part due to the different wording of the questions (how much they *valued* leaving early, versus how *pleasant* the tasks were), a limitation of these studies. None of the five participants with a hit in Watt and Nagtegaal's (2000) experiment chose to leave early. This indicates that leaving early is a moderately favourable choice for participants, and thus it follows that the no-contingent condition might have acted as an incentive in Luke et al.'s (2008) experiment, as suggested above.

There were some major similarities between Luke's and Watt's experiments; both used a forced-choice nonintentional psi task, and both used affective judgements. Forced-choice affective judgements respond to unconscious information whereas, for example, forced-choice recognition tasks do not (Kunst-Wilson & Zajonc, 1980; Merikle & Daneman, 1998). They might, therefore, be well suited for psi experiments, if psi information is non-consciously received. Taking the affective judgement nonintentional psi experiments overall, three found a significant psi effect (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008) and two did not (Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). Possible reasons for the difference in findings are the experimenter effect, different populations, and random variation. The most viable non-psi explanation is that there was an artefact in Luke et al.'s experiments that could explain the findings. In summary, these studies provide some proof for nonintentional psi.

2.7. Nonintentional Psi in Exams

The final area of nonintentional psi research reviewed here concerns whether there is any psi effect from hidden targets on university students' exam performance. This is conceptually related to the first section of this review, which considered whether psi could play any role in standard psychological tests, as exams are a standard testing situation and an area that is not normally thought to be affected by psi. The exam

studies are also conceptually related to the PMIR studies reviewed above, in which the role of motivation was considered, because the students in these exams were thought to have a high level of motivation to succeed. The exam studies also aimed to investigate spontaneous nonintentional psi in a real-life context (Johnson, 1973).

The pioneering research was conducted in a series of three experiments on students writing short, essay-style answers to eight psychology exam questions (Johnson, 1973). In the first two experiments, the correct answers to four questions were randomly pre-selected as the psi targets for each student. Target packs were created, with the psi target answers photocopied into the relevant spaces on the exam papers, and blank spaces following the control questions. To ensure students could not see the targets, they were covered in aluminium foil and sealed in envelopes. The target packs were stapled to the actual exam papers, with the psi target answers aligned with the spaces into which the students would write their answers. The students were told that the answer packs were to facilitate marking the exams, and not to open them (no envelopes were opened). The exams were scored by raters, who were blind to the target identities, before being matched to the targets to determine the hit rates. In both experiments, students' answers to questions with hidden psi targets scored more points than the control questions. One limitation of both of these experiments was in the marking of the exam questions. In the first experiment the two raters' scores were significantly different for the subset of the exams they both rated, and Johnson did not specify whose ratings were used for the analysis. In the second experiment there was only one rater. These two experiments found evidence of nonintentional psi, and support the PMIR model as the students nonintentionally responded to the psi targets in a way that fulfilled their needs of increasing their exam scores.

Johnson's (1973) third experiment used a very similar set-up and target packs, but incorporated negative psi targets. The negative psi targets had two parts: false, but relevant, information for the exam question and a discouraging statement, such as "You are too stupid to pass this examination." (Johnson, 1973, p. 213). The "control"

exam questions were accompanied by encouraging statements only, and no information relating to the answers. Inter-rater reliability was improved in this experiment. Scores for answers with negative psi targets were lower than those with encouraging statements. It is, however, debatable whether this experiment supports the PMIR hypothesis. It would depend upon whether the difference in scoring between the negative and positive psi target answers were due to a reduction on the scores for the negative target answers, or whether there had been an increase in the scores for the positively toned controls. The mean scores were lower in the third experiment, indicating that the negative targets might have reduced the scoring. The strengths of these experiments were the individually randomised targets, within-subject controls, and the measures taken to restrict sensory leakage. The weakness in the third experiment was the lack of a valid control condition. All three of the experiments by Johnson provide evidence for a nonintentional psi influence on behaviour.

These exam performance experiments were conceptually replicated by several independent investigators. The first replication failed to find any effect on exam scoring from the hidden psi targets (Willis, Duncan, & Udofia, 1974). (Two experiments were carried out, but in the second experiment the students were told that the psi target packs contained hints about the exam, so it was not a true nonintentional test, and is excluded from this review.) Psychology students were given 35-item multiple choice exams, and the answers to the first six questions were used as psi targets, between-subjects. The targets were presented similarly to those in Johnson's (1973) experiments in that the target answers were aligned with the space into which the students would write their answers, but no information was reported about safeguards against cheating. The students were told that the purpose of the answer packs was to facilitate scoring, as Johnson (1973) had. There was no psi effect in this experiment: Students' error rates did not differ significantly between the participants with the answers and those without. This could have been because there was no psi influence to be detected. There might also have been a ceiling effect, as

the error rates were very low. The psi targets could only confer 17% of the total score, (compared to 50% in Johnson's experiment) which, combined with the overall low error rate, meant there might have been inadequate incentive to motivate the students to access the hidden target information. Students' performance might also easily have been affected by order effects, because only the first six questions had psi targets (Schechter, 1977). This experiment had many flaws, and failed to test the impact of nonintentional psi from hidden targets in exams adequately.

A second replication of Johnson's (1973) experiments found support for nonintentional psi. W. G. Braud (1975) investigated the effect of hidden psi targets on a parapsychology exam with essay-style answers. The students received answer packs, made in a similar way to Johnson's, with good control against sensory leakage and cheating, with either one half (A), or the other half (B) of the correct answers as psi targets. 'A' or 'B' was clearly marked on the exam packs that students received, but they were not told what it meant. This might have introduced a systematic bias into which students received which pack, especially as the students were instructed to choose the pack that felt right to them. This might have changed the nature of the task, introducing an element of intentionality in what was purportedly a test of nonintentional psi. This is a less serious problem than it was in Willis et al. (1974), because W. G. Braud's (1975) experiment also included an intentional psi task, and the students were led to believe that the answer pack contained only the items for the intentional task. Error rates for the answers with psi targets were lower than for the control answers. Therefore, the psi targets appeared to help participants answer the questions. The limitations of this experiment were the non-random allocation of the target packs to the students and the lack of individually randomised targets for each participant. Participant's increase in performance on the answers with psi targets was inversely correlated with their performance on the control items. W. G. Braud interpreted this as a confirmation of the need-strength sub-proposition of the PMIR model, assuming participants with the poorest performance had the greatest need for the psi information. Stanford (1990) strongly criticised this argument, as the highest

performing students might have been the ones with the greatest need to pass. This disagreement highlights the need for checks of how much the participants desired the outcomes assumed to act as their incentives. This effect could also be interpreted as the response bias effect: The participants who performed worse were less likely to choose the right answers, and were those for whom the psi targets had a greater effect.

The most recent investigation into hidden psi targets and exam performance was conducted by Schechter (1977). Only the first two of his experiments are reviewed here as the third was less well controlled. The target packs were prepared and presented in a similar way to Johnson's (1973) with good controls against sensory leakage and cheating. The psi targets were the answers to either the odd or the even numbered questions. The answer packs were stacked so that they alternated odd and even psi targets, and were handed out to the students in sequence. Schechter (1977) found a difference in performance between the participants with odd numbered and even numbered psi targets, so there might have been a systematic bias in the way that the students received the packs. It is a limitation of this experiment that neither true random allocation of targets to participants, nor individually randomised target lists were used. In both experiments participants scored higher on the questions with psi targets than the control questions. The nonintentional psi hypothesis was supported by these findings.

In summary, the tests of nonintentional psi in exams predominantly support the psi hypothesis. Unfortunately, three of the experiments had potential randomisation problems, and only Johnson's (1973) experiments involved truly random allocation of targets to participants. Johnson was also the only researcher to create individual psi targets for each participant. Johnson's three experiments used the best methodology and provided the most conclusive proof for nonintentional psi.

One question raised by the exam studies is why the answers in the psi targets exerted an influence over each student's performance whilst other possible sources of psi did not. The other potential psi influences in these experiments included other students' answers, other students' knowledge, the teacher's knowledge, each student's notes and textbooks, and each student's future knowledge from the time when they could check the answers after the exam. Schechter (1977) considered a few possible reasons why the psi targets might have exerted an influence while the other sources did not. Firstly, the psi targets might have exerted a greater influence due to their physical proximity. W. G. Braud (2010) reviewed the findings relating to distance and psi. While he found some evidence for a decline in psi effects over distance, he attested that it was inconclusive, because participants' and experimenters' expectancies about distance were not controlled and extreme distances have not been tested. Proximity remains a possible reason for the effectiveness of the psi targets. Secondly, Schechter (1977) thought that the target packs might have caused the students to focus their attention on the material inside, somehow increasing psi information transfer. This applies to W. G. Braud's (1975) experiment as the participants were asked to focus their attention on the answer packets (for the intentional psi part of the exam). The targets might have been psi conducive because they had the same layout as the exam question and answer sheets (Schechter, 1977). The proximity and layout might both have led to cheating or sensory leakage, but there were good measures against the participants seeing the psi target answers in all the experiments with positive effects (W. G. Braud, 1975; Johnson, 1973; Schechter, 1977). It is not known why the information in the answer packs would have influenced the students' performance when other sources of psi did not, but psi target packs appear to be effective clairvoyance targets.

In summary, there is evidence of psi nonintentionally affecting participants' behaviour in standard psychological tests, in PMIR experiments in timing and affective judgement, and in students' exam performance. As this current thesis will explore whether *individual differences* in participant personality and other

characteristics play a role in parapsychological phenomena, this review will briefly relate the individual differences to be considered. Then it will consider the role of individual differences in parapsychology in general, before reviewing the evidence for individual differences effects in nonintentional psi tasks.

2.8. Individual Differences in Psi Research

Individual differences is the hypernym for variables, such as personality and IQ, that differ between people. As most of the individual differences that we will consider herein are personality variables we discuss those first. Contemporary psychology describes personality using the concept of *traits*: habitual patterns of thought and emotion that influence behaviour, that vary between individuals, and are relatively stable over time (Matthews & Deary, 1998). One of the most prevalent models of personality traits is the *Big Five*: extraversion, neuroticism, openness to experience, agreeableness, and conscientiousness (Costa & McCrae, 1992a). These traits emerged from factor analysis of personality-related adjectives, and represent a comprehensive empirical system for quantifying individual differences in personality. Each of these trait *domains* can be further subdivided into *facets*. For example, extraversion is made up of gregariousness, assertiveness, activity, excitement-seeking, and positive emotions. The Big Five traits that will be used in the experiments in this thesis are extraversion, neuroticism, and openness to experience. In addition to these personality traits, we will also investigate the role that two further individual differences: state anxiety and belief in psi, might play in psi task performance. All of these variables will be measured by questionnaire (the methodological details are described in the experimental chapters).

Eysenck (1967) emphasised the importance of considering the reliability of psi scores when comparing them to other factors, such as personality. On the evidence of the limited number of studies that reported reliability scores, Palmer (1977) noted that reliability varied considerably between studies, and he estimated that mean

reliability for psi scores would be positive, but low. Consequently, correlations between psi and individual differences are likely to be small and unstable, hence it is unwise to base conclusions on the outcome of individual studies (Palmer, 1977). Therefore, the review below will focus upon meta-analyses and reviews of multiple studies where possible. In the following sections, each of these individual differences characteristics are defined, and research evidence for their role in parapsychology research in general is outlined.

Extraversion is the personality trait associated with being outgoing and sociable. People with high extraversion are referred to as *extraverts*, and those low in extraversion as *introverts*. Extraverts prefer being in groups, are talkative, and like excitement (Costa & McCrae, 1992a). Four narrative reviews of the research into psi and extraversion independently concluded that psi performance and extraversion correlate positively (Eysenck, 1967b; Palmer, 1977; Rao, 1974; Sargent, 1981). Eysenck (1967) noted similarities between aspects of psi performance in general and extraverts' performance in general, such as decline over time and improved performance with novel tasks or stimuli. Rao (1974) and Palmer (1977) both noted instances of the relationship between psi and extraversion being reduced to non-significance if participants' neuroticism was also considered. The narrative reviews all used the vote-counting technique, counting statistically significant studies that provide evidence for, and against, an hypothesis. These reviews can be criticised for their over-reliance on statistical significance. The vote-counting technique also does not take into account the effect sizes of the studies, and cannot estimate an overall effect size.

In order to estimate the overall effect size of the relationship between psi and extraversion, a large meta-analysis was conducted, investigating 60 independent studies involving 2,963 participants (Honorton, Ferrari, & Bem, 1990, 1998). The relationship between psi and extraversion was analysed separately depending on whether the psi task was free response, e.g., Ganzfeld studies, or forced choice, e.g.,

guessing Zener cards. While the relationship between psi and extraversion was evident in free-response studies ($r = +.20$), the authors challenged the validity of the relationship between psi and extraversion in forced-choice studies. The studies that showed evidence for a positive correlation between psi and extraversion were those in which the extraversion measure was taken after the ESP test. Honorton et al. (1998) concluded that the relationship between psi and extraversion was artefactual, caused by the participants' knowledge of their psi scores having influenced their self-ratings of extraversion. For example, participants who were happy to have high psi scores might rate themselves higher on extraversion. Honorton et al. also suggested that the relationship between psi and extraversion might be an experimenter expectancy effect, caused by the experimenter's knowledge of the participants' ESP scores leading them to bias the participants on the extraversion measures.

Palmer and Carpenter (1998) challenged these suggested artefacts on the following grounds. Firstly, the test-retest reliability of extraversion measures is high; they are designed not to respond to state changes with any degree of magnitude. Secondly, Honorton et al. (1998) had not provided any evidence that these artefacts did in fact occur. Honorton et al.'s claims were investigated in an experiment specifically conducted to test whether informing participants that they have scored well in a psi task inflates their extraversion ratings (Krishna & Rao, 1991). Student participants first did a forced-choice ESP task, after which half the students were told that they had scored well on the psi task (irrespective of their actual scores); the other half were told they had scored poorly. The students then completed a High School Personality Questionnaire to assess extraversion. There was no difference in the mean extraversion scores between participants misinformed that they had high, or low, psi scores. Thus, the artefact of participants' knowledge of psi scores influencing their extraversion ratings was countered, although the effect of participants knowing their true psi scores was not tested. In a re-analysis of the same data used by Honorton et al. (1998), testing situation (individual versus group

testing) was found to be confounded with the order of the ESP test and the extraversion measure (Palmer & Carpenter, 1998). Testing situation appeared to be the explanatory factor, with a positive relationship between psi and extraversion in forced-choice tests being found only when participants were tested individually, with a comparable effect size to the free-response studies ($r = +.21$). However, Palmer and Carpenter could not rule out poor study quality as a confound in the relationship between psi and extraversion. There is a need for the relationship between psi and extraversion to be investigated further in good quality studies without risk of the artefacts mentioned above.

Palmer (1978) suggested three reasons for the relationship between psi and extraversion. Firstly, extraverts have higher spontaneity, which is apparently psi-conducive. Secondly, extraverts might create a more psi-conducive atmosphere by getting on better with the experimenter. The third reason relates to cortical arousal levels. Eysenck (1967) proposed that psi is an ancient form of perception, and might have developed prior to the cortex. Eysenck proposed that a central brain area, the ascending reticular activity system, governs psi, and that psi is inhibited by increased cortical arousal. There is evidence for low cortical arousal being psi conducive in prior literature. Honorton (1977) argued that sensory deprivation, which lowers cortical arousal, creates a need for participants to increase their arousal by seeking out psi-target information. W. G. Braud (1975) found that psi retrieval is associated with low levels of arousal. Lower arousal might be psi conducive because the mind becomes quiet, and one's attention is free to notice stimuli that might otherwise be overridden by stronger sensory information (W. G. Braud, 1981). As extraverts have lower cortical arousal levels (Matthews & Deary, 1998), they might have better psi ability.

The evidence for extraversion being psi conducive comes from intentional psi studies. As relaxation and sensory deprivation are also conducive to retrieval of subliminal information, a process of conscious access to non-conscious information

(Nash, 1986), this indicates that lower cortical arousal might facilitate the retrieval of unconscious information. It does not necessarily follow that extraversion is associated with psi “reception”. This can only be tested in situations in which there is no conscious guess component, such as nonintentional psi tasks.

In summary, there is likely to be better performance from extraverts in forced-choice psi experiments, although it is uncertain whether this will extend to nonintentional psi tasks. Extraversion should be considered along with neuroticism, in case these two characteristics interact (Palmer, 1977; Rao, 1974). The association between psi and neuroticism is considered below.

Neuroticism is the personality trait of being prone to negative states, such as “fear, sadness, embarrassment, anger, guilt, and disgust” (Costa & McCrae, 1992a, p. 14). The neuroticism domain comprises six facets: anxiety, angry hostility, depression, self-consciousness, impulsiveness, and vulnerability (Costa & McCrae, 1992a, p. 14). People with high neuroticism cope poorly with stress and have irrational ideas.

Previous research indicated that there is a tendency for low neuroticism to correlate with higher psi scoring. For example, in a series of studies investigating neuroticism and psi scoring on a card-guessing ESP task, high neuroticism was associated with below-chance scoring (Kanthamani & Rao, 1973). A review of 24 studies found that the seven significant findings all showed a negative correlation between psi and neuroticism, across many different personality scales, although some studies using the Manifest Anxiety Scale showed the reverse effect (Palmer, 1977), and this was corroborated by another review with overlapping studies (W. G. Braud, 1981). These reviews used the vote-counting method and are therefore not as strong evidence as an assessment of effect size through meta-analytic techniques.

Palmer (1977) also suggested that people’s reaction to anxiety affects how anxiety and psi relate. For example, people who cope with anxiety defensively, and therefore avoid anxiety-provoking stimuli, perform worse at psi tasks. Conversely, people with

increased sensitivity to anxiety, who are vigilant to anxiety-provoking stimuli, perform better at psi tasks. This was further explored in research with the Defence Mechanism Test, which investigates people's thresholds of perception of threatening subliminally presented stimuli (Haraldsson & Houtkooper, 1992; Haraldsson, Houtkooper, & Hoeltje, 1987; Johnson & Haraldsson, 1984). Defensive participants, those with higher thresholds for conscious awareness of threatening stimuli, were found to be poorer at intentional psi tasks. Haraldsson suggested these participants were also defensive against psi. Therefore, defensive participants had raised thresholds against psi information, making them less able to become aware of putative psi information. This paradigm was extended to include participants with vigilance, lower thresholds for threatening stimuli, who were found to have better psi scoring than defensives (Watt & Morris, 1995). Overall it appears that there is a negative relationship between psi performance and neuroticism.

The relationship between psi performance and neuroticism could be underpinned by differences in arousal. Neuroticism is highly correlated with trait anxiety, which is in turn highly correlated with sympathetic nervous system arousal (Spielberger, 1983). While neuroticism has not been found to correlate consistently with cortical and sympathetic nervous system arousal, even under stress (Matthews & Deary, 1998), there is some evidence of neuroticism-related cortical arousal (W. G. Braud, 1981). As reviewed above, higher cortical arousal in introverts is thought to be psi-inhibitive. In a similar way, high arousal in neurotic people might also be psi-inhibitive.

State anxiety is a temporary emotional state in which people feel tension and apprehension; it is also characterised by increased autonomic nervous system activity (Spielberger, 1983). In contrast to neuroticism, which is a stable trait concerning the tendency to become state anxious, state anxiety is transitory. If anxiety has any part to play in the relationships discovered between neuroticism and psi scoring described above, then it might follow that state anxiety also plays a role in psi task

performance. If, as W. G. Braud (1981) suggested, low autonomic nervous system arousal levels facilitate ESP performance, state anxiety might differentiate between low and high scorers.

There has been very little parapsychological research conducted with state anxiety in comparison to extraversion and neuroticism. Broughton and Perlstrom (1986; 1992) investigated participants' state anxiety, measured on the State-Trait Anxiety Inventory (STAI) (Spielberger, 1983), and psychokinesis (PK) in three experiments. Participants with higher state anxiety performed worse on a PK task, presented as a computer game, in which they rolled dice to win points. A negative relationship between state anxiety measured on the STAI and ESP was found in a dog-race computer game in which participants selected the dog they hoped would win (Roe, Davey, & Stevens, 2003). Participants with higher state anxiety were less successful in selecting the winning dog. This effect reached significance only when the dog-race was disguised as a PK task, i.e., when participants were attempting to will their dog to win, but the winner was actually predetermined. Considering this evidence and the conceptual similarity between state anxiety and neuroticism, we would predict a negative relationship between state anxiety and psi scoring.

People with high *openness to experience* are imaginative, creative, open to new and unorthodox ideas, and sensitive to their inner feelings (Costa & McCrae, 1992b). The facets of openness to experience on the NEO PI-R are openness to: fantasy, aesthetics, feelings, actions, ideas, and values (Costa & McCrae, 1992b). Although openness to unorthodox ideas and sensitivity to inner feelings have intuitive appeal as psi-conducive characteristics, there has been surprisingly little research investigating psi and openness to experience.

A positive relationship between psi performance and openness to experience has been shown in a handful of previous investigations. A significant positive correlation was predicted, and found, between openness to experience measured with the

International Personality Item Pool (IPIP) (Goldberg, 1999) and performance on a presentiment task (S. P. Wright, 2003). Openness to experience and its facets, measured with the NEO PI-R, were hypothesised to correlate with performance in a free-response Ganzfeld ESP experiment (Morris, Cunningham, McAlpine, & Taylor, 1993). A significant positive correlation between psi score and openness to actions was found, but there were no significant findings with any of the other facets, nor with the openness domain. Osis and Bockert (1971) found a significant positive correlation between performance on two psi tasks and a scale they devised of self-transcendence and openness. The psi tasks were a motor task in which participants were influenced to select a square on a grid, and a telepathy task with art pictures as targets. Their scale of openness was not the same as openness to experience on the NEO PI-R, it was more oriented to describing meditative states of mind, but it had some conceptual similarity. More recently, on a pilot test of a new psi-testing paradigm, *precognitive déjà vu*, participants high on openness to experience scored particularly high (Bem, 2004). In *precognitive déjà vu*, participants rate the familiarity of faces prior to being subliminally shown the randomly selected target.

Openness to experience might be associated with experiencing and interpreting psi events. Significant positive correlations between openness to experience and an index of psi experiences were found in creative participants (Zingrone, Alvarado, & Dalton, 1999), indicating that people with high openness might experience more psi events. Creative people have tended to achieve high hit rates in free-response ESP tasks (Bem & Honorton, 1994; Dalton, 1997); as creativity is related to openness, this indicates that individuals high on openness might also score high in psi tasks. The characteristic of perceptual defensiveness, the tendency unconsciously to screen out threatening stimuli, correlated negatively with psi task performance (Haraldsson & Houtkooper, 1995). A negative correlation between perceptual defensiveness and openness to experience was found (Watt, 1993), which suggests that individuals with high openness to experience would score well in psi tasks. Overall, considering the evidence from empirical studies and the relationships between openness to

experience and other psi-conducive characteristics, we would expect people high on openness to experience to be more likely to demonstrate psi ability.

The final individual difference that we will consider the effect of is belief in psi. People who believe in psi have been found to score higher in psi experiments than people who do not believe in psi (Lawrence, 1993). Schmeidler named the believers “sheep” and the disbelievers “goats” (Schmeidler & McConnell, 1958, p. 30). The phenomenon of psi believers scoring higher in psi experiments is often called the *sheep-goat effect*. The sheep-goat effect is one of the more robust findings in parapsychology, and once thought promising as a reliable enough effect to refute sceptical counter arguments (Rao, 1974).

The effect of belief in psi was first investigated in forced-choice clairvoyance psi tasks, using the standard Zener cards (Schmeidler, 1945). Participants guessed the symbols on cards that they could not see. Prior to the psi test, all participants were asked how they thought they would score. Based on their answers, participants were classified as sheep, those who expected to succeed, or as goats, those who expected to score at chance. Across five independent experiments, the sheep scored significantly above chance, and the goats scored non-significantly below chance. Schmeidler (1945) noted that the overall effect of these experiments, had the sheep and goats scores been analysed together, would have been at chance. Schmeidler suggested that null results in other psi studies could have been due to conglomerating sheep’s and goats’ scores together. It is therefore important to take the sheep-goat effect into account in psi research.

Since this early research, many further studies have corroborated the sheep-goat effect. An early review of 17 experiments into belief in psi found that 13 of them found the sheep-goat effect, six of which were independently significant (Palmer, 1971). Palmer’s review used the vote-counting technique, which overly relies on statistical significance. It was improved upon by a meta-analysis, which found a

highly significant positive association between belief in psi and performance, across a wide variety of psi tasks (Lawrence, 1993). The effect size estimated is small (mean trial based $r = .03$). In meta-analyses that include only published studies there is a risk of finding a spuriously high effect size estimate, resulting from biases towards publishing studies with significant findings: the *file-drawer effect*. Lawrence's estimate had a low risk of the file-drawer effect, with 24 unreported non-significant studies required, for each of the 73 reported, to nullify the sheep-goat effect. As Palmer (1971) concluded, the sheep-goat effect appears to be a genuine phenomenon.

There are other characteristics which differ between sheep and goats. People who believe in psi report more psi experiences (Haight, 1979). People who reported more psi experiences were also found to score better in a forced-choice psi task (Haight, 1979) and in free-response psi tasks (Honorton, 1997). This suggests that sheep might have more psi experiences. Sheep were found to be more extraverted than goats (Thalbourne & Haraldsson, 1980). Extraversion, as noted above, has been associated with positive psi scoring in experiments. It is not known whether there is a causal link between extraversion and belief in psi both being psi-conducive. It might be instructive, therefore, to investigate extraversion and belief in psi in conjunction. Goats were found to be shyer than sheep (Thalbourne & Haraldsson, 1980). Thalbourne and Haraldsson point out that people who are shy might wish to retain privacy, psychically as well as socially. These differences were, however, minor, and there are no recognised personality traits with which we can predict whether someone is a sheep or a goat.

Schmeidler originally classified participants by whether they believed in the possibility of a successful psi result in their particular experiment (sheep), or whether they rejected this possibility (goats) (Schmeidler & McConnell, 1958). These terms have been used more loosely since then, encompassing belief in psi and disbelief more generally, and are usually assessed by questionnaire (e.g., Bhadra, 1966; Jones

& Feather, 1969; Van de Castle & White, 1955). Palmer (1971) noted that using participants' expectation of success in their particular experiment was more successful at finding the sheep-goat effect than more abstract measures of belief in psi. Conversely, Lawrence (1993) found no significant differences in the sheep-goat effect depending on which scale of belief in psi was used. This indicated that all the scales measure a common element. Given that both psi experiences and expectation of success in experiments predict positive psi scoring, a questionnaire that includes both should be used. Such a questionnaire was produced by the Koestler Parapsychology Unit at the University of Edinburgh (see Appendix 1), and was used throughout the experiments presented in this thesis.

The sheep-goat effect could occur for several reasons. Firstly, sheep have more positive expectations of their performance in experiments, and this might be why they are indeed more successful. On the other hand there is evidence that participants who strongly believe that they can score high actually score lower than sheep who just believe that psi is possible (Palmer, 1972). So the sheep-goat effect does not seem to be caused by positive expectations alone. Secondly, sheep might have better psi ability. This would lead to them performing better in psi tasks and having more psi experiences, which might in turn lead to them believing in psi. A counter-argument to the idea that sheep have better psi ability than goats is the fact that goats sometimes score significantly below chance (Roney-Dougal, 1991). This phenomenon, called *psi-missing*, does not imply that goats have less psi ability, but it does imply that they use their ability to repudiate a psi effect. If this is the case, it follows that goats would score well at psi tasks, if they did not know that they were psi tasks.

One example of this in the previous literature aimed to investigate whether sheep and goats would score in line with their beliefs (Lovitts, 1981). Participants were classified as sheep or goats using the Bhadra (1966) questionnaire. Half the participants were misinformed that they were in a perception without awareness

experiment in which high scores would disprove ESP. The other half were told that they were in a psi experiment in which a high score would prove the existence of ESP. Participants attempted to guess which of five typed symbols, similar to the Zener card symbols (+ = O L X), were projected at sub-threshold speed and luminosity. The actual visual stimuli presented were conglomerates of the five symbols superimposed on one another. Unbeknown to the participants, individual packs of hidden psi targets had been created for each of them, with a target symbol for each trial. Participants scored hits if their guesses corresponded with their target symbols. There was a significant interaction between participants' belief and their perceived purpose of the experiment. The goats scored higher when they thought that this would disprove ESP, and the sheep scored higher when they thought this would prove ESP. This experiment showed that goats have psi ability, and that people score in line with their beliefs. This supports Palmer's (1971; 1972) suggestion that people use psi to vindicate their previously formed beliefs. It follows, therefore, that instead of intrinsic differences in psi ability leading people to believe or disbelieve in psi, that an initial difference in belief drives how people use, and consequently experience, psi. In other words, psi might not be a case of 'you have to see it to believe it', but rather that 'you have to believe it to see it'. There has been one attempt to replicate Lovitts' findings, which failed to find the same effect (Lawrence, 1990-1991). Both the original and the replication study used a similar, but small, sample size so neither finding is more robust based on power. Both studies found a positive association between female gender and psi belief, and Lawrence proposed that a gender imbalance between the groups in his study might partially explain the failure to replicate, but admitted it would not explain it entirely. The question of whether the reversal of the sheep-goat effect is genuine requires further research with gender-balanced samples.

If the sheep-goat effect is the product of intrinsic psi ability, this has consequences for nonintentional psi. In nonintentional psi tests, participants do not know that they are in a psi test, and so if, as Lovitts' (1981) findings suggest, goats have psi ability,

then goats would score well in nonintentional psi tasks. If participants do not know that they are in a psi test, then the argument that sheep score better because of their positive expectations cannot apply. Nonintentional psi experiments are therefore a forum to test out whether there is a difference in sheep's and goats' intrinsic psi ability. Further research into the effect of differences of belief in psi on psi scoring when people are not aware that they are in a psi task is required to understand the aetiology and mechanisms of the sheep-goat phenomenon.

2.9. Prior Research considering Personality and other Individual Differences in Nonintentional Psi

Investigations into how personality characteristics and psi performance are related have focussed predominantly on intentional psi experiments, as reviewed above (Stanford, 1990). There has been very little investigation of the role of personality in nonintentional psi. This is unfortunate, because nonintentional psi experiments are more similar to spontaneous psi experiences, and are therefore more generalisable to real-life situations. There are also confounding factors in intentional psi experiments that nonintentional psi paradigms avoid. For example, participants in intentional psi experiments usually aim for declarative knowledge of the psi information. It is therefore ambiguous whether personality traits associated with good psi performance are indicative of greater psi receptivity, or of greater ability to bring psi information to consciousness. Research into personality and nonintentional psi would address this issue. In intentional psi tasks participants know that they are in a psi experiment, so their performance might be affected by their beliefs about, and attitude toward, psi. Belief and attitude should not interfere with nonintentional psi, and so we would not expect the sheep-goat effect to emerge in nonintentional psi. Unless, that is, the sheep-goat effect is underpinned by a difference in the psi ability of the sheep and the goats. Investigating belief in psi and nonintentional psi would disentangle the role of belief and psi ability, and shed light on the origin of the sheep-goat effect. More research into personality and belief in psi in nonintentional psi experiments would

therefore extend the understanding of the role of personality in parapsychology. The previous research investigating neuroticism, openness to experience, and belief in psi in nonintentional psi experiments is reviewed below.

Watt and Ravenscroft (2000) investigated nonintentional psi and neuroticism in a PMIR experiment. Neuroticism was measured on the 60-item NEO FFI, a shorter version of the NEO PI-R. Participants completed an affective judgement task, in which they rated how pleasant they found Japanese kanji script characters. Hits were scored if participants' ratings concurred with hidden psi targets. This led to another person, paired with the participant, receiving a reward or punishment task. A negative correlation between psi hitting and neuroticism was predicted. A weak negative correlation was found that was just short of significance. The experiment did not find an overall effect from nonintentional psi, but Watt and Ravenscroft noted that it had low power. This investigation of nonintentional psi and neuroticism was inconclusive.

Openness to experience has been investigated in two PMIR-style nonintentional psi experiments (Watt & Ravenscroft, 2000; Luke, Roe et al., 2008). Watt and Ravenscroft (2000) measured openness on the NEO FFI. The task was the affective judgement task described above. Openness was hypothesised to correlate positively with hit rate. No relationship between hit rate and openness to experience was found. Luke, Roe et al. (2008) used the openness to experience scale from the International Personality Item Pool (IPIP) (Goldberg, 1999). The nonintentional psi task was to make affective judgements: Participants rated how much they liked a series of fractal images. If their ratings concurred with their precognitive psi targets hits were scored. Hit rates higher than MCE entered participants into a reward condition; lower hit rates entered them into a punishment condition. The hypothesis was again that openness would correlate with psi performance. Openness correlated positively with hit rate, with a large effect size ($r = .46$). So far there have been two very different findings with openness to experience and psi in nonintentional psi tasks. Could the

difference in findings be due to the use of different personality scales? The IPIP and the NEO items are equivalents, and the correlations for the sub-facets of openness to experience are between .70 and .80 (Goldberg, 1999), so the difference in findings should not be due to the researchers using different scales. There was also a difference in the populations used: Watt and Ravenscroft (2000) used students, and Luke, Roe et al. (2008) used visitors to a museum. One other difference between these studies is in the nature of the reward and punishment task used to give incentive to the PMIR. In Watt and Ravenscroft's (2000) study another person, not the participant, received the reward or punishment. In Luke, Roe et al. (2008) the participant who did the nonintentional psi task was the one who received the reward or punishment accordingly. In both experiments the participant who did the nonintentional psi task's openness was measured. Perhaps Watt and Ravenscroft (2000) should have considered the openness of the other participant as well. The other main difference between the studies is that Luke, Roe et al. (2008) found an overall positive hit rate but Watt and Ravenscroft (2000) did not. An overall null effect does not rule out the possibility of relationships with personality variables, but if there was an unknown aspect of Watt and Ravenscroft's (2000) experience that had suppressed any potential psi effect, then it might have suppressed a relationship between psi and personality. In summary, the relationship between openness to experience and nonintentional psi is not yet clear.

The relationship between belief in psi and nonintentional psi scoring was first investigated in forced-choice tasks (Kennedy & Haight, 1978). Belief in psi was measured by asking participants whether they thought that ESP was possible in this experiment. Participants answered *yes* (sheep), *no* (goats), or *undecided* (excluded from analysis). The hypothesis was that sheep would have a higher psi score than goats. The nonintentional psi task was the Mood Adjective Check List. Participants ticked the mood adjectives that applied to their current mood. The psi targets were individually randomised hidden clairvoyance targets. Hit rates were determined by the proportion of mood adjectives a participant ticked that had been designated their

psi targets. The overall hit rate was non-significantly different from MCE. This might have been because there was no attempt to influence the participants' mood, and their mood would have strongly influenced their answers. The authors also did not report whether conflicting moods, such as happy and sad, might have been selected for the same participant, which could have reduced the likelihood of a hit. Nonetheless, the sheep scored significantly higher than the goats, supporting the hypothesis. A more recent investigation into forced-choice nonintentional psi tasks and belief used both a belief in psi scale and a belief in the paranormal scale (Luke, Delanoy et al., 2008). The belief in psi scale was a five-item sheep-goat scale, based on Palmer (1971). The belief in paranormal scale was a 12-item subset of an anomalous experience inventory that asked whether participants believed, for example, that mind can control matter. The nonintentional psi task was the affective judgement task described above. The hypothesis stated that both belief scales would correlate positively with psi performance. This hypothesis was supported by significant positive correlations between psi scoring and both belief scales. In summary, in the limited research that has been carried out into the role of belief in psi and nonintentional psi performance, a positive association between belief and psi performance has been found. This is very important in understanding the sheep-goat effect. Differences in scoring between believers and disbelievers in nonintentional psi tasks cannot be due to cognitive effects of belief. A sheep-goat effect in nonintentional psi experiments, indicated by the findings so far, therefore points to the difference in scoring between sheep and goats being due to intrinsic differences in their psi ability.

In conclusion, very little research has been conducted into the role of personality characteristics in nonintentional psi. As there is considerable evidence, as reviewed above, for extraversion, neuroticism, openness to experience, and belief in psi playing a role in intentional psi experiments, but little understanding of how these interactions might function, research into personality and nonintentional psi is vital. Prior research has focussed, in the main, on univariate relationships between psi and

individual differences (Palmer, 1977). As personality variables can interact with each other, and with the testing situation, multivariate approaches are recommended. To date, investigations into nonintentional psi and personality appear not to have considered the role of extraversion. Extraversion scores were almost certain to have been collected by Watt and Ravenscroft (2000), because the NEO FFI should only be administered in its entirety. It is unfortunate that they did not investigate the relationship between extraversion scores and psi hit rate in their experiment. As extraversion is associated with higher psi performance in intentional psi experiments (as reviewed above), it is worth investigating whether extraversion plays a role in nonintentional psi. In the next two chapters, we present two experiments that investigate the role that personality factors, including extraversion, might play in nonintentional psi. These two studies use different vehicles for investigating nonintentional psi, and we therefore motivate their precise methodologies in more detail in the following chapters.

3. Experiment 1: The Effect of Hidden Targets on a Line-Length Judgement Task

3.1. Overview

This chapter presents an empirical study, Experiment 1, which investigated nonintentional psi with a novel task, and what role personality might play in this phenomenon. Participants made judgements about the relative length of visually presented lines, when there were actually no differences in the length of the lines. Unbeknown to the participants there were hidden targets with the “answers” to the line-length judging task. The associations between participants’ hit rates on the nonintentional task and their extraversion, belief in psi, openness to experience, neuroticism, and state anxiety were investigated.

3.2. Introduction

Nonintentional psi experiments ask whether participants, without conscious effort, respond to information that can only be gained through psi. As reviewed in Chapter 2, hidden targets (i.e., those that participants do not know exist) can influence memory of a story (Stanford, 1970) as well as performance in exams with both multiple choice (Schechter, 1977) and essay-style questions (W. G. Braud, 1975; Johnson, 1973). In Experiment 1, below, hidden targets were used in a similar way to those in the exam studies, as written lists of targets in a format that parallels the answers participants will give. Hidden targets and hidden senders (i.e., senders who participants do not know exist) can also influence participants’ performance on standard psychological tests, to varying extents (Kreitler & Kreitler, 1972, 1973). Experiment 1 used a standard psychological test, namely perceptual judgements, as the vehicle to assess nonintentional psi. One advantage of using a standard psychological test was that it provided a plausible reason for participants to be in the

experiment, because they were required not to know that it was a parapsychological study.

In Experiment 1 participants were presented with pairs of lines for a very short duration, aiming for perception on the threshold of conscious awareness. Participants were led to believe that there were differences in the length of the lines, although there actually were not. Their task was to select whether the line on the left or the right was the longest. Following the nonintentional psi hypothesis (that information that can only be accessed through psi can, without intention or awareness, guide behaviour in the same way it would if it were conscious knowledge) it was predicted that participants would choose answers to the line-length judgement task in accordance with a set of hidden targets more often than chance.

The choice of task was motivated by prior empirical literature. Versions of the line-length judgement task have been used in both psychology and parapsychology. In the former, it has been shown that one person's judgement about line-lengths can be influenced by the consensus of a majority (Asch, 1951, 1955; R. Bond & Smith, 1996). This is a strong influence, which can influence people to say that a clearly shorter line is the longest. In parapsychology, line-length judgements can also be influenced by telepathy, under certain circumstances (Kreitler & Kreitler, 1973). In Kreitler and Kreitler's (1973) experiment, participants saw clearly visible lines that were the same length, and still selected one as the "longest". Therefore line-length judgements are susceptible to psychological and parapsychological influences. The current experiment used the line-length task to investigate whether these judgements can be influenced by hidden targets.

In addition to hypothesizing that the overall psi effect would be positive, we also hypothesized a response bias effect. The response bias effect is that there is a proportionally higher psi hit rate on the responses that are not favoured by any other (non-psi) response bias (Stanford, 1967). In the line-length judgement task in the

experiment presented here, participants were expected to have a response bias, and answer either *left* or *right* more frequently when choosing which line is longest. Participants' most frequent answer, their *preferred side*, is the one favoured by their response bias. So proportionally more psi hits are predicted on the *non-preferred side*. For example, a participant with a bias to respond *left* would have a higher proportion of psi hits in the trials when he responds *right*. This response bias effect has been found in previous experiments assessing nonintentional psi with recall of a story (Stanford, 1970), word association (Wilson, 2002a, 2004), recognition of subliminally presented letters, and autokinetic motion (Kreitler & Kreitler, 1972), as reviewed in Chapter 2. In these previous experiments, with the exception of autokinetic motion test (Kreitler & Kreitler, 1972), the response biases were taken from the combined responses of the participants at group level, and were not individual to each participant, and so may not have applied to all the participants equally. In Experiment 1 the response bias was calculated individually for each participant, extending the previous findings by considering the response bias with a new task, and at individual participant level. The participants' less frequently given responses are expected to show a higher psi hit rate.

As reviewed in Chapter 2, there has been minimal research into the role of personality characteristics in nonintentional psi experiments. In previous research in intentional psi experiments, extraversion appeared to correlate positively with psi performance (Eysenck, 1967b; Palmer, 1977; Sargent, 1981; Honorton et al., 1998). Additionally, in previous research with intentional and nonintentional psi tasks openness to experience correlated positively with psi scoring (Luke, Roe et al., 2008; Osis & Bokert, 1971; Wright, 2003 #95), but not in Watt and Ravenscroft (2000). Belief in psi has also been found to correlate positively with psi scoring in intentional and nonintentional psi experiments (Kennedy & Haight, 1978; Luke, Delanoy et al., 2008). Neuroticism has usually correlated negatively with psi scoring in intentional tasks (W. G. Braud, 1981; Honorton, 1965; Kanthamani & Rao, 1973; Palmer, 1977). State anxiety has not previously been considered in nonintentional psi tasks,

but was predicted to be associated with lower hit rates due to previous negative associations between state anxiety and psi performance in PK and ESP tasks (Broughton & Perlstrom, 1986, 1992; Roe et al., 2003). The current experiment extends this work by considering a broader range of traits within a single nonintentional psi study. Following from the previous findings in both intentional and nonintentional psi research, extraversion, belief in psi, and openness to experience are likely to be positively associated with performance on the nonintentional psi task. Neuroticism and state anxiety are likely to be negatively associated with performance on the nonintentional psi task.

In Experiment 1, participants judged which of each pair of lines was the longest, although they were actually the same length. Unbeknown to the participants, there were hidden targets with the answers. We first assessed whether participants' line length judgements concurred with the hidden targets more often than chance would dictate. Second, we assessed whether there was a response bias effect by assessing whether participants showed a proportionally higher hit rate on their non-preferred side. Finally, we assessed associations between participants' extraversion, belief in psi, openness to experience, neuroticism, and state anxiety on their hit rate on the nonintentional task.

3.2.1. Hypotheses.

1 When required to choose which line in each pair of identical lines is the longest (the one on the left or the right), participants will choose in accordance with their hidden psi targets (score a hit) more often than MCE.

2 Participants will score a higher proportion of hits on the line-length judgement task when they respond with their less frequently chosen answer (non-preferred side) compared to their more frequently chosen answer (preferred side).

3 Participants' hit rates in the line-length judgement task will be positively correlated with their extraversion, belief in psi, and openness to experience scores, and negatively correlated with their neuroticism and state anxiety scores.

3.3. Method

3.3.1. Design.

This experiment used a one group design. Participants' responses were compared to MCE of 50%.

3.3.2. Participants.

Fifty participants volunteered, ranging from 17 to 45 years of age ($M = 23.33$, $SD = 5.11$ years), 28 of whom were female and 22 male. They were ostensibly recruited for an experiment in perceptual judgement.

3.3.3. Ethical Considerations.

We identified one potential ethical issue in this experiment. This was the intentional deception of participants regarding the overall aim of the experiment and the exact function of the experimental task. It was integral to the experimental design that participants would be deceived without their consent. We anticipated that this deception would not induce psychological harm or stress. We inferred that giving participants the right to terminate the experiment at any time and a full debrief following the experiment would adequately address this issue. The Psychology Research Ethics Committee at the University of Edinburgh granted ethical approval for this experiment to proceed.

3.3.4. Materials and Apparatus.

Personality questionnaires.

The 240-item NEO PI-R (Costa & McCrae, 1992a) was used to score extraversion, neuroticism, and openness to experience. The NEO PI-R measures neuroticism, extraversion, openness to experience, agreeableness, conscientiousness, and the six facets (sub-sections) of each of these domains. It includes negatively phrased items; these are important to counteract the common tendency to answer yes more than no, which could lead to spurious findings if all the items were positively phrased. Participants circle their answer on five-point Likert scales anchored at 0 (*strongly agree*) and 4 (*strongly disagree*) in response to each item. An example item for extraversion is “I’m known as a warm and friendly person”. An example of a negatively phrased item for neuroticism is “I rarely feel fearful or anxious”. The items are scored from 0 to 4, leading to a possible maximum score for any domain of 192. High scores indicate high levels of that trait, e.g., high extraversion.

To measure state anxiety, participants completed the state anxiety section of the State-Trait Anxiety Inventory (STAI) (Spielberger, 1983). This is a well-validated and reliable measure of both state anxiety and trait anxiety; the internal consistency of the scales is .9, which is very high (Kline, 1998). The test-retest reliability for the state anxiety scale is low, but as state anxiety varies with the presence and absence of stressful influences it should vary from one test to another. Kline (1998) concluded that it is a very good quick test of anxiety. The state anxiety scale is a 20-item list of short statements that includes negatively phrased items, such as “I feel pleasant”. Participants fill in a circle on four-point Likert scales anchored at 1 (*not at all*) and 4 (*very much so*) in response to each item. The items are scored from 1 to 4, with a minimum total score of 20 and a maximum of 80. High scores indicate high state anxiety.

Belief in psi was measured using the Koestler Parapsychology Unit belief in psi 12-item questionnaire (Appendix 1). This comprises questions about participants' belief in and experience of telepathy, clairvoyance, precognition, and psychokinesis; for example "Have you ever had an experience which is best explained by telepathy?" It includes two questions which ask whether participants think they have psi ability, in general "What best describes your own psi ability?", and in experiments "Do you believe that you might be able to demonstrate any psi ability in a controlled laboratory experiment?" The seven-point response scales vary in wording due to the phrasing of the questions, but most are anchored at 1 (*yes*) and 7 (*no*). This questionnaire has a minimum score of 12 and a maximum of 84; high scores indicate a high belief in psi. This is neither a validated questionnaire, nor does it contain any negatively phrased items. It has been used in previous research (e.g., Wilson, 2002a).

Line-length judgement materials.

The stimuli comprised 50 pairs of identical lines. The lines were always the same length: 250 pixels high; the screen was set to a resolution of 1024 x 768 pixels. The lines were white on a black screen, and the experiment program was run in Shockwave on a Sony VAIO Laptop. There were stickers on the two keys that the participants would press to respond in the line-length judgement task: red for the left on the A key, blue for the right on the L key.

Hidden targets.

The hidden targets were the "answers" to the line-length discrimination task, and these answers were displayed in several different modalities. There is conflicting evidence regarding what information is received in psi experiments; it could be a visual image without the meaning, as was found in remote viewing experiments (Sinclair, 1962). Alternatively, there are accounts of the meaning being transmitted, without accurate imagery, such as hunches or symbolic dreams (Broughton, 1991). To maximise the chances of the psi target conveying information that would lead the

participants to achieve hits, information was represented in the following modalities: the meaning, a visual image, and the key that participants would press to score a hit. Each target therefore included: the word left or right; a picture of the lines, with the “longer” one represented by a line and the “shorter” one by a dot (e.g., | . or . |); the colour of the sticker on the key that they should press (red or blue); and the letter of that key (A or L). Thus, the left target was: left | . red A, and the right was: right . | blue L. Although the lines differed in length in the hidden psi targets, they did not differ in length in the visual stimuli presented to the participants.

Since each participant would do 50 trials (see Procedure below), 50 hidden target answers were created for each participant. These answers were printed on sheets of A4 paper, with each trial’s number (1 – 50) listed next to each trial’s target answer, e.g., 1. left | . red A. There was one vertical column of trial numbers, and one vertical column of target answers, aligned so that the answer to the first trial was at the top, and the last trial at the bottom. Nothing else was written on the sheet. Each participant had an individually randomised list of target answers to overcome the risk of the stacking effect (an unwanted artefact, see Chapter 2).

3.3.5. Procedure.

Hidden target selection and security.

Prior to the start of the experiment, a member of staff at the University of Edinburgh, otherwise unconnected with the experiment, created the randomised lists of hidden target answers using a random function in Visual Basic. He created 50 unique lists, each with 50 target answers, balanced so that 25 targets were *right* and 25 were *left* in every list. The lists were printed and placed into opaque envelopes by a colleague, who was also otherwise unconnected with the experiment. She sealed the envelopes and wrote a participant number (from 1 – 50) on the outside. The first participant’s hidden targets were in envelope 1, and so on. The target envelopes were sealed with standard envelope gum, but no further measures were taken to prevent them being

opened or tampered with. The participants did not know about the target envelopes, see them, or handle them, and so extra measures against fraud were not deemed necessary. The experimenter did not see any targets until after all the experimentation was concluded. At this point the envelopes were checked and none of them showed any evidence of having being tampered with. The target lists were kept in a locked drawer in the experimenter's office until needed. Prior to each participant's arrival, the experimenter took the envelope corresponding to the participant's number, and concealed it under a PC on the testing desk, next to the laptop that participants used in the experiment. The envelope was out of sight.

Session procedure.

Participants were tested individually. They were greeted at the door to the psychology building, and shown to a waiting room in which they completed the NEO PI-R, which took between 20 and 35 minutes. Participants also signed a standard consent form agreeing to participate, and on the understanding that they could terminate the experiment at any time (no participants chose to). The experimenter then showed participants into the experimental room. One office in the Department of Psychology at the University of Edinburgh was used for testing. To maintain a constant level of luminosity a blackout blind was drawn over the window and the room was lit by artificial lights. Participants sat around 60 cm from the screen of the testing computer, a Sony VIAO Laptop computer, which was on a desk. This laptop had a screen size of 23 cm high by 30.5 cm wide; the screen resolution was set to 1024 x 768 pixels. There was also a PC on the desk, under which the hidden targets had been concealed. The desktop computer was switched off, see Figure 2.

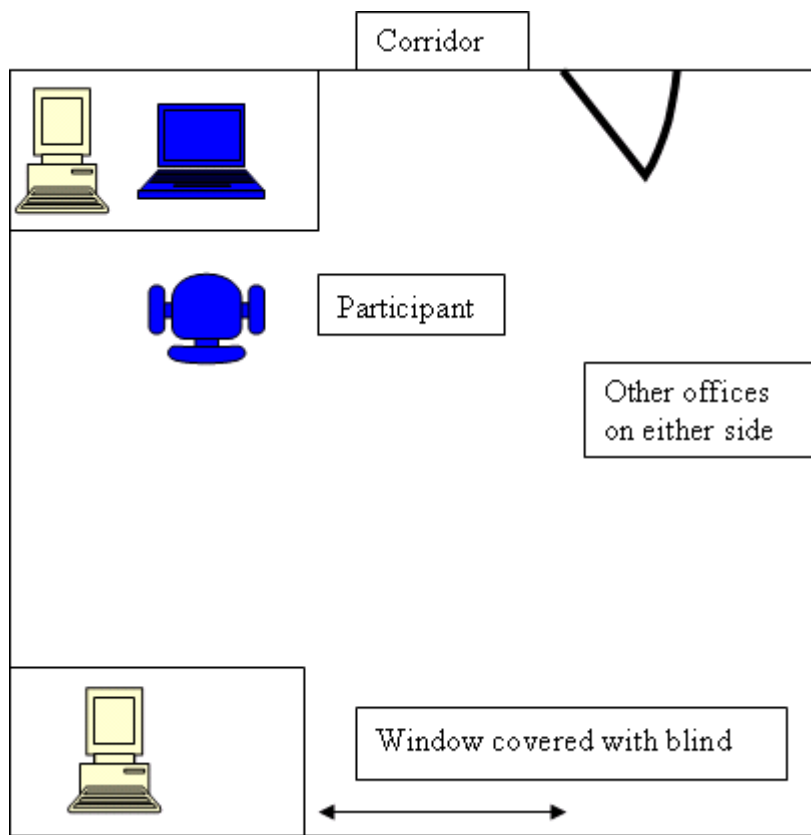


Figure 2: The Experimental Room for Experiment 1 (not to scale)

The experimenter gave verbal instructions for the line-length judgement task, and participants completed one practice trial, in which the lines were clearly a different length. Participants then had the opportunity to ask questions before moving onto the line-length judgement task. (Before beginning this task, participants briefly completed another task, not reported here. See Chapter 7). The experimenter left the room before the lines task began; participants did the line-length judgement task alone and unobserved. Each trial started with a one-second presentation of a fixation spot in the centre of the screen, in white on a black background. This was followed by a 40-millisecond presentation of the two vertical lines on the screen. (The

duration of 40 milliseconds was chosen after pilot testing. It was the duration at which most participants reported that they only indistinctly saw the lines.) The two vertical lines were immediately followed by a visual mask of two identical vertical columns of 'x' s where the lines had been. The mask aimed to prevent after-images and preserve the short duration of the presentation of the lines. Participants entered their choice for the longest line as the one on either the left or the right, by pressing the 'A' key on the laptop for left or the 'L' key for right. The computer program would not accept any answer other than 'A' or 'L'. Once the participant had responded, the next trial began automatically, immediately presenting the fixation spot. When the trials were over, participants fetched the experimenter from the waiting room. The experimenter then asked a manipulation check question to establish how participants had experienced the task "Did you see the lines clearly?" (She also asked other questions related to the task reported in Chapter 7). Participants then completed the STAI. The experimenter then showed them out of the building. It was only after the results were analysed that the participants were debriefed by email.

3.3.6. Analyses.

Analyses were conducted in SPSS version 14 (SPSS Inc, 2005) and Microsoft Excel (Microsoft Corporation, 2003).

3.4. Results

Manipulation check.

All participants were able to perform this task, regardless of whether they reported conscious awareness of seeing the lines. Therefore, these stimuli were fit for the purposes of this task. Seventeen participants reported not seeing the lines, and a further eight were unsure whether they had seen the lines. However, none of the participants challenged the task, and none declined to respond left or right.

Hypotheses 1 and 2: When required to choose which of each pair of identical lines is the longest (the one on the left or the right), participants will choose in accordance with their hidden psi targets (score a hit) more often than MCE. Participants will score a higher proportion of hits on the line-length judgement task when they answer with their less frequently chosen answer (non-preferred side) compared to their more frequently chosen answer (preferred side).

Data was coded such that participants scored a hit when they pressed the button for the left or right line being longest corresponding to their hidden target for that trial. Three participants' data were lost due to a programming error. The data that were included in the analysis are presented in Table 1.

Table 1: Means, standard deviations and range of hits on the line-length judgement task, overall and split by side (preferred/non-preferred).

	mean	SD	min	max
Total hits as %	49.96	5.86	38	60
Preferred side hits as %	49.76	5.13	39	59
Non-preferred side hits as %	51.37	8.14	36	69

Note. $N = 47$.

Each participant's hit rate was calculated as the number of hits/total number of trials as a percentage. The mean percentage of hits on the lines task was 49.96. This was found to be non-significantly different from MCE of 50 by one-sample t test, $t(46) = -.05$, $p = .96$, effect size $f = -0.01$. This effect size is the equivalent of d (J. Cohen, 1977); this is an extremely small effect size. Hypothesis 1 was not supported. There was no overall evidence of psi in this experiment.

Each participant's hit rate for his or her preferred side was calculated as the number of hits on the preferred side/the number of trials in which the preferred side was the answer as a percentage, and likewise for the non-preferred side. The percentage of

hits on the preferred and non-preferred sides were significantly different from each other by paired-samples t test, $t(46) = -2.11$, $p = .04$, Cohen's $d = 0.24$, a small effect size. Hypothesis 2 was supported; participants got more hits on their less frequently chosen side.

Hypothesis 3: Participants' hit rates in the line-length judgement task will be positively correlated with their extraversion, belief in psi, and openness to experience scores, and negatively correlated with their neuroticism and state anxiety scores.

Table 2 shows the outcomes of Pearson's correlations between overall hit rate on the psi task and neuroticism, extraversion, openness to experience, belief in psi, and state anxiety. The alpha level was set to $p = .01$ to correct for there being five analyses. As the directions of effects were stated in the hypothesis all ps are one-tailed. A sub-set of personality questionnaires ($N = 29$ for the NEO PI-R, $N = 23$ for belief in psi, $N = 22$ state anxiety) were lost prior to coding due to a break-in, and after it was no longer practical to ask participants to repeat them. The remaining data are shown below.

Table 2: Personality and individual differences correlation coefficients with overall hit rate

Variables Correlated	N	Pearson's r	p
Neuroticism	21	.22	.19
Extraversion	21	.48**	.01
Openness to Experience	21	.34	.06
Belief in Psi	27	.13	.25
State Anxiety	28	.04	.43

Note. * $p < .05$. ** $p < .01$.

There was a significant correlation between the total number of hits on the lines task and extraversion. There was no significant correlation found with neuroticism, state anxiety, belief in psi, nor openness to experience.

To assess the personality variables in conjunction with each other, the data were re-analysed with a regression. Extraversion, openness to experience, state anxiety, neuroticism, and belief in psi were entered into a backwards stepwise linear regression model with entry point determined as $p = .05$ and exit point determined as $p = .10$. Extraversion, openness to experience, and state anxiety were retained in the model. These variables were put into a regression model using the *enter* command. The model constant and outputs are displayed in Table 3.

Table 3: Summary of stepwise regression analysis for personality variables and psi task hit rate

Variable	<i>B</i>	<i>SE B</i>	β	<i>p</i>
Constant	4.21	13.27		
Extraversion	0.16	0.55	.54**	.01
Openness to Experience	0.14	0.72	.35	.07
State Anxiety	0.22	0.12	.35	.08

Note. * $p < .05$. ** $p < .01$.

The standardised beta value β is the number of standard deviations that the outcome (overall number of hits) will change as a result of a 1 standard deviation change in the personality variable, so it is an indication of the size of the relationship between the personality variable and hit rate. There was a significant relationship between hit rate and extraversion. Openness and state anxiety did not have a significant relationship, but contributed to the model which explains 44% of the variance in the hit rates between participants ($R^2 = .44$). This provides further support that the relationship between extraversion and hit rate in Table 2 is genuine. Therefore, Hypothesis 3 was supported for extraversion, but not for any of the other personality variables.

3.5. Discussion

This study aimed to investigate whether participants' behaviour could be altered by psi targets that they did not know existed, and whether this behavioural effect was

influenced by personality. Participants judged which of two lines was longest in a task where the lines were in fact the same length, and their responses were compared to a set of pre-determined hidden target answers. There were three hypotheses, and these are considered in turn below.

The first hypothesis was that there would be an overall positive psi hit rate, i.e., that participants' responses would match the hidden target answers more often than would be expected by chance. This hypothesis was not supported. This may be because there was in fact no psi influence in this experiment; however, it is important to consider possible reasons why this experiment might have failed to find a psi effect, if one existed. Although versions of the line-judgement task have been used previously, they have shown effects in somewhat different circumstances to those used here. Asch (1952) found that participants would give false line-length judgements to conform with a unanimous group. Asch also displayed the lines for a long duration, so they were clearly seen. In the current study, in contrast, the lines were displayed for a very short period of time (around the threshold of awareness) and the influence came from a psi source, which we expected to exert a weaker influence than the social influence of pressure to conform. So, although the line-length judgement task was inspired by research into conformity, the adaptations to make it suitable for a nonintentional psi task rendered it quite different in nature from these social conformity experiments.

Nonetheless, a line-length judgement task has previously been used in a nonintentional psi task (Kreitler and Kreitler, 1973). However, there were two key differences between Experiment 1 and Kreitler and Kreitler's study. Firstly, there was no sender in the current study. Kreitler and Kreitler found that only when a sender was actively trying to transmit information was the receiver's choice affected. Possibly there was no evidence of psi in Experiment 1 because there was no sender. However, this conclusion would be at odds with other experimental findings that show there is no difference between sender and no-sender designs in, for example,

free-response GESP experiments (Morris, Dalton, Delanoy, & Watt, 1995; Palmer, 1978). Therefore, the absence of a sender alone cannot explain the findings.

A second difference between this lines task and the one used by Kreidler and Kreidler (1973) was that in their experiment the stimuli were clearly presented lines of the same length, or circles the same size, with distorting illusions subliminally superimposed in two-thirds of the trials. A significant deviation from chance was found only in the trials in which the psi information contradicted the subliminal information. For example, the subliminal illusion made the right line look longer, but the psi-target information was that the left line was longer. No psi effect was found when the psi and subliminal information were in agreement, or when there was no subliminal illusion presented. In Experiment 1 the lines were presented at what was aimed to be the threshold of conscious awareness. One limitation was that it was not possible to adjust the presentation duration individually for each participant, and so some did not see the lines, and some reported seeing them clearly. For the 17 of the 50 participants who reported that they did not see the lines, the presentation was below the subjective level of conscious awareness. For the others it was still arguably a weak stimulus. The line stimuli presented were always the same length as each other, and so conflicted with the psi information to an extent, but not as strongly as the stimuli in Kreidler and Kreidler's (1973) experiment, in which the subliminal information and psi information gave opposite answers. There was no overall deviation from chance, however, and so this does not corroborate Kreidler and Kreidler's (1973) finding that a psi effect is more likely to occur when its information contradicts another weak stimulus. Admittedly the stimuli in Experiment 1 were quite different, and a more exact replication of the Kreidler and Kreidler's (1973) experiment might duplicate their findings.

An additional suggestion as to why an overall psi effect was not found relates to the participants' motivation in the lines task. Stanford (1974, 1990) and Broughton (2006 a,b) have argued that participants need an incentive to manifest a psi effect, even in

situations where psi would be acting nonintentionally and without awareness. The experiments on nonintentional psi in exams used the students' intrinsic motivation as the incentive, and these found evidence that supported a nonintentional psi effect (W. G. Braud, 1975; Johnson, 1973; Schechter, 1977). Another way to create an incentive is to provide a reward if the participants score in line with their hidden targets, or a punishment if they do not (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008; Stanford & Associates, 1976; Stanford & Stio, 1976; Stanford & Thompson, 1974; Watt & Nagtegaal, 2000). These studies have also predominantly found significant effects. The participants in Experiment 1 were unlikely to be as strongly motivated to perform well on the lines task as students sitting an exam, although a need for the psi-target information was created by asking them to pick the longest line for each trial. Participants who did not see the lines might also have experienced reduced motivation because they found the experiment frustrating and thought that they could not do it. They also were not provided with any reward for psi performance, and therefore, if an incentive is important in psi tests, this might have been a reason for the overall null effect.

The lack of feedback might also have had an effect on participants' performance in the line-length judgement task. Trial-by-trial feedback has been found to be psi-conducive (Honorton & Ferrari, 1989). This could be because it acts as a reward for the participant, as they receive a sense of achievement, although this might depend upon how well they do at the task. The importance of feedback is also implicated by the observational theories of psi, which state that the source of psi has to observe the outcome in order for a psi effect to occur (Broughton, 1988). If the participant is the source of psi and the observational theories are correct, it might be vital for participants to receive feedback. The participants in Experiment 1 did not receive trial-by-trial feedback, and their individual performance on the lines task was not disclosed to them in the debriefing. This might have resulted in a reduced psi effect.

In summary, regarding Hypothesis 1, there were issues in this experiment concerning aspects of the task, feedback, and participant motivation that might have reduced the possibility of finding a psi effect. In future experiments manipulations to increase motivation, checks that this has worked, and giving feedback to the participants could be used to increase the chances of a psi effect.

The second hypothesis in this study was that there would be a higher proportion of hits on the participants' less preferred responses, in accordance with the response bias effect. This hypothesis was supported. This replicates previous response bias effect findings, all of which found a greater psi effect in the response for which there was no non-psi bias (Kreitler & Kreitler, 1972; Stanford, 1967, 1970, 1973; Wilson, 2002a, 2002b). The analysis in Experiment 1 used each participant's individual preference to answer left or right. This differs from most previous research, in which the response biases were calculated for the whole experimental sample (Wilson, 2002a; Kreitler & Kreitler, 1972; Stanford, 1967) or from published norms (Stanford, 1973). In these experiments for which the response bias was calculated at a group level there is a risk that some of the participants would have had different response biases, affecting the accuracy of the calculations to an unknown level. Experiment 1 therefore adds support to the response bias effect, and has extended the previous findings by investigating this effect with a new task.

The response bias effect has now been demonstrated in word association tasks (Stanford, 1973; Wilson, 2002a), memory tasks (Stanford, 1970), intentional clairvoyance tasks (Stanford, 1967), letter recognition, autokinetic motion and responses to TAT cards (Kreitler & Kreitler, 1972). In addition, a similar effect has been found with a selected psychic participant (Morris, 1971) indicating that it is a robust effect. The only previous experiment to have considered participants' response biases at an individual level was the autokinetic motion study (Kreitler & Kreitler, 1972). A similar pattern was found in both Experiment 1 and in the autokinetic motion studies in that there was a null overall effect, and a significant

response bias effect. This indicates that it is worth looking for internal effects for psi even in studies with null overall effects. The response bias effect, if it is a true psi effect, indicates that psi acts upon the behaviours that are otherwise less likely to be carried out. This could be a function of the behaviour being rarer, as there is a greater capacity for a rare event's frequency to be increased (Kreitler & Kreitler, 1972). It could also be because an unusual psi-mediated impulse is less likely to be ignored (Kreitler & Kreitler, 1972; Stanford, 1967). Morris (1971) argued that psi information cuts through the usual behaviour pattern. Whatever the reason, there is an implication that psi might influence behaviour in the absence of other information. As other sensory information is clearer, and more verifiable, it might only be when sensory information is lacking that one would need to use psi. It might also be the case that any other stronger sensory signals override psi information. Further investigations of the response bias effect might lead to a greater understanding of how psi functions.

The third hypothesis was that there would be an association between participants' hit rates and their personality and individual differences scores. These relationships were investigated to increase the understanding of how personality might relate to performance in nonintentional psi tasks. A secondary aim of considering these factors was to ensure that any patterns in participants' performance that were related to personality were not masked by an overall null effect. For instance, an overall null effect could result from high performance from extroverts and low performance from introverts. Extraversion, openness to experience, and belief in psi were hypothesised to be positively correlated with psi scoring. Neuroticism and state anxiety were hypothesised to be negatively correlated with psi scoring. The outcomes with each personality variable will be considered in turn.

Extraversion scores correlated significantly with psi scoring on the nonintentional psi task, with a large effect size ($r = .48$). This was in the expected direction: Higher extraversion was correlated with higher psi scores. This is the first exploration of the

role of extraversion in nonintentional psi. Extraversion has previously been found to be associated with higher psi scoring in intentional psi tasks (Eysenck, 1967b; Honorton et al., 1998; Palmer, 1977; Sargent, 1981). The effect size estimated for individually tested participants in forced-choice intentional psi experiments was $r = .15$ (Honorton et al., 1998). The effect size in Experiment 1 was larger, although this should be interpreted with caution given the small number of participants. Honorton et al. concluded that the relationship between extraversion and psi scoring in forced-choice psi tasks was artefactual, the result of participants rating their extraversion higher if they received high psi scores. In Experiment 1 the participants completed the extraversion measure before the psi task was explained to them, and did not know that there was a psi task, so there was no risk of that artefact. The positive correlation between extraversion and psi in Experiment 1 therefore supported the extraversion and psi association in general, and extended the findings to a nonintentional task. Although no strong conclusions can be drawn from just one experimental finding, the correlation between extraversion and psi in this nonintentional psi task suggests that the relationship between extraversion and psi is not due to any confounding factors present in intentional psi tasks, such as the attempt to become consciously aware of psi information, but that extraversion is related to psi ability per se.

Openness to experience correlated non-significantly with hit rates. This is a failure to replicate the positive correlation between psi and openness in a nonintentional task (Luke, Roe et al., 2008). It concurs with the null finding of Watt and Ravenscroft (2000). Although non-significant, the correlation between hit rate and openness in Experiment 1 had a medium to large effect size ($r = .34$), and so with a larger sample size might have reached significance. The effect size in Luke et al.'s (2008) experiment ($r = .46$) was of a similar magnitude. Further replication is required to draw any conclusions about the role of openness to experience in psi, in both nonintentional and intentional tasks.

There was no significant relationship between psi score and belief in psi in Experiment 1, a failure to replicate the positive correlations between hit rate and belief in psi in two previous nonintentional psi experiments (Kennedy & Haight, 1978; Luke, Delanoy et al., 2008). This difference in findings might be due to random variation, or methodological or sampling differences between the experiments. The importance of belief in psi in nonintentional tasks is the insight it could give into the origins of the sheep-goat effect. Whereas the two previous findings indicated that there is an effect of belief in nonintentional psi tasks, and therefore that psi ability might lead to the difference in belief, Experiment 1 does not.

There was no significant correlation found between hit rate and state anxiety or neuroticism in Experiment 1. The null correlation with state anxiety failed to replicate previous findings in which high state anxiety was associated with poorer psi scoring (Broughton & Perlstrom, 1986, 1992; Roe et al., 2003), but these previous findings derive from intentional psi tasks. The null correlation between psi and neuroticism in Experiment 1 concurred with Watt and Ravenscroft (2000), who did not find a significant relationship between psi and neuroticism in a nonintentional psi task. In intentional psi tasks, negative correlations between psi and neuroticism have been found (W. G. Braud, 1981; Palmer, 1977). This has not been upheld in the nonintentional psi task investigations so far.

One limitation of the investigation into the effect of personality in this experiment was the low number of participants who provided personality data. This reduced the power of the analyses. The NEO PI-R took up to half an hour to complete, which was a drawback because it restricted recruitment; not all potential participants could spare such a long time for the experiment. As the NEO PI-R is completed on a copyrighted booklet it is not viable for participants to complete only individual subscales; the whole questionnaire has to be completed. This wasted time, as agreeableness and conscientiousness data was collected but not used. A further

drawback of the NEO PI-R was that it was scored manually, risking human error. A quicker, computer-based questionnaire would be preferable.

3.6. Conclusions

There was no overall psi effect. There was a replication of the response bias effect. There was a significant positive correlation between hit rate and extraversion, supporting the hypothesis that psi ability and extraversion are related, and extending this for the first time into a nonintentional psi task.

4. Experiment 2: The Effect of Hidden Targets on Incidental Timing of a Maths Task

4.1. Overview

This chapter presents Experiment 2, which investigated the effect of hidden targets and personality in a novel nonintentional psi task. Experiment 2 aimed to improve upon Experiment 1 by using a larger sample and an incentive for the participants to score in accordance with the hidden targets. Participants timed themselves completing a maths task. One aspect of the timing of the maths task dictated whether the participants received a pleasant reward or an unpleasant punishment. The associations between hit rate on the nonintentional task and participants' extraversion, belief in psi, openness to experience, neuroticism, and state anxiety were investigated.

4.2. Introduction

According to the PMIR model, behavioural responses to psi information happen via the triggering of people's usual behaviours, and one such mechanism suggested is timing (Stanford, 1974, 1990). As reviewed in Chapter 2, effects on timing have been found in nonintentional psi experiments, under certain conditions (Stanford & Associates, 1976; Stanford & Stio, 1976). In these previous experiments, the relative timing of one key trial in a word association task dictated whether participants entered a reward condition or a punishment condition. Timing has not previously been used as a vehicle for nonintentional psi in any task other than word association. In Experiment 2, participants timed themselves completing a maths task. Unbeknown to the participants, hidden targets (targets that the participant does not know exist) had been created, relating to the timing of the task. Specifically, the hidden target stated whether the last digit of the stopwatch, which recorded 100ths of seconds (centiseconds), should stop on an odd or an even number to score a hit. This

determined whether the participant received a reward or a punishment. The centisecond digit was chosen in preference to the minute digit, for example, because altering the number of minutes would be confounded with other factors such as the participant's maths ability. This task in Experiment 2 also differed from the word-association timing tasks in that a change in the total time of the maths task was the dependent variable, not the speed of one trial relative to the others. We hoped that this would reduce noise and confounds. Participants were hypothesised to adjust their timing nonintentionally to get an odd or an even final digit on the stopwatch in line with a randomly selected hidden target, more than mean chance expectation (MCE).

A reward might be important in providing people with an incentive, albeit nonintentionally, to "use" psi (Broughton, 2006a; Stanford, 1974, 1990). In Experiment 1 there was no incentive for the participants to generate psi hits, such as a reward or feedback. This lack of incentive may have reduced the chances of finding a psi effect. In Experiment 2 participants' responses on the nonintentional task will, unbeknown to them, lead to either a favourable reward if they achieve a hit or to a punishment condition if they do not. The following rewards, contingent upon hit rate, have been used in nonintentional psi experiments that found an overall significant psi effect: cartoons (Luke, Roe et al., 2008), erotic pictures (Luke, Delanoy et al., 2008; Stanford, 1973; Stanford & Associates, 1976; Stanford & Thompson, 1974), and erotic-toned relaxation (Stanford & Stio, 1976). The following punishment conditions have been used in nonintentional psi experiments that found an overall significant psi effect: a forced-choice ESP task (Stanford & Stio, 1976), a pursuit rotor task (Stanford & Associates, 1976), and vigilance or inspection tasks (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008; Stanford, 1973; Stanford & Thompson, 1974).

The reward condition in Experiment 2 was winning chocolate, and the punishment condition was to tell participants that they had not won this prize. The previous successful rewards (i.e., those that produced a significant psi effect) fell into two

main categories: joy (e.g., cartoons) and sexual arousal (e.g., erotic pictures and relaxation), both of which could be subsumed by pleasure. Chocolate was chosen as the reward for Experiment 2 because it should also be a pleasurable incentive. It is important to check that participants find the reward desirable, otherwise it might not provide an incentive at all (Luke, 2008). Participants' ratings of the desirability of the reward tasks and punishment tasks were checked by Luke, Roe and Davison (2008), in their second experiment only, but in all the other experiments referenced above these were not checked. In Experiment 2 this was checked by asking participants to rate their experiences of the reward or punishment.

Experiment 2 also continued the investigation of the relationship between nonintentional psi performance and the following personality measures: extraversion, openness to experience, neuroticism, state anxiety, and belief in psi. In Experiment 1, a significant correlation between hit rate and extraversion was found, and so extraversion was of particular interest in Experiment 2. In line with the findings in previous literature (see Chapter 2), we hypothesised that higher extraversion, openness to experience, and belief in psi will be associated with psi hitting, as will lower state anxiety and neuroticism.

In summary, in Experiment 2, participants timed themselves with a stopwatch completing a maths task. Unbeknown to the participants, there were hidden targets relating to timing, of either odd or even numbers. If participants stopped the stopwatch with the final digit (centiseconds) showing an odd or an even number that concurred with their hidden target (i.e., scored a hit), they won a reward of chocolate. If they did not score a hit they were told that they had not won chocolate. Firstly, we assessed whether participants stopped the stopwatch on a digit that concurred with their hidden targets more often than MCE. Secondly, we assessed the associations between participants' extraversion, belief in psi, openness to experience, neuroticism, and state anxiety on overall hit rate in the nonintentional psi task.

4.3. Hypotheses

1 More participants will stop their stopwatch on a digit in accordance with their hidden target (score a hit) than MCE.

2 Personality variables will be associated with the likelihood of a hit on the nonintentional psi task. Extraversion, openness to experience, and belief in psi are hypothesised to have a positive association with the likelihood of a hit on the nonintentional psi task. State anxiety and neuroticism are hypothesised to have a negative association with the likelihood of a hit on the nonintentional psi task.

4.4. Method

4.4.1. Design.

This experiment used a one group design. Participants' responses were compared to MCE of 50%.

4.4.2. Participants.

One hundred participants with a mean age of 22.87 years, 38 of whom were male and 62 female, participated for course credits. The participants were naïve to the purpose of the experiment, and did not know of the experimenter's interest in parapsychology.

4.4.3. Ethical Considerations.

We identified one potential ethical issue, deliberate deception of the participants, similarly to Experiment 1 (see Chapter 3). We addressed this by giving participants the right to terminate the experiment at any time and a full debrief following the experiment. The Psychology Research Ethics Committee at the University of Edinburgh granted ethical approval for this experiment to proceed.

4.4.4. Materials and Apparatus.

A 50-item maths task composed of 50 simple multiplication questions was presented on paper (see Appendix 2). An ATECH SW210 stopwatch, accurate to centiseconds, was provided along with a clipboard and pen.

Personality Questionnaires.

Extraversion, neuroticism, and openness to experience were measured on scales from the IPIP (Goldberg, 1999). Each scale has 10 items, such as “I feel comfortable around people” for extraversion and “I have a vivid imagination” for openness to experience. Some of the items, such as “I am not easily bothered by things” for neuroticism, are negatively phrased items. Participants respond on five-point Likert scale anchored at 1 (*strongly disagree*) and 5 (*strongly agree*). Each trait scale is scored from a minimum of 10 to a maximum of 50 points. High scores indicate, for example, high extraversion. The IPIP measures the same five-factor personality constructs as the NEO PI-R (Goldberg, 1999), but it is in the public domain, and therefore free and editable. A bespoke version was constructed (see Appendix 3), omitting the scales for agreeableness and conscientiousness, saving experiment time and money.

State anxiety was measured on a six-item state anxiety questionnaire (Marteau & Bekker, 1992). Participants rate items, for example “I am tense” on four-point Likert scales anchored at 1 (*not at all*) and 4 (*very much*). State anxiety is scored from a minimum of six to a maximum of 24 points; high scores indicate high state anxiety.

The final questionnaire was the Koestler Parapsychology Unit belief in psi 12-item questionnaire (Appendix 4). Participants respond on seven-point Likert scales anchored at 1 (*no*) and 7 (*yes*). Minor changes in the wording of two of the questions ensured the same responses on the seven-point Likert answer scale could be used

throughout, which facilitated online presentation. Belief in psi is scored from a minimum of 12 to a maximum of 84; high scores indicate a high belief in psi.

4.4.5. Procedure.

Target selection and security.

Prior to Experiment 2 starting, the experimenter generated the hidden targets using Microsoft Excel's random function. This produced a list of 100 numbers to correspond to the 100 participants. Each number ranged between 0 and 9. If the first number was odd the target for the first participant was odd, and so on. The list remained in electronic form. It was stored in a password-protected data area on the Department of Psychology's secure server at the University of Edinburgh. The experimenter was not blind to the targets.

Session procedure.

The experiment was carried out in one room within the experimental laboratory suite in the basement of the Department of Psychology, University of Edinburgh. Participants were tested individually. The experimenter greeted participants in the waiting area of the suite, or, occasionally, by the door to the building. They were shown into the experimental room. Participants sat at the testing desk with an experimental PC (Vision Master Pro 413) on it. The experimenter verbally explained the procedure. After an opportunity to ask questions, all the participants signed a standard consent form agreeing to participate. This explained that they could terminate the experiment at any time (no participants chose to). Participants practiced using the stopwatch.

The participants completed the personality questions and the STAI on the PC in Lime Survey, an online survey host (Lime Survey, 2005), and then went on to generate data that is discussed elsewhere in this thesis (see Chapter 8). Then they

completed the maths task on paper. Once each participant finished the maths task, the experimenter checked the final digit of the participant's recorded time for the maths task against his or her target. Participants scored a hit if their final digit was odd and their target was odd, or if both were even. The experimenter then told the participant that there had been a hidden task within the maths task for which they could win a prize of chocolate, and whether or not they had been successful. They chose their prize immediately from a large assortment if they won. They were not told what the hidden task had been. Participants were requested not to discuss the experiment with their classmates because they might also be participating. After the maths task the participants did the post-experiment questionnaire online. This included a manipulation check of how much the participants were motivated to win the reward and avoid the punishment and the belief in psi scale. All participants were asked "Did you win chocolate for the maths task?", and answered *yes* or *no* by clicking a tick-box onscreen. Participants who answered *yes* were then asked: "How happy were you to win chocolate?" and they answered on a seven-point Likert scale anchored at 1 (*very little*) and 7 (*very much*). Participants who had answered *no* were asked: "How disappointed were you NOT to win chocolate?", and answered on a similar Likert scale. The questions were routed by a function called *skip-logic* which enables different questions to be presented depending on the participants' previous answers. The belief in psi scale was presented last to avoid indicating to participants that the experiment had anything to do with parapsychology during the experiment. After the questionnaire was completed, participants had an opportunity to ask questions and were then shown out of the laboratory. Only after the results had been analysed were participants de-briefed by email, but they were never told what the hidden psi task had been.

4.4.6. Analyses.

Analyses were conducted in SPSS version 14 and Microsoft Excel.

4.5. Results

Manipulation check: Were participants motivated to win chocolate?

Participants' motivation for the reward was assessed by averaging participants' rating of their answers to the manipulation check questions (How happy/disappointed were you to win/NOT to win chocolate?). A low score on either question indicated low motivation for the reward. Participants overall expressed a medium motivation level for the reward ($M = 4.10$). Four is the midpoint on a seven-point scale. Thus this reward was moderately desired by the participants.

Hypothesis 1: More participants will stop their stopwatch on a digit in accordance with their hidden target (score a hit) than would be expected by chance.

Of the 100 participants, 47 scored a hit and 53 did not. This was non-significantly different from the MCE of 50%, exact binomial p (two-tailed) = .62. Therefore, this hypothesis was not supported as the participants did not get hits any more often, or less often, than chance. The two-tailed analysis was chosen to allow for the effect of psi-missing, significant below chance deviation, even though a positive deviation was hypothesised.

Hypothesis 2: Personality variables will be associated with the likelihood of a hit on the nonintentional psi task. Extraversion, openness to experience, and belief in psi are hypothesised to have a positive association with the likelihood of a hit on the nonintentional psi task. State anxiety and neuroticism are hypothesised to have a negative association with the likelihood of a hit on the nonintentional psi task.

This was assessed by backwards stepwise logistic regression with inclusion and exclusion criteria of .05 and .10. None of the personality and individual differences variables (predictors) were retained in the model. To produce the figures in Table 4, the predictors were forced to enter a logistic regression model.

Table 4: Logistic Regression for Personality and Individual Differences Predictors and Psi Task Performance

	95% CI for exp <i>b</i>				
	<i>B</i> (<i>SE</i>)		exp <i>b</i>	Lower	Upper
Constant	-1.74	2.64	1.42		
Not included in model					
Extraversion	-0.04	0.04	0.96	0.89	1.04
Neuroticism	-0.02	0.04	0.98	0.91	1.06
Openness to experience	0.06	0.04	1.07	0.98	1.16
State Anxiety	0.06	0.09	1.06	0.90	1.26
Belief in psi	0.01	0.01	1.01	0.98	1.04

Note. *N* = 98

The value of exp *b* indicates the direction of the effect, values greater than 1 mean that the odds of a hit increase with an increase in the predictor. Values of less than 1 mean a negative association between the predictor and the odd of a hit. The personality variables are significant predictors if the 95% confidence intervals do not contain 1. Therefore, none of these variables significantly predict a hit on the nonintentional psi task; the hypothesis was not supported.

4.6. Discussion

Experiment 2 aimed to investigate whether participants' behaviour would nonintentionally respond to information in hidden targets, when these dictated whether or not participants would "win" a reward. Participants timed themselves with a stopwatch completing a maths task. If participants stopped the stopwatch with the final digit (centiseconds) concurring with their hidden target, in terms of being showing an odd or an even number, they won a reward of chocolate. The first hypothesis, that there would be an overall positive deviation from chance for the hit rate, was not supported. Therefore, the results of this experiment do not support the psi hypothesis. This null finding was unexpected given that there have been two previous experiments that found a significant timing effect from nonintentional psi (Stanford & Associates, 1976; Stanford & Stio, 1976); however, in both those studies the significant effect was only found in certain conditions. Stanford and Stio (1976) found a significant timing effect between trials in which the target direction was *fast*, but not when it was *slow*, while Stanford and Associates (1976) only found a significant difference between trials with female and male experimenters. The findings from Experiment 2 concurred with Stanford and Thompson (1974), who did not find a significant effect in their word association and timing experiment. One explanation for the null findings is that the psi hypothesis is false. If this is not the case, there might be indications in previous experiments as to why there was a null effect in Experiment 2, and these are considered below.

One possible reason that the current study did not find a psi effect is that the timing task itself was quite different to that in the three Stanford experiments. In Experiment 2 participants scored a hit if the final digit on their stopwatch matched their hidden target by being odd or even, when timing themselves completing a maths task. In Stanford's experiments, participants scored a hit by being faster or slower on a randomly determined key trial in producing word associations relative to the speed of the other (non-key) word association trials (Stanford & Associates, 1976; Stanford &

Stio, 1976; Stanford & Thompson, 1974). These other trials might add noise, and so Experiment 2 used overall timing, not relative timing, as a simpler dependent variable. The exploratory idea that overall timing might better capture a psi effect was not supported. Stanford and Stio (1976) argued that their word association task involved participants giving their *dominant responses* (the responses that are most readily made) as participants voiced the first word that came to mind. Stanford and Stio (1976) reasoned that dominant responses are more likely to be facilitated than inhibited by psi. This led them to hypothesise that trials in which the key word association had to be produced faster (rather than slower) than the other responses would show a greater psi effect, and this was found. The same argument cannot be applied to whether participants stopped a stopwatch on an odd or an even digit, as neither response is more dominant, and this might account for the null effect found in Experiment 2.

A second difference between Experiment 2 and the timing and word association studies relates to Stanford's finding that better psi performance occurred with female experimenters (Stanford & Associates, 1976; Stanford & Stio, 1976). This might have been because the presence of an attractive female experimenter increased the all-male participants' need to take part in the erotic reward tasks, but it also might have been because female/male experimenter/participant pairs work better overall, as has been indicated in some intentional psi experiments (Dalton, 1994; Dalton & Utts, 1995). It is possible that Experiment 2 failed to detect a psi effect on timing because the participants were of mixed gender. To test this, a post-hoc chi-squared analysis was run, but it showed no difference in the scoring rates for male and female participants in Experiment 2: $\chi^2(1, N = 100) = .59$, non-significant. Therefore, in Experiment 2, neither gender scored better than the other, and so the null results are not attributable to the mixed-gender sample. In summary, the fact that the timing task was quite different in nature from that used in the word association and timing experiments might account for some of the difference in results.

There are other possible explanations for the null results in Experiment 2. Although Experiment 2 had a large sample size, each participant contributed only one trial, with a binary outcome. This might not have been sensitive enough to detect a psi effect, if there was one. Other studies with only one trial per participant did find results that support the psi hypothesis, however (Stanford, 1990), suggesting that sensitivity may not be the key issue here.

A further difference between the procedure in Experiment 2 and those of previous, successful experiments is the proximity of the targets to the participants. In previous experiments with hidden answers to exams the answers were attached to the students' answer documents (W. G. Braud, 1975; Johnson, 1973; Schechter, 1977; Willis et al., 1974). In Experiment 2 the targets were not in close proximity to the participants, and only existed in electronic form as numbers on a spreadsheet. Schechter (1977) suggested that the proximity of the target answers in the exam studies is one reason why the target answers affected the students' responses, but there have been other successful, nonintentional psi experiments in which the targets were not in close proximity to the participants. For instance, Stanford (1970) generated the psi targets after all the experimental trials had been run, and so at the time of the experiment the targets did not even exist. The table of random numbers that Stanford (1970) used to determine the targets did exist at the time of the experiment, but the entry point was only determined afterwards. Similar precognitive targets, generated by a random algorithm after each trial, were used in experiments that found an overall nonintentional psi effect (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008). Furthermore, in Experiment 1 the targets were presented in close proximity to the participants and an overall psi effect was not found. Therefore, target proximity is unlikely to be the reason for the chance effects in Experiment 2.

Finally, Schechter (1977) also suggested that the format of the targets might have been important in the exam studies. In the exam studies, the targets were presented in the same format that the answers would be in. In Experiment 2 they were not.

Nonetheless, it is unlikely that target format explains the null results, because other studies with targets in a different format from the participants' answers or responses have shown a psi effect. For example, precognitive targets that were not yet created and, therefore, had no format, have demonstrated an influence (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008; Stanford, 1970).

Stanford (1974, 1990) proposed that people nonintentionally use psi to fulfil their needs or reflect their inclinations. This is tested experimentally by arranging rewards and punishments contingent upon performance in nonintentional psi tasks, (e.g., Stanford 1973). Experiment 2 incorporated an incentive for participants to stop the stopwatch in accordance with their hidden target. Unbeknown to the participants they would win a reward of chocolate if they scored a hit. In order for this reward to function as an incentive it has to be desirable to the participants. As noted in the introduction, this has rarely been checked in previous experiments. In Experiment 2 participants' motivation ratings were near the midpoint of the scale, and so the reward might not have been a good enough incentive to motivate them, nonintentionally, to win. If the chocolate was not a good enough incentive, then, according to the PMIR theory, we would not expect a psi result in this experiment. Therefore, while the results of Experiment 2 did not show evidence of psi, they could be interpreted as supporting the PMIR theory. However, participants' motivation for the reward was estimated from amalgamated ratings from both those who won the reward and those who did not. The motivation scale used in Experiment 2 was confounded by the different wording for the two manipulation check questions used: Those who won rated how happy they were to have won, and those who did not win rated how disappointed they were. In previous research all the participants rated how pleasant their contingency task was, whether it was a reward or a punishment task (Luke, Roe et al., 2008; Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). In hindsight, it would have been better to use comparable questions when asking participants to rate the reward and punishment conditions, so that direct comparisons could be made.

The rating of how happy the participants were to get the reward in Experiment 2 is comparable to the ratings of how pleasant the rewards tasks were in the previous experiments (Luke, Roe et al., 2008; Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). The rating of how disappointed the participants who did not win in Experiment 2 were is not comparable, as the question was negatively phrased. As a post-hoc comparison, we can compare the ratings from the participants who won in Experiment 2 to the ratings of the rewards in the previous experiments. They were: Experiment 2: 5.66 on a 7 point scale (81% of the scale maximum), Luke, Roe and Davidson (2008): 7/10 (70% of the scale maximum), and Watt's experiments: 4/6 (67% of the scale maximum) (Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000). Therefore the chocolate reward was rated as being desirable by the participants in Experiment 2, and more desirable than the rewards in previous experiments. However, the key to making an effective incentive might not simply be that the reward is desirable, but that it is significantly more desirable than the punishment. In Experiment 2 the low ratings for how disappointed participants were in the punishment condition (2.69, 62% of the scale minimum) indicates that the participants were not particularly disappointed by the punishment, although this rating is confounded by the question being negatively phrased. In conclusion, if the reward and punishment provided an incentive, then Experiment 2's findings contradict the PMIR theory that participants will nonintentionally use psi to fulfil their needs. If, however, the reward and punishment failed to provide an incentive, the PMIR theory is not contradicted. Due to the limitations in the manipulation check questions neither conclusion can be reached. Either way, Experiment 2 did not support the existence of nonintentional psi.

It was an important aim of Experiment 2 to consider the role of personality in nonintentional forced-choice psi tasks. In Hypothesis 2, extraversion, openness to experience, and belief in psi were hypothesised to be positively associated with psi scoring, and neuroticism and state anxiety were hypothesised to be negatively

associated with psi scoring. No relationship was found between psi scoring and any of these individual difference variables.

There was no significant relationship found between psi scoring and extraversion in Experiment 2. In intentional psi tasks, extraversion has previously been found to be associated with higher psi scoring (Eysenck, 1967b; Honorton et al., 1998; Palmer, 1978; Sargent, 1981). Experiment 2 did not lend support to the idea that this relationship exists between between psi scoring and extraversion in nonintentional psi tasks.

No significant association was found between psi scoring and openness to experience. This finding is in contrast with a positive correlation previously found between performance on a nonintentional affective judgement psi task and openness measured on the IPIP (Luke, Roe et al., 2008). One potential reason for the difference in findings is that Luke, Roe and Davison's (2008) experiment was significant overall, whereas Experiment 2 was not. This would appear to be supported by the fact that the other investigation into the role of openness in a nonintentional psi experiment found both a null overall result, and a non-significant correlation with openness to experience (Watt & Ravenscroft, 2000). It would, however, theoretically be possible for participants high on a certain trait to score high on the psi task and those low on that trait to score low, producing an overall chance outcome. Therefore, an overall chance outcome does not necessarily mean that no association between psi and individual differences will be found. In Experiment 2, however, the evidence points to there having been no psi effect, which is one possible reason for there being no relationships found with the personality traits. The evidence for a relationship between psi and openness to experience in intentional psi tasks is currently minimal (see Chapter 2), and the relationship requires further research in both intentional and nonintentional tasks.

Experiment 2 found a non-significant relationship between hit rate and belief in psi. Previous investigations of the role of belief in psi in nonintentional psi tasks found positive relationships between hit rate and belief in psi (Kennedy & Haight, 1978; Luke, Delanoy et al., 2008). In intentional psi tasks the positive relationship between hit rate and belief in psi has been well documented (Lawrence, 1993; Palmer, 1971; Schmeidler, 1945). Contrary to the previous research, the experiments in this thesis do not lend support to there being a sheep-goat effect in nonintentional psi tasks. If there were a sheep-goat effect in nonintentional psi tasks, it would indicate that an initial difference in psi ability between sheep and goats led to the difference in belief. Although further research would be required to draw any firm conclusions, these findings cast doubt on that theory.

In Experiment 2, the relationships between psi hit rate and both neuroticism and state anxiety were non-significant. In a previous nonintentional psi task there was a non-significant relationship between psi score and neuroticism (Watt & Ravenscroft, 2000). This is at odds with the negative correlations found between psi hit rate and both neuroticism and state anxiety in intentional psi tasks (W. G. Braud, 1981; Broughton & Perlstrom, 1986, 1992; Palmer, 1977; Roe et al., 2003). Stanford (1990) theorised that neuroticism might be associated with psi-missing, because neuroticism is associated with self-defeating behaviour. Whilst this relationship appears to be upheld in intentional psi tasks, the limited investigations with nonintentional psi tasks do not support this theory so far.

On a practical note, the online survey method of administering the personality questionnaires greatly facilitated the coding and analysis. Creating a bespoke questionnaire was possible because the IPIP is in the public domain. This saved time in the experiment as the participants only completed the scales that were being investigated. It would also be possible in online surveys to randomise the order that the questionnaire items are presented in, if there were any concern about order

effects. In all, the online surveys were a great improvement on the paper booklet personality questionnaires.

4.7. Conclusions

The nonintentional psi task did not find evidence of a psi effect: The results were at chance. There were no significant relationships between personality variables and psi task performance.

5. General Discussion of Experiments 1 and 2: Hidden Targets and Nonintentional Psi

This section reviews the findings from Experiments 1 and 2. We start by briefly comparing similarities and differences across the findings of these studies. Given the number of overall null results, we also discuss what the optimal conditions for finding for a psi effect might be, and whether the designs of Experiments 1 and 2 approximated those conditions. We also describe what type of design might be better used for any hypothetical replications, and then make suggestions for future research. Throughout this general discussion, we will also address what implications our findings have for the wider literature.

Experiments 1 and 2 aimed to discover whether hidden targets could influence participants' behaviour in psychological tests. Experiment 1 investigated whether participants' judgements about line length would concur with hidden targets that contained the desired answers (although, in fact, both lines were the same length). Experiment 2 also used hidden targets, but these targets related to the timing of the task. Specifically, the targets determined whether participants should stop their stopwatch at the end of the task on an odd or an even number to achieve a hit. Experiment 2 also included a reward for participants who achieved a hit, as a psi incentive. In both studies, we also assessed whether psi ability correlated with extraversion, neuroticism, openness to experience, state anxiety, and belief in psi. Neither experiment found a significant psi effect, although there is suggestion of a psi effect in the significant response bias effect finding in Experiment 1. We also found a significant correlation in Experiment 1 between psi score and extraversion. We discuss these findings in turn below.

The null overall psi result in both studies raises two questions: whether psi exists and whether the experiments presented in this thesis would capture a psi effect, if there is one. We return to the second question later in this discussion, where we describe the

optimal conditions for psi testing and how these compared to the methods used in Experiments 1 and 2. Regarding whether psi exists at all, the review in Chapter 1 showed there is experimental evidence both for and against the psi hypothesis. The evidence from Experiments 1 and 2 did not support the psi hypothesis. Participants were no better at making line-length judgements in accordance with their hidden targets, and there was no indication of an effect from hidden targets on participants' reaction times on the maths task.

One indication that there might have been a psi effect in Experiment 1, despite the overall null results, was the replication of the response bias effect. The response bias effect is an apparent psi effect that manifests within participants' existing response biases. In Experiment 1 participants responded *left* or *right* to indicate which of each pair of lines they thought was the longest, over a series of presentations. Participants have naturally occurring response biases, this is, individual preferences to say *left* more frequently than *right*, or vice versa. The response bias effect is the tendency for there to be a proportionally higher psi hit rate on responses that are not favoured by any other (non-psi) bias. A response bias effect was indeed found; participants scored a higher proportion of hits on their less preferred responses. For example, if a participant tended to respond *left* more often than *right* overall, the hit rate was higher within their *right* responses. The response bias effect was predicted on the basis of previous research, which found a higher hit rate in the less frequently chosen answers. The response bias effect has been found in both intentional psi tasks (Stanford, 1967, 1973) and nonintentional psi tasks (Kreitler & Kreitler, 1972; Stanford, 1970; Wilson, 2002a, 2004). In most of the previous research, however, response biases were taken from the combined responses of the participants at group level, or group norms, and were not individual to each participant. They may, therefore, not have applied to all of the participants. In Experiment 1, we extended this finding by considering preferences at the individual level, thereby strengthening the claim that the response bias effect exists.

The response bias effect appears to be prevalent, manifesting in a wide variety of psi tasks, and we should consider what it tells us about psi function. Stanford (1967, 1973) and Kreitler and Kreitler (1973) originally explained the response bias effect as follows: People are more likely to notice or act upon a psi impression if it is somehow out of the ordinary or incongruous. In other words, a commonly-occurring thought would be more readily dismissed as not being a psi impression, because there is already a non-psi explanation for it (Stanford, 1973). This explanation, however, relies upon conscious awareness of the psi information, which may not be relevant in nonintentional psi experiments. In the line-length judgement task used in Experiment 1, we postulated neither that participants would have conscious awareness of the information in their hidden targets, nor that they would select the longest line based on deliberating whether they were receiving psi information. As the response bias has occurred in nonintentional psi experiments, in which participants are not necessarily aware of the psi information, we require an explanation that does not rely on conscious awareness of psi.

Stanford (1975) related the response bias effect to signal detection theory, which is concerned with how people discern uncertain signals, such as distinguishing a quiet sound from background noise. If psi information is seen as being a relatively weak “signal”, we can apply signal detection theory to the response bias effect in our study as follows. We assume that people will usually choose their preferred response in the line-length judgement task unless there is a strong enough psi signal to overcome this response bias. For example, people with a bias to respond *left* will give the answer *left* on all of the trials with too weak a psi signal to overcome this bias. However, a stronger psi signal may arise, and this would direct responses according to the hidden target (to either the left or the right). Importantly, when this strong signal triggers a *left* response, the overall hit rate on the left side will be proportionally lower (since this one correct hit is diluted by the many misses (false alarms) also on the left). Conversely, where this strong psi signal triggers a *right* response, the overall hit rate on the right side will be proportionally higher. This argument based on signal

detection theory works well within the current study, but it does rely on the assumption that some psi signals are stronger than others. Why this would be is not known, and so further research is required to understand the response bias effect.

As noted above, the null overall psi result in both studies raises the question of whether psi exists, to which these findings, despite the replication of the response bias effect, do not provide a conclusive answer, and also whether or not the experiments presented in this thesis would capture a psi effect, if there is one. To address the second question, the optimal conditions for psi testing will now be compared to the methods used in Experiments 1 and 2, with suggestions for improvements, future analyses, and future research. Three optimal conditions for psi testing were previously identified in a meta-analysis of forced-choice precognition experiments (Honorton & Ferrari, 1989). This meta-analysis focussed on intentional psi tasks, but these findings might be transferable to nonintentional tasks, and we review these considerations here.

Firstly, Honorton and Ferrari (1989) found that selected participants, those who were pre-tested and found to be above-average psi task performers, scored better than unselected participants. The participants recruited for Experiments 1 and 2 were unselected on the basis of previous psi task ability; we might have been more likely to find significant results if we had used pre-selected participants. Participant selection could be problematic for nonintentional psi tasks, as it is important that participants are not informed that they are in a psi experiment, and the selection process might reveal this. It might be feasible, however, to combine a nonintentional psi task with an intentional psi task for which participants are selected, without revealing the nonintentional task. This has the further advantage of enabling comparison between intentional and nonintentional psi task performance (e.g., Stanford & Thompson, 1974), and so this approach is recommended for future research. A second aspect from Honorton and Ferrari (1989) that might elicit a psi effect relates to their finding that individual participant testing, as opposed to group

testing, was psi-conducive. Both Experiments 1 and 2 used individual testing, and so cannot be improved on that account, and future studies should continue to test individually.

Other aspects of the participant sample might also be important in increasing the chance of finding any psi effect. Indications as to the optimal participants for psi testing were discovered in investigations of participants who performed well in free-response ESP tasks in the Ganzfeld (Honorton, 1997). To remind the reader, the Ganzfeld is a sensory deprivation and relaxation technique for participants, which has provided much support for the psi hypothesis (Bem & Honorton, 1994). Four characteristics of high performing participants were identified. Although these participant characteristics all relate to a specific testing paradigm, the Ganzfeld, they might apply to all ways of testing for psi. The first of these was prior parapsychology testing experience. It is worth reflecting on why prior experience might help; it might indicate that these participants were very keen to contribute to parapsychology research, and the issue of motivation will be considered further below. Another possibility is that participation increases peoples' psi ability. Data on prior parapsychology testing experience was not collected in Experiments 1 and 2. One suggestion for future research is to use participants who have participated in parapsychology experiments before, or if that is not practical, to train naïve participants. Once again, this might be difficult in a nonintentional psi paradigm as participants should be blind to the fact that they are in a psi experiment.

The second characteristic of successful Ganzfeld participants was involvement with mental disciplines, such as meditation (Honorton, 1977). Mental disciplines might assist psi task performance because they reduce internal noise and cognitive interference, and allow people's attention to come to their internal mental state (Honorton, 1977). For this reason, future studies might test only participants who are involved with mental disciplines. Again, however, this might not be relevant to nonintentional psi experiments, such as those used here. As argued in Chapter 1,

becoming aware of psi information might be a two-stage process, the second of which is gaining conscious awareness of unconsciously held information (Tyrell, 1947). If this is the case, a quiet mind and internal mental states might assist the second stage. However, if people's behaviour is nonintentionally influenced by psi, without them becoming aware of it, there might be no advantage in quiet internal mental states. Therefore, it is uncertain whether using participants involved in mental disciplines would lead to a greater effect in nonintentional psi tasks. This is one additional avenue for future research. One final characteristic of successful Ganzfeld participants was self-reported personal psi experiences, which will be considered below along with belief in psi. We next consider how psi might be enhanced in future studies by manipulating motivation.

Motivation is considered to be important in eliciting psi effects (Broughton, 1988, 2006a). Participants could be motivated by their innate enthusiasm about participating in the experiment, either because they are interested in psychology experiments in general, or because they are particularly interested in parapsychology. In Experiments 1 and 2, the purpose of the tasks, and the very fact that they were parapsychology experiments, was deliberately concealed from the participants. Therefore, this latter type of motivation is not possible in nonintentional psi experiments. Regarding motivation for any type of psychology experiment, those in Experiment 1 did volunteer, whereas those in Experiment 2 were students who had a requirement to take part for course credits, some of whom may not have participated otherwise. Therefore, Experiments 1 and 2 may not have used adequately motivated participants. Participants could also be motivated by the possibility of a future reward, and avoidance of a future punishment, contingent upon their performance in the nonintentional psi task. This is the basic premise of PMIR experiments, which aim to test whether participants will, without knowing about future rewards or punishments, behave so that they enter the reward condition more than chance would dictate (Luke, Delanoy et al., 2008; Luke, Roe et al., 2008; Stanford & Associates, 1976; Stanford & Castello, 1977; Stanford & Stio, 1976; Stanford & Thompson,

1974; Watt & Nagtegaal, 2000). Experiment 1 did not include a reward and punishment contingency, but Experiment 2 did: Participants entered the reward condition by responding with a reaction time in accordance with their hidden targets. Therefore, Experiments 1 and 2 did not clarify whether motivation is psi conducive.

As reviewed in Chapter 2, there is experimental support for the PMIR theory in that people appear to behave in accordance with hidden targets more often than chance in studies where this would enter them into a reward condition. However, the question of whether rewards and punishments are necessary for this effect has not been conclusively answered, because the one study that attempted a direct manipulation of the reward contingency, Luke, Roe et al., (2008), was confounded. In their no-contingency condition the participants were allowed to leave early, which might have constituted a motivating reward in and of itself (see Chapter 2). Given this, a reward was included in Experiment 2 as a conservative measure. Interestingly, the fact that a reward was included in Experiment 2 but not Experiment 1 might appear to offer a test of the reward contingency manipulation itself. No psi effect was found in either study, but these experiments were also sufficiently different that any hypothesised differences in results could not categorically be attributed to the reward motivation alone. One suggestion for future research is to continue to err on the side of caution by including incentives for participants, until the issue of whether reward and punishment contingencies do facilitate finding a psi effect becomes clearer. Another suggestion for future research is directly to compare participants' performance on identical tasks with and without reward and punishment contingencies, but to use a non-confounded design.

One additional problem with comparing tasks with and without reward and punishment contingencies comes from the theory that psi is need-serving (Stanford, 1990). In Experiment 1 there was no reward, which raises the question of whose needs would be served by any possible significant result, when there is no reward for the participants. Out of all the people involved in an experiment, the experimenter is

frequently the most invested in the experiment and motivated for it to succeed (Kennedy & Taddonio, 1976; Palmer, 1997). Therefore, a nonintentional psi experiment with no reward contingency could be construed as a test of the experimenter's psi. (A similar argument could apply to the no contingency condition in Luke, Roe et al.'s (2008) experiment.) For example, in Experiment 1, in which participants judged line-lengths without knowing that the lines were the same, or even that there were hidden targets, the only true beneficiary of a high hit rate would have been the experimenter. Furthermore, in Experiment 1, there was no reason for the participants to access their own target list through psi, as opposed to someone else's target list, or, indeed, any other list of answers that might exist, other than that the experimenter intended it. To recapitulate, the participants each did the line-length judging task with a list of hidden targets located close by. Other than proximity, there was no link between each particular participant and his or her hidden targets, unless that link was made by the experimenter's intention.

It is not possible to rule out the influence of experimenter psi unless the very notion of psi itself is ruled out (White, 1976). This leads to the possibility that individual differences among experimenters' own psi ability might lead to greater experimental success for some experimenters than for others. Indeed, it has been observed that some experimenters do consistently find evidence supporting the psi hypothesis, while others do not (Kennedy & Taddonio, 1976; Palmer, 1997). Further experiments might have a greater chance of finding a psi effect if experimenters with a good track record are used.

Another psi-conducive factor is trial-by-trial feedback, and short intervals between testing and feedback (Honorton & Ferrari, 1989; Palmer, 1978). This is perhaps because feedback might act as a reward, with a greater effect the sooner the feedback is given. In Experiment 1, participants were not given feedback. If further work were carried out on this line-length judgement paradigm, one option for trial-by-trial feedback would be to generate hidden targets randomly on the computer in real time.

This would facilitate trial-by-trial feedback, which was not possible with the paper target set-up that was used. Conversely, in Experiment 2 there was only one trial, and participants were given feedback almost immediately, and so this task cannot be improved in this way.

We turn now to our findings relating to psi ability and personality. A positive correlation between hit rate and extraversion was found in Experiment 1, but not in Experiment 2. We also tested for the influences of neuroticism, openness to experience, state anxiety, and belief in psi, but these were all non-significantly associated with psi hitting. Below we discuss these findings in turn. In this, we again relate our findings to the wider literature, and discuss ways in which future studies might better approximate optimal conditions.

In Experiment 1, psi hit rates were positively correlated with participants' extraversion. This accords with previous findings from intentional psi experiments (Eysenck, 1967b; Palmer, 1977; Sargent, 1981; Rao, 1974). Honorton, Ferrari and Bem (1998), however, concluded that the relationship between psi scoring and extraversion in forced-choice intentional psi tasks was artefactual, the result of participants' knowledge of positive psi scoring inflating their self-ratings of extraversion. This artefact could not apply in Experiment 1, as the extraversion measure was administered before the psi task. Experiment 1 therefore extended the extraversion and psi findings into a nonintentional task, while avoiding the risk of the aforementioned artefact. A second advantage of Experiment 1 is that we considered a range of personality factors along with extraversion. In some previous research extraversion and neuroticism were correlated, and the effect of neuroticism explained the apparent effect of extraversion on psi performance (see, e.g., Palmer, 1977). Hence any correlation between psi and extraversion that did not consider neuroticism might have been confounded. In Experiment 1, therefore, we avoided this confound by independently showing that extraversion, but not neuroticism, was the influential factor.

Importantly, this correlation between psi and extraversion was not replicated in Experiment 2. Experiments 1 and 2 had considerably different sample sizes (21 vs. 100, respectively). Had we failed to find an effect with extraversion in the study with the smaller sample, we might have attributed this failure to a lack of power. However, the reverse was found: Experiment 1 had only 21 participants, but still revealed this effect. Additionally, a positive relationship between psi and extraversion has previously been found with a similar sample size of 23 (Bellis & Morris, 1980). For this reason, we cannot easily attribute the differences in the findings to a difference in power between these studies.

There might have been some differences in task sensitivity which led to a relationship between hit rate and extraversion in Experiment 1 but not Experiment 2. The task in Experiment 2 was arguably less sensitive than the task in Experiment 1, as the task in Experiment 2 had only one trial per participant. This could have reduced the likelihood of this task detecting an association with extraversion, assuming that there is an association to be found. Future replications should perhaps therefore include a greater number of trials per participant, in order to generate a better estimate of means across items.

In addition to sample size and sensitivity, a third difference between these two studies is that we used different questionnaires to measure extraversion. We used the NEO PI-R in Experiment 1, and the IPIP in Experiment 2. Although these scales have been found to correlate highly, and are thus thought to measure the same constructs (Goldberg, 1999), they are not exactly the same. A recent study compared extraversion scores on the Eysenck Personality Inventory (EPI) and a short form of the NEO PI-R, the NEO-FFI, to performance on intentional forced-choice psi tasks (Roe, Henderson, & Matthews, 2008). Roe et al. (2008) found a significant positive correlation between psi scoring and extraversion measured on the EPI, but not between psi scores and extraversion measured on the NEO-FFI. Although Roe et al.'s study compared the EPI and the NEO-FFI, not the NEO PI-R and the IPIP as in

this thesis, this example demonstrates the possibility of two different extraversion questionnaires leading to two different outcomes.

In summary, we found an apparent relationship between psi and extraversion that does not appear to be attributable to issues relating to power or task-ordering, or to confounds with other personality traits. If there is a genuine relationship between nonintentional psi and extraversion, we should consider what that indicates about how psi functions, although findings from just one experiment are only tentative evidence until replicated. Firstly, any relationship between a personality characteristic and psi performance might indicate that psi is not merely an anomalous deviation from chance, but an effect that co-varies with other factors known to influence human behaviour. This in turn strengthens the psi hypothesis. A relationship between psi performance in laboratory tests and extraversion, specifically, could be due to several reasons: extraverts' increased spontaneity, greater ease and comfort in social situations, and low cortical arousal (Sargent, 1981). Palmer (1978) concluded from a wide review of previous research that spontaneity is psi-conductive. Palmer suggested that more spontaneous people are more likely to produce responses that go against their usual response biases, thereby favouring their chances of psi hitting through the response bias effect (see above). One suggestion for future analyses of the data from Experiment 1 would be to investigate this, by testing whether participants' extraversion is related to their tendency to show increased psi hitting on their less-preferred responses.

A second reason why extraversion might relate to psi was noted by Rao (1974). Rao suggested that extraverted participants interact more easily with the experimenter than introverted participants do, and this might create a psi-conductive environment. Experiment 1 was not very social; the participants were alone most of the time, and so being more at ease with the experimenter is unlikely to have contributed to the extravert's improved performance. Eysenck (1967a,b) argued that extraverts' lower levels of cortical arousal might also be psi-conductive. This is corroborated by

experiments that found that increased relaxation levels (W. G. Braud & Braud, 1974) and sensory deprivation (Honorton, 1977), which both lower cortical arousal, were psi-conducive. As argued in Chapter 2, the advantage of low cortical arousal could be that it aids the conscious retrieval of psi information. In nonintentional psi tasks, however, a psi effect is indicated by a behavioural change, rather than conscious awareness per se. Hence, an effect from extraversion in a nonintentional task would imply that extraversion or low cortical arousal is advantageous not only to becoming aware of psi information, but also to receiving it. The possibility that extraverts are more receptive to psi might be because extraverts desire stimulation (Matthews & Deary, 1998), thus they might attract more information through psi than do introverts. Future research might investigate psi performance and extraversion in comparable intentional and nonintentional tasks. One final implication of our positive association between psi performance and extraversion is that future studies would be well advised to screen participants for extraversion to maximise the chances of finding a psi effect.

We turn now to the fact that there were null results in both Experiments 1 and 2 for the relationships between psi and neuroticism, state-anxiety, openness to experience, and belief in psi. In this section, we describe why these effects may not have been found in the current studies, and what light this sheds on the existing literature on psi and individual differences.

Firstly, we will consider the null findings between psi and measures of neuroticism and state anxiety in both our experiments. Previous findings have indicated that there may be reduced psi, or psi-missing, with higher neuroticism (Palmer, 1977). This negative association between neuroticism and psi was also predicted by Stanford (1990), on the basis that the self-defeating behaviour of neurotic people might lead them to use psi against themselves, (i.e., to achieve the opposite effect than serving their needs). The limited previous research with state anxiety showed that it, also, was negatively associated with psi task performance (Broughton & Perlstrom, 1986,

1992; Roe et al., 2003). These characteristics were included in our study primarily to further explore the effects of neuroticism and state anxiety in nonintentional psi performance. Secondly, the interaction found between extraversion and neuroticism (see above), meant that any effect found with the former would necessitate investigating an effect in the latter, to avoid a misleading result. In Experiments 1 and 2, we found no significant relationship between psi scoring and neuroticism or state anxiety. These null results accord with the non-significant relationship found between psi scoring and neuroticism in a nonintentional affective judgement psi task (Watt & Ravenscroft, 2000), but not with the previous findings from intentional psi studies. There have been two reviews of the relationship between psi and neuroticism, and these concluded that a relationship between psi hitting and low neuroticism does exist (W. G. Braud, 1981; Palmer, 1977).

The last of the Big Five personality traits considered in Experiments 1 and 2 was openness to experience. Neither experiment found a significant relationship between hit rate and openness to experience. This is a failure to replicate the positive association found between psi hitting and openness in a nonintentional affective judgement task (Luke, Roe et al., 2008). There is less previous research investigating openness to experience than extraversion or neuroticism. Some previous studies have found a relationship between psi and openness to experience (Osis & Bokert, 1971; S. P. Wright, 2003) whilst others have not (e.g., Roe, Holt, & Simmonds, 2003). Given this, the experiments reported in this thesis might be added as evidence against the hypothesis for a robust relationship between psi and openness to experience, on the assumption that our methodology provided an adequate test. One issue for consideration is that, although Experiment 1 found a non-significant correlation between hit rates and openness, there was a large effect size, which suggests that with a larger sample a significant effect might have been found. In other words, there may yet have been a suggestion that openness plays a role, and future studies might try to elicit that effect with a larger sample size.

The sheep-goat effect, that believers in psi tend to score better in psi tasks than disbelievers, was not found in either Experiment 1 or 2. This is at odds with the previous findings in nonintentional psi experiments, (Kennedy & Haight, 1978; Luke, Delanoy et al., 2008), and with previous findings in intentional psi experiments (Palmer, 1971, 1972). As the estimated effect size of the sheep-goat effect is small, it is possible that Experiment 1, which had a small sample size, lacked adequate power to detect this effect. On the other hand, Experiment 2 had 100 participants, the same sample size as Luke, Delanoy et al.'s (2008) experiment, which did find a significant positive correlation between hit rate and belief in psi. One possible reason for the failure to replicate the sheep-goat effect may be related to the different scales used to measure belief in psi. Luke, Delanoy et al. used a five-item scale modified from Palmer (1971), which asked about the participants' belief in psi and their perceived psi ability only. Experiments 1 and 2 used the 12-item Koestler Parapsychology Unit questionnaire, which asked about belief in a variety of psi events, first- and second-hand experience of the same psi events, and perceived psi ability. It is possible, therefore, that these different scales tested slightly different constructs. Moreover, although Lawrence (1993) concluded that all scales for belief in psi detected the sheep-goat effect equally well, his assessment included neither the Palmer (1971) questionnaire nor the KPU questionnaire.

A second possible reason for the failure to replicate the sheep-goat effect concerns the type of questions within each scale. The original sheep-goat question used by Schmeidler (1945) asked whether participants believed in, or rejected, the "possibility of paranormal success under the conditions of the experiment" (Schmeidler & McConnell, 1958, p. 30). Since this question generated a significant effect, we might have benefited from asking exactly the same question here. Nonetheless, a very similar question did appear on the scale used in both Experiments 1 and 2 "Do you believe that you might be able to demonstrate any psi in a controlled laboratory experiment?", the difference being just that our question referred to psi experiments in general, and not specifically to the task participants

were about to do. We decided to use this question because it was not possible in Experiments 1 and 2 to ask a question that linked the task at hand and belief in psi, as participants were required not to know that they were in a psi experiment. As such, the version we presented was designed to be the best possible approximation under the circumstances. With the benefit of hindsight, however, a preferable alternative might have been to ask “Schmeidler’s question” exactly, but prior to an intentional task and nonintentional task combined. This approach is recommended for future research.

A second difference between Luke, Delanoy et al.’s (2008) experiment and Experiments 1 and 2 that might explain the failure to find the sheep-goat effect, is the difference in their overall findings. Luke, Delanoy et al. had an overall positive psi effect, and Experiments 1 and 2 did not. If there were no psi effect at all in Experiments 1 and 2, there would have been no psi effect to correlate belief in psi with. However, since a correlation was found with extraversion in Experiment 1, it was worth investigating whether the overall null findings masked differential scoring between sheep and goats, or indeed from any of the other individual differences variables investigated here.

The sheep-goat effect has predominantly been investigated in the context of intentional psi experiments, from which it was uncertain whether sheep scored better because of their positive expectations, enthusiasm for psi, beliefs, or ability (Rao, 1966). Research on the effect of belief in psi in nonintentional psi experiments would differentiate between these effects, as, of the four reasons above, only psi ability could conceivably influence performance when participants were not aware that they were using psi. Previous research has found a sheep-goat effect in nonintentional psi experiments (e.g., Kennedy & Haight, 1978; Luke, Delanoy et al., 2008), indicating that intrinsic differences in sheep’s and goats’ psi ability underpins the sheep-goat effect. The findings from Experiments 1 and 2 did not support this claim, and suggest

that the sheep-goat effect might not be due to intrinsic differences in psi ability, but due to differences in belief, expectation, or enthusiasm.

5.1. Summary and Rationale for Experiments 3, 4, and 5

The psi tasks presented in Experiments 1 and 2 did not show overall support for the psi hypothesis, although a psi effect was indicated by a response bias effect and a correlation between hit rate and extraversion in Experiment 1. We have discussed improvements to our designs that might be implemented in future studies and how our findings relate to the previous literature. In the following section, we now turn to the second half of this thesis, which investigates nonintentional psi effects in the context of social influences.

Experiments 1 and 2 investigated whether psi would manifest in nonintentional behavioural responses to hidden targets. This effect was not found. One observation made earlier was that Experiment 1 had no social influence, and a social influence had previously been a factor that influenced line-length judgements similar to the task used in Experiment 1. In these earlier, non-psi studies, participants gave incorrect estimates about the length of a line, if those same judgments had previously been given by others in a social context (Asch, 1951, 1955). In a similar way, Experiment 1 also aimed to elicit incorrect judgements from participants about which line was longest (since the lines were, in fact, the same length), but did so through a psi manipulation. In other words, Experiment 1 attempted to create a psi analogue of a social effect (pressure to conform in line-length judgements), but without including a psi equivalent of the social pressure to conform, such as a sender. Indeed, neither Experiment 1 nor Experiment 2 had any social component in the psi influence. Interestingly, some studies have shown that a better psi effect is achieved with a sender (e.g., Kreitler and Kreitler, 1973) although the value of a sender has been debated and is by no means established (see, e.g., Roe, Holt, & Simmonds (2003) for a more complete discussion). Given that social influences may generate positive

effects in both non-psi line-length judgements, and also in prior psi studies, a better approach might have been to combine a social and a psi influence together, an idea that we address in the second half of this thesis.

A second consideration for the latter half of this thesis comes from a proposal of Stanford's (1990), already discussed above, that, through psi, people nonintentionally respond to circumstances beyond their sensory reach, if those circumstances would elicit a response given sensory knowledge of them. This account of psi might be interpreted to suggest that psi-mediated behavioural responses would parallel "normal" responses to those same events. In other words, people's responses to psychic information about 'x' might be the same responses that they would make to explicit knowledge about 'x'. This comparison is implicit in the nonintentional psi experiments reviewed above, but was not explicitly compared. For example, previous nonintentional psi studies made no comparison between the behaviour that participants were expected to make in response to their hidden targets, and the behaviour that actual knowledge of those hidden targets would have generated. This omission might be due to this comparison appearing too obvious. For example, it might seem self-evident that explicitly telling participants that responding a certain way on a task will lead to a reward will make them respond in that way. However, if done carefully, we might successfully compare hidden psi and explicit non-psi influences on behaviour.

Combining the ideas above, the second half of this thesis presents three studies that juxtapose a social influence and a psi influence, while comparing their effect on behavioural responses. In this, we take as our dependent variable a well-established behavioural response to a social situation: changes in participants' task performance from being watched. Then, we compare both explicit non-psi and hidden psi influences that might lead to this behaviour. Specifically, we compare being watched by a physically present observer to being watched remotely by a hidden observer. This thesis will now go on to review the literature on social influences on behaviour,

social facilitation effects, and any evidence for psi-mediated effects from observation: remote observation, and how these phenomena might be combined.

6. Literature Review of Social Facilitation and Remote Observation

6.1. Overview

The purpose of this thesis is to investigate ways in which people's behaviour is nonintentionally affected by information or influences that can only be received through psi, whether or not the people are consciously aware of the psi information or influence. Experiments 1 and 2 investigated the effects of hidden targets on participants' performance on psychological tasks. We argued in Chapter 5 that the effect of a hidden psi influence on behaviour might be better investigated by using a social influence that gives rise to a well-researched behavioural response. In Experiments 3-5, we will therefore combine a psi influence with a well-researched behavioural response: the social facilitation effect. The social facilitation effect is that people perform tasks differently when they are being watched, compared to when they are alone. We will investigate the social facilitation effect in conjunction with a psi influence known as remote observation. Remote observation is observation that cannot be detected by normal sensory means. We shall define both these terms (social facilitation, remote observation) in greater detail below, and link them to the studies that follow in the subsequent chapters. In brief, these studies will investigate whether remote observation (a psi influence) leads to the same behavioural responses as observation (by non-psi means) found in the existing social facilitation literature. Therefore, in this chapter, we present reviews of the literature on social facilitation and remote observation detection. In this, we make a case for why there might be a social facilitation effect from remote observation. We also consider how this current research would add to the fields of both social psychology and parapsychology.

6.2. Social Facilitation

This section reviews the effect of normal (non-psi-mediated) observation on people's behaviour. We examine the effect of observation on behaviour by considering the field of social facilitation, a term first coined by Allport (1920). Starting with investigating the effects of *competitors* (Triplet, 1898), the field of social facilitation has examined the way that many different social influences affect one participant's task performance. These social influences have predominantly included *co-actors* (Allport, 1924), people who are doing the same task as the participant, and *audiences* (Travis, 1925), people who watch, but do not take part in the tasks with the participant. This review distills, from the wide field of social facilitation, the research that is relevant to the question of whether remote observation could elicit a social facilitation effect. It will, therefore, focus mainly on the effect of audiences, rather than co-actors, because the behaviour of the co-actors is an additional and possibly confounding variable. In Experiments 3-5 we used audiences, not co-actors.

In most social facilitation research, audiences were both physically present and observing the participant (C. F. Bond & Titus, 1983; Guerin, 1993); however, at present we require these influences of observation and presence to be disentangled and these terms to be defined. Some research considered the effect of presence without observation (e.g., Cottrell, Wack, Sekerak, & Rittle, 1968) and others observation without presence (e.g., Geen, 1973). For the purposes of this review, *presence* is defined as the physical existence of another person (or people) in the same room as the participant, and does not necessarily imply observation. *Observation* is defined as being watched, and does not necessarily imply that the observer is present. Observation from an audience that is both present and observing will be termed *proximate observation*. Later, we shall define other types of audience as they become relevant.

Firstly, however, we will explore the social facilitation effect. We will consider the importance of task complexity in social facilitation, how this has been researched previously, and issues relating to it. We then discuss the leading theories of social facilitation and how they tie in with previous research evidence. Following that, we consider previous studies that investigated participant characteristics and the observer-participant relationship in social facilitation experiments, and how these link with the most pertinent of the theories. We then consider whether observation, in and of itself, can influence behaviour. Throughout this review, we distill the methodological implications for testing social facilitation in our subsequent experiments.

6.2.1. Task Complexity.

We have defined social facilitation thus far as an effect that alters behaviour according to whether one is being watched. Specifically, the social facilitation effect is that people perform simple tasks better (*facilitation*), and complex tasks worse (*inhibition*), when they know that someone is watching them, or when they are in the presence of another person. Social facilitation therefore involves both facilitatory and inhibitory effects from presence or observation on performance. For this reason, it is sometimes called *social facilitation/inhibition* (e.g., Zanbaka, Ulinski, Goolkasian, & Hodges, 2007) or *SFI* (e.g., Huguet, Galvaing, Monteil, & Dumas, 1999) but it is usually just called *social facilitation*. Following convention, this thesis will use the term social facilitation, but it is important to note that this encompasses both facilitatory and inhibitory effects.

The distinction between what are now usually called simple and complex tasks was first made by Zajonc (1965). In a review of the seemingly contradictory previous research, some of which found performance was facilitated by audiences, and some that it was inhibited; Zajonc noted that the tasks that audiences facilitated were well-learned. He also noted that tasks inhibited by audiences were ones that participants

were still learning, or that were novel. He proposed that this effect was due to the audience inducing participants to produce the response they are most likely to make, (i.e., their dominant response). On well-learned tasks, people's dominant responses are likely to be correct; hence the audience facilitates performance, whereas the opposite applies to novel tasks. Later the distinction between *simple* and *complex* tasks replaced the previous terminology (e.g., Zajonc, 1980) to accommodate easy tasks which are facilitated without the need for practice, and difficult tasks that remain inhibited even when well-practiced. This led to the current delineation between *simple tasks*, which are easy or well-practiced, and *complex tasks*, which are difficult or novel (e.g., Uziel, 2007).

A variety of simple and complex tasks have been investigated previously, using both quantitative measures such as speed, and qualitative measures such as accuracy (C. F. Bond & Titus, 1983). An example of a simple task is that Grant and Dajee (2002) found that proximate observation increased the speed of participants' completion of simple multiplication, compared to participants who completed the task alone, (see also Dashiell, 1930). Another simple task was used by Bergum and Lehr (1963), who trained participants in detecting when a light failed to come in a sequence of flashing lights. Participants under proximate observation performed, on average, 34% more accurately than those who were alone. Some experiments used both speed and accuracy measures. For instance, students completed a simple maze task both faster and with fewer errors under proximate observation compared to those who did the maze alone (Rajecki, Ickes, Corcoran, & Lenez, 1977). Finally, Ferris and Rowland (1980) found participants' performance was facilitated by proximate observation when playing a simple video game.

Strauss (2002) considered the complexity of motor tasks. He categorised them as those that require predominantly co-ordination skills, such as balancing; those that require strength, such as weight-lifting; and those that require both, such as gymnastics. Strauss argued that tasks that require strength were simple tasks, and, in

a review of the literature, found that they were usually facilitated by an audience. Of tasks that require co-ordination, those that were easy, or in which the participants were well trained, were facilitated by audiences. For example, participants well-trained in a pursuit-rotor task of following a moving target with a stylus performed better with an audience compared to their performance when alone (F. G. Miller, Hurkman, Feinberg, & Robinson, 1979; Travis, 1925). The direction of the effect of an audience on tasks which require both co-ordination and strength depends upon the participants' initial skill level, and thus whether the tasks are simple and complex to individual participants (Strauss, 2002).

Some examples of complex tasks that were inhibited by observation include a decision making task used by Wapner and Alper (1952). Participants chose one of two words that matched the meaning of a preceding phrase, for example, *a musical composition* followed by the choice of: *symphony* or *concerto*. This was deemed a complex task because there were competing dominant responses; both words could be right so it was difficult to choose between them. Participants' decision-making time was lengthened by observation. In another study with a complex task, Bradner and Mark (2001) found that performance on a cognitively demanding maths task was slowed by observation through videoconferencing media. Observation has also been found to inhibit complex task accuracy. For example, proximate observation impaired participants' accuracy on difficult, novel, computer-based anagram-solving tasks (Aiello & Svec, 1993), on learning complex word pairs (Berkey & Hoppe, 1972) and on learning complex nonsense words (Pessin, 1933). Finally, Rosenbloom, Shahar, Perlman, Estreich and Kirzner (2007) compared the success rates of people who took driving tests either with the examiner only, or with the addition of another testee in the back of the car, a type of audience condition. A higher pass rate was achieved by the people with the examiner only. These examples show that the social facilitation effect has been demonstrated with a wide variety of tasks.

To estimate the magnitude of the overall social facilitation effect, C.F. Bond and Titus (1983) conducted a meta-analysis of 241 studies. They estimated the mean effect sizes for the social facilitation effect, separately considering simple and complex tasks, and speed and accuracy measures (see Table 5).

Table 5: Effect Size and Fail-Safe *N* Estimates for the Social Facilitation Effect (data from C.F. Bond & Titus, 1983)

Task complexity	Measure	Effect size <i>d</i>	<i>N</i> studies	Fail-safe <i>N</i>
Simple	Speed	0.32	154	6183
	Accuracy	0.11	112	194
Complex	Speed	-0.20	54	160
	Accuracy	-0.36	147	5697

Note. The positive effect size values indicate facilitation from observation, and the negative effect sizes indicate inhibition from observation.

As can be seen in Table 5, the greatest effect sizes were found in experiments measuring the speed of simple tasks or the accuracy of complex tasks, which were both small to medium effect sizes (J. Cohen, 1988). These are also the two effects least at risk of the file-drawer effect, a potential spurious finding based upon the selected publishing of significant studies only. The fail-safe *N* is the estimated number of unpublished null studies which would reduce the effect to non-significance (Rosenthal, 1984). For the effect sizes of speed of simple tasks and the accuracy of complex tasks, around 40 unpublished null studies would have to exist for every study considered in the meta-analysis. For the smaller effect sizes of the accuracy of simple tasks and the speed of complex tasks, only around two unpublished null studies would have to exist for every study considered in the meta-analysis to reduce the effect to non-significance. These effect size estimates indicate that in order to best capture an effect, experiments should include both speed and accuracy measures of both simple and complex tasks.

This meta-analysis has limitations that might reduce the accuracy of the effect size estimates pertaining to the effect of proximate observation. Firstly, only 162 of the

241 studies included used observers, the others used co-actors. Secondly, C.F. Bond and Titus (1983) classified participants as being “alone” if the researchers had called them alone, even though in 96 of these studies an experimenter was in the room with the participant, and in 52 of those studies the participant could see the experimenter. The true effect size for social facilitation might therefore be larger than C.F. Bond and Titus estimated, as greater facilitation and inhibition effects were found in studies with a physically alone condition compared to the overall mean (this analysis only considered a subset of studies, see Uziel, 2007). (The implications of poor control in the alone condition in social facilitation research will be discussed in more detail below, see Section 6.4.2.) Thirdly, C.F. Bond and Titus accepted the researchers’ decision of whether a task had been simple or complex, and this distinction is somewhat problematic.

The use of the simple/complex task distinction was criticised for lacking any objective criteria by which to classify tasks (Uziel, 2007), and for being arbitrarily decided (Strauss, 2002). If stricter classifications for simple and complex tasks were used, larger effect sizes might have been found in the meta-analysis cited above. As noted above, if the distinction between simple and complex tasks were that the dominant responses were correct or incorrect, then there would be an objective criterion; however, this is seldom used. One rare example is from a study by Rajecki et al. (1977), who used a criterion of over 50% correct answers for their simple task. One other problem with the simple/complex task distinction is that it could depend upon individual differences in skill level. It might be possible for a task to be simple for some of the participants and complex for the others (e.g., Aiello & Kolb, 1995; Baron, Moore, & Sanders, 1978; Berkey & Hoppe, 1972). Despite these caveats, simple task facilitation and complex task inhibition are the recognised social facilitation effects, and the theories that seek to explain social facilitation focus on these performance effects.

6.2.2. Social Facilitation Theories.

The following section briefly outlines the theories of social facilitation. These theories can be grouped into three main types: activation theories, social conformity theories, and cognitive process theories.

The activation theories argue that presence or observation leads to an increase in *activation* in the participant; that is, an increased attentiveness or readiness for action. Zajonc (1965, 1980) proposed that this increase in activation is due to an increase in alertness in preparation to respond to the potential threat imposed by social presence, underpinned by an increase in generalised *arousal*. According to the Hull-Spence drive theory (see Spence, 1958) this increased activation leads to an increase in dominant responses, and hence to the social facilitation effect. Activation has been used synonymously with arousal and *drive* (Zajonc, 1965), but following Strauss (2002) this thesis will use the term activation when referring to these theories. In addition to presence, other aspects of being observed have been proposed to increase activation and thus lead to the social facilitation effect. For example, self-presentation concerns, such as the apprehension of evaluation by others (Cottrell et al., 1968; Henchy & Glass, 1968; Feinberg & Aiello, 2006), comparing oneself negatively to others, or concern about the impression one makes (Baumeister, 1982; C. F. Bond, 1982). Activation can also result from the distracting effect of the participant watching the audience (Dashiell, 1935), or thinking about the audience (Allport, 1924). The conflict between the distraction and the cognitive demands of the task is called distraction-conflict, and is also hypothesised to cause social facilitation by increasing activation (Baron, 1986; Baron, Moore, & Sanders, 1978).

The social conformity theories all concern standards of behaviour in the presence of others. The objective self-awareness theory argues that the people view themselves the way that the observer does, or is imagined to, and this measuring of actual performance to the expected standard is the cause of the social facilitation effect

(Wicklund & Duval, 1971). The controls system model also argues that people seek to reduce the discrepancy between their actual performance and their standard (Carver & Scheier, 1978). Neither of these theories propose a mechanism for the way that the aversive state of performing below one's expected standards leads to the social facilitation effect, unless it acts like activation (Guerin, 1993).

The cognitive processing theories include attentional theories. One attentional theory argues that the observer is a disruption that facilitates simple task performance and inhibits complex task performance by bringing more attention to the tasks (Manstead & Semin, 1980). Other theories claim that the observer draws the participant's attention away from the task (Huguet et al., 1999), or that the presence of co-actors leads to increased attention focussing, and that this in turn leads to the social facilitation effect (Muller & Butera, 2007).

There have also been attempts to unite the theories to make one that is universally applicable, but they have all failed (Guerin, 1993). All of the theories focus on the simple task facilitation, complex task inhibition pattern, and most of the theories explain this pattern almost equally well. The theories are therefore underdetermined. Uziel (2007) argued that the theories are not mutually exclusive, that they each represent a way of reacting to observation, and can co-occur. It is very likely that aspects of all of the processes described in the theories above all occur in social facilitation, especially as the predicted outcomes are very similar. The majority of empirical research has focussed on the activation theories, which have the most proof (especially for presence and evaluation apprehension), and provide the best theoretical framework (Geen & Gange, 1977). The other theories tend to be supported only by their originator's own research (Strauss, 2002). One limitation of this research is that in many experiments presence and observation were manipulated, and task performance was measured, with no direct test of whether activation mediated the social facilitation effect (e.g., Matlin & Zajonc, 1968; Deffenbacher, Platt, & Williams, 1974; Geen, 1973; Plantania & Moran, 2001;

Zajonc & Sales, 1966; J. L. Cohen & Davis, 1973). There is, however, evidence, although not without caveats, that activation is increased by observation and/or presence. As we will see below, activation provides a link to the influence of remote observation, which has been found to affect sympathetic nervous system activation.

6.2.3. Proximate Observation and Activation.

As noted above, activation theories state that an increase in arousal underpins the social facilitation effect. One problem with investigating arousal is that it lacks a universally accepted definition (Mullen, Bryant, & Driskell, 1997). It is operationally defined by the way that it is measured, for instance: self-reports (e.g., state anxiety scales) and physiological measures (e.g., palmar sweat, heart rate, and *electrodermal* response, a measure of the electro-conductivity of the palmar skin, an indirect measure of palmar sweating). The high correlations between such scales imply that they tap into the underlying construct of arousal. We will consider evidence from experiments and a meta-analysis for whether there is an effect of observation, presence, and task complexity on arousal.

Chapman (1974) measured participants' arousal levels under presence and alone conditions. Participants were given a relaxation session prior to testing to stabilise their baseline tension levels. Participants then attended to a short story, and their forehead muscle tension levels were automatically recorded every minute while either in the presence of the experimenter or while alone. The participants' muscle tension was significantly higher in the presence condition than in the alone condition. The participants were not involved in a task, and also reported that they did not feel evaluation apprehension, and so this arousal increase was due to the presence of the experimenter, not to evaluation apprehension. This supports Zajonc's (1965) proposal that presence increases arousal. Mullen, Bryant and Driskell (1997) conducted a meta-analysis of 16 experiments which used self-report measures of arousal, and eight which used electrodermal measures of arousal. These experiments

compared participants' arousal levels between audience presence, co-action, or alone conditions. Both electrodermal measures and self-report measures revealed an overall increase in arousal from presence. This meta-analysis and Chapman's (1974) study support the activation theories in that they found an increase in arousal from proximate observation.

Other studies, however, did not find an overall increase in baseline arousal solely from proximate observation, but only when participants were involved in complex tasks. In one, participants learned a complex motor task either alone, or in the presence of an audience of 10 students (Martens, 1969). Palmar sweat was measured by taking prints of the hands, which were inspected for open sweat pores, at baseline and at intervals throughout the trials. There was no significant difference at baseline between the two groups. During the task, however, the proximately observed participants showed a significant increase in palmar sweat and made significantly more errors compared to the alone participants. This supports Zajonc's (1965) claim that increased arousal interferes with learning a complex task, and demonstrates an arousal effect from proximate observation, both of which support the activation theory. Similarly, two studies found that increased cardiovascular activity only occurred when participants were performing a complex task with proximate observation, but not when alone or when performing a simple task (Gendolla & Richter, 2006; R. A. Wright, Killebrew, & Pimpalpure, 2002). Cardiovascular measures, however, do not respond to easy tasks as low effort is required (Gendolla & Richter, 2006). C.F. Bond and Titus's (1983) meta-analysis also found that audience or co-actor presence increased arousal in complex tasks, but did not significantly affect arousal during simple tasks. Thus, these studies and meta-analysis provide only partial support for the activation theories.

Lastly, there is recent evidence to suggest that audiences increase reactivity to stressors, such as complex tasks, rather than baseline arousal levels. For instance, Cacioppo et al. (1990) measured participants' electrodermal activity while the

participants thought that the experimenter was just calibrating the equipment, and were not involved in any task performance. They compared an alone condition, in which the participant was physically alone in the room, with being knowingly observed by the experimenter via a one-way mirror. The observed participants exhibited increased electrodermal responses to the aural presentation of tones. Participants' baseline arousal was not affected by observation, but observation increased physiological reactivity to the tone stimuli. Likewise, the presence of a friend, in addition to the presence of an experimenter, increased physiological reactivity to stressful complex tasks compared to the presence of the experimenter only (Allen, Blascovich, Tomaka, & Kelsey, 1990). Thus, there is evidence that presence and observation increase activation, but that this relationship is not straightforward. However, this might be, in part, due to individual differences in how people react to arousal (e.g., Bates & Rock) and the different ways that the various measures of arousal respond to proximate observation (Aiello & Douthitt, 2001). For example, electrodermal activity and palmar sweating show greater activation when there is an audience present during task performance, but there is no consistent pattern for heart rate and muscle tension (C. F. Bond & Titus, 1983; Geen & Gange, 1977; Guerin, 1986).

The optimal arousal curve, or inverted U hypothesis, can be used to show how activation can have both facilitatory and inhibitory effects (see Figure 3).

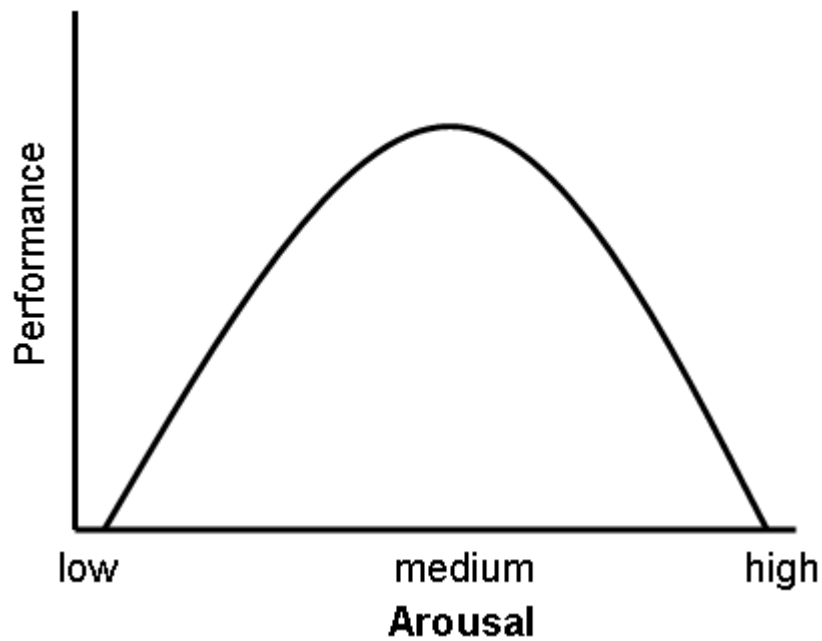


Figure 3: The Optimal Arousal Curve, from http://en.wikipedia.org/wiki/Yerkes-Dodson_law

The optimal arousal curve can also be considered to have activation along the x axis. Prior to any social facilitation research occurring, it had been assumed that increased activation or arousal would improve performance in a linear relationship (Guerin, 1993). The relationship was found instead to be curvilinear, with performance highest at medium arousal and lowest at either extreme: the Yerkes-Dodson law (Yerkes & Dodson, 1908). Other aspects of the observer, the participant, and the task can be conceptualised as forces that increase activation thus pushing the performance to the right along the x axis. Task complexity is one such aspect, observation is another. These can be combined; for example, a simple task without observation would be at the far left, with low activation and low performance. A complex task with observation would be at the far right with high activation and low performance. Either a simple task with observation, or a complex task without observation, either of which combinations would produce medium activation, would be in the middle

with optimal performance for that task. Influences on behaviour can be conceptualised as either increasing or decreasing activation, and, depending on the baseline level, this will either facilitate or inhibit performance. Up to a point they can be considered additive. Thus the optimal arousal curve provides a model of how different influences might combine to produce activation, and enable performance facilitation or inhibition to be predicted.

One implication of the inverted U hypothesis is that simple task performance would not be infinitely facilitated by observation or other activating influences, but, if activation were to increase beyond the optimal level, performance would be inhibited. There is support for this in the phenomenon of “choking under pressure”, performance decrements under situations of high stress (Baumeister, 1984). Another implication is that all the influences that could affect participants’ behavioural responses should be considered when predicting whether a task should be facilitated or inhibited. These influences might include personality variables, and aspects of the relationship between the observer and the participant.

6.2.4. Social Facilitation and Personality.

There has been remarkably little social facilitation research that also investigated personality (Uziel, 2006). Uziel attributed the paucity of personality and social facilitation research to the focus on task complexity, which directed research attention to aspects of the task, not the participant. However, when an observer is introduced to an experimental situation, a social element is created. Therefore, we should consider participants’ personality traits, because they affect social preferences (Costa & McCrae, 1992b). In Chapter 2, we described the Big Five personality traits (Costa & McCrae, 1992b); we now consider how two of these: extraversion and neuroticism, might impact upon the social facilitation effect. We will also consider the effect of participants’ state anxiety. We shall determine whether these individual differences affect people’s responses to observation, task performance, and arousal.

Regarding the relationship between observation and extraversion, one study compared extraverts' and introverts' performance on a letter deletion task (Colquhoun & Corcoran, 1964). Their task was to delete the letter 'e' in lines of prose, and their speed was measured. Extraverts and introverts were defined by a median split from the Heron personality inventory. Participants were assigned to work alone (although they were observed by the experimenter through an open door) or in co-acting groups with unfamiliar co-participants. There was a significant interaction between personality and group condition: The extraverts performed better than the introverts when in groups, and the introverts performed much better than the extraverts in isolation. Although Colquhoun and Corcoran investigated co-action, not observation, co-action usually has a similar effect to observation on performance. Thus, the indication is that extraverts would perform better when observed and introverts when alone.

A similar outcome was found in a study investigating extraversion and audience effects on a sporting task (Graydon & Murphy, 1995). This experiment aimed to replicate the environment of a sport competition, with either a large, noisy audience or a single, silent scorer. In neither condition were the participants alone and unobserved. Extraverts and introverts were defined as the 10 highest and 10 lowest scoring of 50 participants screened with the Eysenck Personality Inventory (EPI). Their task was to serve table tennis balls into a marked grid, scored for accuracy, and they were all familiar with, but not expert at, table tennis. The interaction between extraversion and audience condition was highly significant. The extraverts performed much better with the large audience, and the introverts performed better with the silent scorer; this shows once more that extraversion is a factor in social facilitation.

In one final study considering extraversion and social facilitation, extraverts' and introverts' performance was compared on a simple maths task (Grant & Dajee, 2003). Participants were categorised as extravert or introvert based on a median split of their scores on the EPI. Participants were randomly allocated to proximate

observation or alone conditions; they then measured the time taken to complete the maths task themselves. Both personality groups were facilitated by the proximate observation condition, but in contrast to the two studies above, the introverts were facilitated to a much greater degree. Once again, there was a significant interaction between personality and observation condition, but this time in the opposite direction.

The relationship between extraversion and observation was considered in a meta-analysis, which concluded that extraverts' task performance tends to be facilitated by proximate observation (Uziel, 2007). Uziel also concluded that introverts' task performance is inhibited by proximate observation, irrespective of task complexity. However, extraversion might also interact with task complexity. Extraverts performed faster and more accurately on a variety of complex, cognitive tasks and introverts performed slowly, but more accurately (Eysenck & Eysenck, 1985). In sum, the prior literature reviewed above suggests that extraversion interacts with observation and task complexity, but that this relationship is not straightforward.

In the following section, we consider how arousal might play a role in the relationship between extraversion and task performance. If extraversion is characterised by lower cortical arousal than introversion (Eysenck, 1967a), then, according to the optimal arousal curve, we would expect extroverts to perform better under conditions of higher arousal. This expectation was corroborated by an experiment that compared extraverts' and introverts' performance on an IQ task under one of five different arousal levels (Bates & Rock, 2004). Arousal was manipulated by sound: from silence, through different volumes of white noise, to screams. There was a significant interaction between arousal level and extraversion. The extraverts performed increasingly better the higher the arousal level, and the reverse was found for the introverts. Other research, however, suggested that extraverts and introverts might have qualitatively different responses to the same physiologically measured arousal states (Matthews & Amelang, 1993). Matthews

and Amelang suggested there may be a positive association between arousal and performance in extroverts, but a negative association between arousal and performance in introverts.

If the social facilitation effect is mediated by arousal, then, according to the optimal arousal curve, we would expect extroverts to perform better at complex tasks or while observed. The effect of combinations of all these factors might be very difficult to predict. Nevertheless, the evidence reviewed above indicates that extraversion should be considered in social facilitation research, especially as the performance effects of extraversion and introversion are often in the opposite direction to each other. Therefore, omitting considering extraversion risks missing an informative finding in an overall null effect (Eysenck, 1967a).

The other personality characteristics that will be considered in relation to the social facilitation effect are neuroticism and anxiety. Neuroticism is the personality trait of being prone to negative states (Costa & McCrae, 1992a, p. 14). Anxiety can be subdivided into state anxiety and trait anxiety. State anxiety is present-moment subjective feelings of anxiety, and trait anxiety is the propensity to become state anxious (Matthews & Deary, 1998; Spielberger, 1966). Trait anxiety and neuroticism are very highly correlated (Spielberger, 1983), and state anxiety has been found to be more detrimental to performance than trait anxiety (Matthews & Deary, 1998). All three of these personality characteristics are associated with increased autonomic nervous system arousal (Spielberger, 1983). Below, we review studies that considered the relationship between anxiety and task complexity, and anxiety and observation.

Anxiety has been found to improve performance on simple tasks, but to impair performance on complex tasks (Matthews & Deary, 1998; Spence, Farber, & McFann, 1956; Spence, Taylor, & Ketchel, 1956). For example, the effect of anxiety on simple and complex task performance was investigated in a series of three

experiments (Spence, Farber et al., 1956; Spence, Taylor et al., 1956). Participants were drawn from the lowest and highest scorers on the Manifest Anxiety Scale. Their task was to learn pairs of words, so that when presented with the first word they could respond with the second word of the pair. Some paired associate words had a strong semantic relation to each other (e.g., adept-skilful), these word pairs were the simple task. Other paired associate words had strong semantic links to other words on the list; these were the complex task because participants' dominant responses would have been the incorrect answers. Participants with high anxiety levels outperformed participants with low anxiety on the simple task. Participants with low anxiety outperformed participants with high anxiety on the complex task. These experiments demonstrated therefore that high anxiety has a similar effect on performance as observation: They both facilitate the performance of simple tasks and inhibit the performance of complex tasks. This is the pattern predicted by the optimal arousal curve. Similarly, Carron (1965, as cited in Bargh & J. L. Cohen 1978) found an interaction between trait anxiety and task difficulty: Participants with high trait anxiety performed better than those with low trait anxiety on a simple motor task, and this pattern was reversed on a difficult motor task. These findings indicate that social facilitation research should consider participants' anxiety levels.

The following experiments investigated the effects of anxiety and observation on task performance. In the first of these experiments, participants learned a series of nonsense words under either alone or observed conditions (Ganzer, 1968). As participants learned the words, they tried to anticipate the next word in the series, and the number of accurate guesses was recorded. In the observed condition they were told that there would be an observer behind a one-way mirror, but there actually was no observer. (In pilot tests this "deception of observation" condition had proven to be a similar influence to actual observation through a one-way mirror.) In both conditions the experimenter was hidden from sight, but in the same room as the participant, and spoke to the participant to conduct the experiment. There was neither a genuinely alone, nor a genuinely observed condition, in this experiment.

Participants were classified as having low, medium, or high anxiety on the Test Anxiety Scale, and were randomly assigned to the observed or alone conditions. The alone participants made a significantly greater number of accurate guesses than the observed participants. There was also a significant effect of anxiety. The high-anxiety participants made fewer accurate guesses than the medium- and low-anxiety participants. There was also a significant interaction between observation and anxiety: The high-anxiety participants who were observed made the fewest accurate guesses. Observation and anxiety were both detrimental for this complex task, thus demonstrating that anxiety can interact with the social facilitation effect.

Also investigating anxiety and observation conditions on performance, Cox (1968) conducted a series of experiments comparing children's speed on a simple motor task. High and low anxiety children were selected from the highest 25% and lowest 25% of large populations screened with the Test Anxiety Scale. Their task was to drop marbles into a box, and the rate of marbles per minute was timed. They were randomly assigned to one of the following observation conditions. In the control condition only the experimenter was present. In the observation conditions, the children began this task in the presence of the experimenter only, and after a minute another observer (their teacher, their father, or a stranger) entered the room and watched them. There was a significant interaction between observation condition and anxiety. The additional observers inhibited the high-anxious children's performance, and facilitated the low-anxious children's performance compared to those in the control condition. Thus, once more, high anxiety and observation were detrimental to task performance.

A more recent study compared the effects of anxiety and observation on participants' performance of a complex anagram task (Geen, 1985). Participants were selected from the highest 15% and lowest 15% of 350 students screened on the Sarason Test Anxiety Scale. Participants were told that the anagram task was a test of IQ, and were allowed considerably less time to complete it than it required, putting them

under pressure. Participants were randomly assigned to one of three observation conditions. These were either: alone; passive observation, in which the experimenter observed the participant, but not their progress on the anagram tasks; or evaluative observation, in which the experimenter observed the participant and their progress on the anagram tasks. Participants also completed the State-Trait Anxiety Inventory (STAI) twice: immediately prior to starting the anagram tasks and as soon as the anagram tasks were over. There was a main effect of anxiety: High test-anxiety participants attempted fewer anagrams. There was also a significant interaction between anxiety and observation condition. Within the high test-anxious participants, fewer anagrams were attempted by the evaluated participants than by the passively observed participants or the alone participants. There was no effect of observation condition on low test-anxious participants' performance. Participants' change in state anxiety from before the experiment, to immediately after, showed a similar pattern to the performance scores. The highest increase in state anxiety was in the high test-anxious participants with evaluative observation. Geen attributed the performance inhibition and the increase in state anxiety to the effect of evaluation apprehension. As state anxiety scores are a self-report measure for arousal, this study also supports the activation theories of social facilitation.

A further study investigated the effects of anxiety and co-action on both learning (complex) and performance (simple) tasks (Pederson, 1970). Participants were classified as having low, medium, or high anxiety on the Test Anxiety Scale. Participants were randomly assigned to either being alone or being in groups with four other co-actors. The learning task was a paired-associates task, in which participants learned to respond with the second of a pair of unrelated words (e.g., stimulus: calendar – response: shoe). There were no main effects of anxiety or group condition, but there was a significant interaction: High-anxiety participants in co-acting groups made more omissions on the paired-associates task. The performance tasks were letter deletion and multiplication. Again, there was a significant interaction between anxiety and group condition; the low-anxiety participants

performed better in groups, and the high-anxiety participants better alone. Once again, these are the findings predicted by the optimal arousal curve, and they support the activation theories of social facilitation.

In the four experiments reviewed above, observation inhibited task performance in high-anxiety participants. This pattern was also found in a meta-analysis of the 10 social facilitation studies that considered neuroticism (Uziel, 2007). It was also suggested as far back as 1898, by Triplett, who suggested that levels of nervous excitement differentiated participants who performed simple motor tasks better or worse with a competitor. Thus, anxiety can interact with presence and observation, and with task complexity, and is therefore an important consideration in social facilitation research.

6.2.5. Observer-Participant Relationship.

There are two aspects of the relationship between the observer and participant that we will consider in Experiments 3 – 5. The first is how familiar they are with each other. If unfamiliar observers pose a risk of threat or danger (Guerin, 1983; 1993; Guerin & Innes, 1982), or create an uncertain social situation which people are predisposed to monitor for threat (Zajonc, 1980), unfamiliar observers would be likely to exert a greater activating effect. Support for this hypothesis was found in a meta-analysis of 23 studies: Unfamiliar observers increased arousal levels in participants performing complex tasks (C. F. Bond & Titus, 1983). There was no difference in arousal for familiar or unfamiliar observers during simple tasks. In support of *affiliation* theory, that people have positive and supportive interpersonal bonds (Schachter, 1959), familiar others had a slight calming influence on participants performing complex tasks (C. F. Bond & Titus, 1983). Familiar observers exerted less of a performance effect on simple and complex task performance, irrespective of whether speed or accuracy measures were taken (C. F. Bond & Titus, 1983). Guerin (1993) also reviewed considerable findings relating to

animal behaviour in which animals responded very differently to familiar and unfamiliar members of the same species. Therefore, unfamiliar observers are more likely to elicit the social facilitation effect.

The second aspect of the relationship between the participant and the observer that we will consider is whether the observer has *peer* or *expert* status to the participant. This concept is closely linked to the evaluative potential of the observer, and the following experiment illustrates the social facilitation effect from both evaluation apprehension and an expert audience. Henchy and Glass (1968) trained student participants to recognise and pronounce nonsense words. They trained with some words many times; these words became familiar to the participants, and were deemed to have acquired correct dominant responses; these comprised the simple task. They trained with other words less frequently; these comprised the complex task. In the testing phase the nonsense words, and blank stimuli comprised of the words with lines obscuring the letters: *pseudo-recognition* trials, were presented very rapidly with a tachistoscope, just at the threshold of awareness. Participants were aware that a word had been presented, but not which word it was, and guessed vocally which word they had seen. Proximate observation had previously been found to increase the proportion of the frequently trained dominant response words on the pseudo-recognition trials (Cottrell et al., 1968; Henchy & Glass, 1968; Zajonc & Sales, 1966). Henchy and Glass (1968) compared participants' performance between four observation conditions: alone, peer audience, expert audience, and future evaluation. Participants in the alone condition were physically alone in the room (although they knew that the experimenter could hear them). The peer audience comprised two confederates, introduced as being students who were interested in watching an experiment. The expert audience comprised two confederates, introduced as being professors who were specialists in this kind of experiment. In the future evaluation condition the participants were led to believe that they were being filmed, and that the film would be analysed by expert professors. The future evaluation and expert audience conditions produced the social facilitation effect of increased dominant

responses on the pseudo-recognition task, presumably by inducing more evaluation apprehension. Similarly, Wright, Killebrew and Pimpalpure (2002) found that only an evaluative expert audience increased cardiovascular activity during task performance. Likewise, participant's performance on a pursuit-rotor task was most facilitated by being observed by a person with expert status (F. G. Miller et al., 1979). Thus, audiences that the participants believe to be experts lead to a greater social facilitation effect. Therefore, to best elicit a social facilitation effect observers who are both unfamiliar and expert should be used.

6.2.6. Evidence that “Mere Observation” might elicit the Social Facilitation Effect.

Zajonc's (1965, 1980) proposal that presence was necessary and sufficient for the social facilitation effect led to research that focussed primarily on the effect of audience presence. Audience presence frequently included observation (proximate observation). As reviewed above, proximate observation does lead to the social facilitation effect. The influence of observation has rarely been considered separately from presence and evaluation apprehension. It is important for this review to consider whether there might be a social facilitation effect from observation without presence or evaluation apprehension, because that would strengthen the claim that there might be a social facilitation effect from psi-mediated remote observation. We will call observation without presence *mere observation*. This section reviews previous research that casts light on whether there might be a social facilitation effect from mere observation, although none of these previous experiments considered the influence of remote observation that one can only detect, or be affected by, through psi. The first body of evidence for a social facilitation effect from mere observation is from experiments in which participants are physically alone, but knowingly observed via videoconferencing media (e.g., Bradner & Mark, 2001). The second group of experiments reviewed below compared proximate observation to a presence condition with no observation (e.g., Cottrell et al., 1968). Presence without

observation will, for the purposes of this review, be termed *mere presence*. The term mere presence is, however, more loosely defined in the wider social facilitation literature; it broadly means that the present person is not engaged in any activity (Zajonc, 1965); this has included conditions in which the merely present person is watching the participant (e.g., Huguet et al., 1999). Lastly, studies that directly compared presence and observation orthogonally (Geen, 1973), and observation and evaluation apprehension orthogonally (Geen, 1974) are considered, to disentangle the effects of presence, observation, and evaluation apprehension.

Bradner and Mark (2001) randomly assigned participants to perform a complex maths task with either: computer monitoring, in which the observer can see the participant's real-time progress on a remote screen; or a two-way video link, in which the participant and observer see real-time images of each other. In both conditions the participants were physically alone. All participants also did the task alone without observation. Performance was significantly slowed by both forms of mere observation, and there was no significant difference between the two-way video and computer monitoring conditions. Bradner and Mark ran a follow-up study in which participants did the same maths task with either computer monitoring or a one-way video link, in which the observer watches the participant's real-time image on a screen, but the participant cannot see an image of the observer. Once again, performance was significantly slowed by both forms of observation, and there was no significant difference between them. These two experiments provide evidence that mere observation can cause the social facilitation effect.

Other studies also investigated whether there are social facilitation effects from mere observation from computer monitoring (Aiello, 1993; Aiello & Douthitt, 2001). Aiello and Shao (1993) reported the outcome of four experiments in which participants performed simple tasks while either unobserved or with computer monitoring. One of these simple tasks was a letter deletion task, in which participants deleted certain letters in arrays of multiple letters. In the other three experiments

participants did simple data entry tasks. In all these experiments, computer monitoring improved simple task performance. Aiello and Shao also ran two experiments with complex anagram tasks. In both experiments, participants' task performance was impaired by computer monitoring. Aiello and Kolb (1995) found that computer monitoring increased the speed at which participants performed a simple data entry task. Douthitt and Aiello (2000) randomly allocated participants to being computer monitored, or not monitored, to perform a complex time-card problem-solving task designed to simulate a genuine office assignment. Correcting for participants' baseline skill levels, the participants were significantly slower with computer monitoring. In all, these experiments found that computer monitoring, a form of mere observation, leads to both simple task facilitation and complex task inhibition.

In none of the experiments discussed above was any comparison made between the effects of proximate observation and mere observation. Aiello and Svec (1993), however, compared proximate observation to computer monitored conditions. All of the participants were shown the computer monitoring network before being randomly allocated to one of the following groups. The *not-monitored* participants were told that they would not be monitored. The *no-instructions* participants were monitored, but were only given instructions relating to the performance task and were not informed about their monitoring status. The *told-monitored* participants were monitored, and told that they would be monitored. There was also a proximate observation condition in which a female observer, not the experimenter, watched the participant while standing just behind them, facing in the direction of the computer screen. All participants completed a complex anagram solving task. The *not-monitored* participants made the least errors, followed by the *no-instructions* participants. The *told-monitored* and proximately observed participants made the most errors, and there was no significant difference in the error rates between these groups. This indicates that mere observation can have as large a social facilitation effect as proximate observation, when participants are made aware that they are

being monitored. A similar effect was found in another experiment with computer monitoring, in which being told about monitoring facilitated participants' performance of a simple data entry task compared to being monitored without being told (Nebeker & Tatum, 1993). In all the above experiments, there is support for mere observation eliciting the social facilitation effect. However, there is also the possibility that evaluation apprehension led to the social facilitation effect, as all the participants were aware that they were being observed. The effects of mere observation and evaluation apprehension are impossible to tease apart in these designs.

The second group of studies that indicate that mere observation might cause a social facilitation effect compared both proximate observation and mere presence (presence without observation) to an alone condition (Cottrell et al., 1968; Markus, 1978; Schmitt, Gilovich, Goore, & Joseph, 1986). Any difference found between the effect of proximate observation and mere presence could be due to the influence of observation in and of itself.

Cottrell et al. (1968) compared participants' performance on a pseudo-recognition task in one of three conditions: alone, mere presence, and proximate observation. Although participants in the alone condition were alone in the test room, they knew that their answers could be heard by the experimenter. In the mere presence condition, two blindfolded confederates, who remained silent throughout the experiment but who were able to hear the participant's responses, were in the testing room. The confederates did not express any interest in the participant; they were there under the pretext of being involved in a later study. In the proximate observation condition, two confederates, also ostensibly waiting for a later experiment, were in the room with the participant and were openly interested in watching the participant. To recap the pseudo-recognition task, participants first received differential training in pronouncing nonsense words, until some of these words become the dominant response. Participants then guessed which of these

words were tachistoscopically presented when, in reality, blank stimuli were presented. As the responses were spoken, and the participant was listened to in all the conditions, including the alone condition, there was a possibility for evaluation apprehension in all the observation conditions. The risk of evaluation apprehension was, however, similar across all the conditions, and so should not have confounded the differences between the observation conditions. Participants with proximate observation produced more of the frequently trained responses on the pseudo-recognition trials than did those in the alone or mere presence conditions. This experiment therefore demonstrated a social facilitation effect from proximate observation, but not from mere presence. If the risk of evaluation apprehension was the same across all the observation conditions, this effect could have been due to the influence of mere observation.

Two further experiments compared mere presence to proximate observation (Markus, 1978; Schmitt et al., 1986). Here we briefly outline the aspects of this experiment that relate to the possibility of a social facilitation effect from mere observation. The methodology of these two experiments is considered in greater detail below (see Section 6.4.2). Markus (1978) aimed to reduce the risk of evaluation apprehension in her experiment by using a non-evaluative task that the participants thought was part of the preparation for a subsequent experiment. Participants put on and removed familiar clothing (the simple task) and unfamiliar clothing (the complex task) in either alone, mere presence, or proximate observation conditions. Participants were timed by a confederate watching through a gap in curtains that covered a one-way mirror. There was an interaction between task complexity and observation condition. Participants who were proximately observed were faster at changing familiar clothing than the participants who were in the mere presence condition, who were in turn faster than the alone participants. This pattern was reversed for the unfamiliar clothing (see Table 6). There was therefore a greater social facilitation effect in the proximate observation condition than there was from mere presence, and a greater social facilitation effect from mere presence compared

to alone. These results support the hypothesis that mere observation can lead to the social facilitation effect, because of the steps taken to reduce evaluation apprehension. The task of donning unfamiliar and ill-fitting clothes was, however, criticised by Schmitt et al., (1986) because it might have caused participants anxiety over their appearance, which might exert a similar activating effect as evaluation apprehension.

This limitation of Markus's (1978) experiment was addressed by an experiment that used a covert task that would neither cause embarrassment nor elicit evaluation apprehension (Schmitt et al., 1986). Participants performed simple and complex typing tasks that were presented as gathering background information prior to the experiment, and were automatically timed by the computer they were typing on. Participants were randomly assigned to alone, mere presence, or proximate observation conditions. In the alone condition the participant was physically alone and neither listened to nor covertly observed. In the mere presence condition there was a confederate, wearing headphones and a blindfold, in the room with the participant. The participants were told that the confederate preparing for a subsequent sensory deprivation task in the experiment. In the proximate observation condition the experimenter stood behind the participant watching over his or her shoulder. There was a significant overall interaction between task complexity and observation condition. Participants typed the simple typing task faster in the proximate observation condition than in the mere presence condition, and faster in the mere presence condition than alone. The complex typing task was, however, most inhibited by mere presence, then proximate observation, compared to being alone (see Table 6). The authors theorised that the mere presence condition might have led to participants worrying about their later sensory deprivation, which might have produced an activation effect. In the simple task at least, this experiment supports the idea that there is a greater social facilitation effect from proximate observation than from mere presence, and, given the safeguards against evaluation apprehension, this is arguably due to the influence of mere observation.

Table 6: Mean Time in seconds for Task Completion in Markus (1978) and Schmitt et al. (1986) by Observation Condition

Study	Task	Alone (A)	Mere Presence (M)	Proximate Observation (P)	(A-M)	(M-P)	(A-P)
Markus (1978)	Simple	16.46	13.49	11.70	2.97	1.79	4.76
	Complex	28.85	32.73	33.94	-3.88	-1.21	-5.09
Schmitt et al. (1986)	Simple	14.77	9.83	7.07	4.94	2.76	7.70
	Complex	52.41	72.57	62.52	-20.16	10.05	-10.11

In Table 6 negative time intervals mean that the task was performed relatively slowly, i.e., it was inhibited. The social facilitation effect from mere presence is demonstrated by the column headed A-M. The effect from proximate observation is demonstrated by the column headed A-P. Both of these effects are all in the expected directions. The effect from mere observation is demonstrated by the column headed M-P: the difference between proximate observation and mere presence. These two studies that compared proximate observation and mere presence provide evidence that mere observation is an influence that can lead to the social facilitation effect, although it might be a weaker effect than mere presence.

The following experiment tested the assertion that mere presence can elicit a social facilitation effect by orthogonally comparing presence and observation (Geen, 1973). This experiment differs from those previously presented in that it did not compare simple and complex task performance, but instead used a memory task that is affected by arousal. Pairs of nonsense syllables and single digits: *paired associates*, learned in a state of high arousal are recalled better after a long interval such as 45 minutes than a short interval such as two minutes, and vice versa for paired-associates learned with low arousal (Kleinsmith & Kaplan, 1964). This experiment aimed to replicate this memory effect with observation and presence. Participants were randomly assigned to one of these four observation conditions: alone, mere

presence, proximate observation, and mere observation. In the alone condition the experimenter was listening from the outside the room, a possible source of evaluation apprehension; the participant was, however, alone in the room. In the mere presence condition the experimenter sat in the same room as the participant, reading papers with her back to the participant. In the proximate observation condition the experimenter watched the participant, standing just behind her, looking over her shoulder. In the observation without presence condition the participant was observed via a camera. This camera was present in all the conditions, but clearly turned off in the other conditions. There was an interaction between observation condition and time to recall. The observed participants, both proximately observed and merely observed, were better at recall after a longer duration than a shorter duration. The non-observed participants showed the opposite pattern. There was no effect of the experimenter's mere presence, but this might have been due to there being evaluative influences in the alone condition. Nonetheless, these findings indicate that mere observation is a more powerful influence than mere presence, and support the activation theories, because observation and high arousal both led to a similar recall pattern.. However, the paired-associates task had clear success and failure criteria, and so might have provoked evaluation apprehension, especially when the participants were informed that they were being observed via the camera. Therefore, in this experiment, similarly to those above (Aiello, 1993; Aiello & Douthitt, 2001), it is not possible to distinguish whether mere observation or evaluation apprehension influenced the participants' performance.

Consequently, a further experiment was run to investigate observation and evaluation apprehension separately (Geen, 1974). The same paired-associates task was used as in Geen (1973). Geen (1974) manipulated evaluation apprehension by either telling participants that good recall was correlated with intelligence (high evaluation apprehension), or that they were testing the list of words for their suitability (low evaluation apprehension). Observation was varied between either a proximate observation condition with the experimenter sitting just to the participant's left and

clearly watching, or a mere presence condition with the experimenter sitting just to the participant's left but facing away and reading papers. The interactions between evaluation and time to recall, and observation and time to recall, were both significant. This experiment revealed an arousal-mediated performance effect from observation that was separate from either presence or evaluation apprehension. Thus, this experiment provides additional, conclusive evidence that mere observation has a social facilitation effect.

6.2.7. Summary of the Social Facilitation Effect Research.

As reviewed above, there is evidence that mere observation, in the form of computer monitoring and videoconferencing, can lead to the social facilitation effect. There is also some evidence, from studies that showed a greater effect from proximate observation than mere presence conditions, that mere observation has a social facilitation effect. To an extent, the effect of mere observation has been separated from evaluation apprehension, but it is possible that any condition in which a participant is knowingly watched could evoke some evaluation apprehension. Therefore, the way to clarify whether observation, without presence or evaluation apprehension, can elicit a social facilitation effect is to include a hidden observer condition, in which participants are not informed that they are being watched. However, if people can be consciously aware of remote observation from a hidden observer through psi, then evaluation apprehension might not be ruled out completely. This would extend the development of the field of social facilitation, which, over the course of time, has investigated competition, co-action, presence, observation, evaluation apprehension, and mere observation, and could now progress by investigating remote observation.

It is important to discover whether there is a social facilitation effect from remote observation by hidden observers, because of the high level of covert surveillance in society at present (e.g., Baker, 2005; Sheldrake, 2001). People are continuously

covertly observed, and if this affects behaviour there might be important implications. As noted above, performance on a driving test was inhibited by the presence of an additional testee (Rosenbloom et al., 2007). If the cameras that monitor speed and safety on roads potentially worsen driving performance that could entail serious consequences. Other implications of a social facilitation effect from remote observation relate to improving experimental control conditions. It is frequently assumed that covert observation that participants are not informed about has no influence on their behaviour (e.g., Markus, 1978); this is common practice in social psychology research (e.g., Lepper & Greene, 1973). The field of social facilitation has done much to challenge previous assumptions that social environments are inconsequential to psychological tests (e.g., Huguet et al.), or that observation via one-way mirrors does not affect behaviour (e.g., Wapner & Alper), but the time is now ripe for an investigation of whether psi-mediated remote observation can also affect behaviour. This review now turns to the parapsychological field of remote observation detection to review the evidence that people can detect or respond to observation through psi.

6.3. Remote Observation Detection

This section reviews evidence that people can detect whether they are being watched, through psi. Remote observation detection is psi-mediated awareness of, or influence from, being looked at by a human. For this to be a psi influence there have to be no ordinary sensory channels by which awareness or influence could be transmitted. For instance, the person who is being observed, the *observee*, must not be able to see or hear that the observer is looking. The term *remote* is used following W. G. Braud to mean “inaccessible through ordinary means” (2003, p. xxvii); the observer need not necessarily be physically distant, but is remote in that there are no conventional sensory pathways to inform the observee that he or she is being watched. Remote observation is also called remote staring (e.g., Wiseman & Schlitz, 1997) and unseen gaze (e.g., W. G. Braud, Shafer, & Andrews, 1990; 1993a), see (Sheldrake, 2005a)

for a more complete discussion of this terminology. The rates of belief in remote observation detection are high, reported to vary from 68% to 94% of various populations surveyed (Coover, 1913; Sheldrake, 2003; W. G. Braud et al., 1993a; Thalbourne & Evans, 1992). Sheldrake (2003) reported many examples of spontaneous remote observation detection experiences, which typically involve the observee feeling as though he or she were being stared at, in the absence of sensory information, and this transpiring to be the case. While spontaneous reports and the high prevalence of belief in remote observation detection are not proof that these experiences are genuinely psychic, remote observation detection warrants experimental investigation to determine whether it is a genuine psi phenomenon. This can be achieved by experiments that test whether remote observation can be detected under conditions that eliminate normal sensory awareness of being observed, and logical inference. We review these studies below, starting with a brief outline of the earliest studies, and other studies with the observer and observee in the same room. Secondly, we review studies in which the observer and observee were physically and sensorially separated (e.g., in separate rooms with observation enabled by video links). In this, we first consider those studies with conscious guess measures. Then we consider those studies with unconscious psychophysiological measures of remote observation detection, which have also been the subject of two meta-analyses (Schlitz & W. G. Braud, 1997; Schmidt, Schneider, Utts, & Walach, 2004). We review evidence from these experiments and meta-analyses to assess whether remote observation can be consciously and unconsciously detected. Lastly, we critique the exiguous remote observation experiments that used behavioural measures of remote observation.

6.3.1. Remote Observation Detection Studies with Conscious Guess Measures and the Observer and Observee in the Same Room.

This section reviews studies that used the following basic procedure. An observer and an observee sat in the same room, with the observee unable to see the observer (e.g., facing the other way). According to a randomisation method, the observer either looked at the back of the observee's head (*observation trial*) or did not look (*non-observation trial*). The observee consciously guessed whether he or she had been remotely observed. Experimentation in this paradigm began at the end of the 19th century (Titchener, 1898). Based on a series of experiments he conducted, Titchener reported that remote observation detection did not occur. Unfortunately, he provided no further details or results, so it not possible to assess his claim.

Coover (1913) reported results of a total of 1,000 such trials using 10 observees who believed that they could detect remote observation. Observation condition was determined by a real-time die roll. The observer signalled the start and end of each 20-second trial by tapping on a desk with a pencil. Overall, 50.2% of the guesses were correct. These data were originally interpreted as being at chance, but a subsequent re-analysis found that the hit rate in the observation trials considered alone (53.3%) was significantly above chance (Sheldrake, 1998).

Poortman (1959) aimed to improve upon Coover's (1913) procedure by using an observer purportedly gifted in influencing others by observing them, and by increasing the duration of the trials. Poortman, who believed he could detect remote observation, was the observee. They took part in 89 trials of about three minutes each in the observer's house. The observation condition for each trial was determined by the colour of a pre-shuffled deck of cards; the observer drew one card before each trial. Each trial was signalled to start by the observer knocking on the table that she

was sitting at. The observee spoke out loud to end each trial. Overall 59.55% of the guesses were correct, significantly greater than MCE (W. G. Braud et al., 1993a).

These early studies reviewed above show the origins of the remote observation detection paradigm. These experiments had some major limitations. In each of these studies there was no barrier to sensory leakage or sound cues. For example, changes in the sound of the observer's breathing, the volume of the pencil tapping, or knocking could have indicated differences between observation and non-observation to the observee. Some participants reported purposefully listening out for such cues (Coover, 1913). The methods of selecting observation conditions (die rolls and card shuffling) might not have been adequately random, and therefore might have concurred with patterns in observee's guessing. There was also a risk of fraud and error in the observer deciding the observation condition in real time. These experiments therefore do not provide conclusive proof for, or against, remote observation detection. In addition to these early investigations, some modern research has also used experimental set ups with the observer and observee in the same room (e.g., Sheldrake, 1998, 1999, 2000, 2001; Schwartz & Russek, 1999).

Sheldrake has amassed an enormous amount of data on conscious guess detection of remote observation from workshops, volunteer third-party researchers, and experiments in schools (Sheldrake, 1998, 1999, 2000, 2001, 2003, 2005b). Observation trials were usually 10 to 20 seconds long, and the sequences were either randomised in advance, or based on a real time die roll or coin toss. Some of them were cued to start by the observer or an experimenter speaking, or by a mechanical clicker. Many of the experiments involved the observer and observee receiving trial-by-trial feedback. Feedback, information regarding whether the observee's guess was correct, can be given to the observer, the observee, or to both. One risk in giving feedback to the observee is that they will, intentionally or unintentionally, learn patterns in non-random observation sequences, creating artefacts. The other risk in providing feedback is that observees might come to associate subtle cues from

sensory leakage with a particular observation condition. Any sensory leakage might convey information about the observation condition, but combined with trial-by-trial feedback this is a greater problem.

Sheldrake investigated remote observation detection in considerable detail, breaking empirical data down by whether hits occurred in the observation trials or the non-observation trials. The overall pattern, which is upheld in all but two of the experiments, is that observees detected remote observation around 5% above MCE, a hit rate of around 55%, but did not detect non-observation significantly differently from MCE. Sheldrake (1998, 1999, 2000, 2001, 2005b) noted that response biases, such as responding *observed* more often than *non-observed*, that would lead to hits in the observation condition would also lead to misses in the non-observation condition. This bias to answer *yes*, or *observed*, more often than *no* does exist (Sheldrake, 2005b), but it cannot be the only explanatory factor because the corresponding misses in the non-observation condition were not found. Cheating and cueing would likewise lead to increased hits in the observation and non-observation trials alike, so are unlikely to be the sole explanation, although a combination of response bias and cheating might be. Sheldrake suggested that the tendency to detect observation better than detecting non-observation might be because observation is a stimulus, and it is easier to detect a stimulus than an absence of stimuli. The consistency of this pattern, of significant remote observation detection solely in the observation trials, across many different experiments strengthens the case that this effect is not artefactual.

The large size of this dataset enabled Sheldrake to test whether potential artefacts had inflated remote observation detection by comparing the results of experiments with different levels of control against sensory leakage. Sheldrake (1999) compared the hit rates of experiments that used a sound created by the observer, such as a tap on the table, to those which used a mechanical clicker to start the observation trials. If there were information about the observation condition being conveyed in the volume of the tapping then the hit rates might be greater than the experiments with

mechanical clickers. The hit rates were almost identical, and the differences between them were non-significant. This argues against the risk that in any comparable experiment, such as Poortman (1959), there was an artefactual effect of observation detection caused by tapping volume. Sheldrake (2001) also investigated the possibility that the observees could see the observers, by varying blindfold wearing within-subjects, and found no significant difference in the hit rates. The randomised sequences that Sheldrake used were criticised by Colwell, Shroder and Sladen (2000), who argued that they were non-random, and therefore contained a pattern that the observees could learn implicitly from the feedback they received. Sheldrake (2001) tested the effect of feedback by varying this within-subjects, and found that it made no significant difference to the hit rates. The implicit learning of the observation sequence hypothesis is also countered by the positive findings, with the same hit rate pattern, in previous experiments without feedback (Sheldrake, 2000, 2001), indicating a genuine ability to detect remote observation.

The pattern of significant hit rates in the observation condition and chance hit rates in the non-observation condition was simulated by a computer program (Lobach & Bierman, 2004). By combining a guessing strategy based on feedback depending on the number of times the observee has been observed, and the response bias to answer 'yes' (or 'observed') 55% of the time, the same pattern was generated. However, this does not explain the success in the experiments without feedback. In summary, the majority of the Sheldrake experiments did not adequately rule out artefacts, but there are indications that the positive findings were not due to inadequate randomisation, implicit learning, or cueing. Additional support for Sheldrake's (1998, 1999, 2000, 2001, 2003, 2005) findings come from a pilot study which aimed to replicate Sheldrake's experimental design, while ensuring the randomisation of the observation conditions and including better controls against sensory leakage (Radin, 2004b).

The final three studies to be reviewed in this section incorporated comparisons between observation, imagined observation, and the body part being observed (Schwartz & Russek, 1999). The first experiment compared observees' ability to detect observation of either the head or the back. Each observer/observee pair was unique. According to a counterbalanced sequence, the observer either looked at the observee's head or back. Trials started with the observer saying *ready* and ended with the observee guessing *head* or *back*. Trials took a few seconds, and there was a 10-second break between trials. The proportion of correct answers was significantly greater than the proportion incorrect. Schwartz and Russek were interested in whether an emission of "biophysical energy" or the observer's intention had led to detection.

Schwartz and Russek (1999) ran another two experiments comparing the detection of observation of the head or lower back, and the *imagined observation* of the head or lower back. In the imagined observation condition the observer kept his or her eyes closed and imagined observing the head or back of their observee. Otherwise the procedure was the same as for the first experiment. Schwartz and Russek argued that imagined observation would test only the effect of the observer's intention to observe, and not any effect from biophysical energy emitted during actual observation. The imagined observation condition did not, however, rule out involuntary changes in the observer's breathing or tone of voice that might act as cues to the observee. This second experiment found a marginally significant greater proportion of correct guesses overall. This same procedure was replicated in a third experiment, which aimed to improve upon the second experiment by increasing the sample size and number of trials to increase the power. This experiment found a highly significant detection rate. There was no difference between the hit rates for actual and imagined observation; these were detected equally well. In these experiments there was a risk of sensory leakage and cueing. The counterbalancing of the observation condition sequences risked the observees also counterbalancing their own guesses and possibly increasing their scores.

If, however, the observation detection effects above were not due to cues, nor to the order of the observation conditions, this experiment shows that imagined observation is as detectable as actual observation. This implies that the observees might be detecting the observer's intention to observe, or attention on the observee, rather than the act of being seen. Schwartz and Russek (1999) noted that this could explain why some sceptical observers do not get significant observation detection effects: The intention that accompanies observation might be crucial. It might be the case that the effect of observation is entirely due to attention, but that looking at the observee or the observee's image on a screen focuses the observer's attention on the observee. The wider implications of an effect from attention or intention affect all remote observation experiments and, indeed, all psychology experiments. If an observer focuses their intention or attention on the observee when they are not observing, a similar effect to observation might occur. This could severely compromise the non-observation condition. If intention or attention has an effect then any experimenter could be affecting the participants in any psychology experiment.

In summary, the early research into remote observation detection, and other experiments with the observer and observee in the same room, predominantly found that observees could detect remote observation more often than one would expect by chance alone. In these studies the observation influence that is tested is direct gaze. This is similar to the spontaneous experience model, in which there is a direct line of sight, and it therefore has ecological validity. In these experiments there was a large risk of sensory leakage and unintentional cueing. The positive results found might therefore be artefacts of poor experimental control rather than genuine psi effects, and it is not possible to conclude that there is a psi-mediated remote observation detection effect from these studies. Risks of cues, sensory leakage, and artefacts of poor randomisation have to be eliminated to claim that an apparent remote observation detection effect is due to psi. Before reviewing studies with better controls against sensory leakage, we will consider participants' self-reports of the feeling of remote observation detection, which were reported in these early studies.

6.3.2. The Feeling of Being Remotely Observed.

A minority of the papers reviewed so far report the observees' feeling of being remotely observed. This is described as being a fairly unpleasant phenomenon "uncanny ...unpleasant tension" (Titchener, 1898, p. 895). Titchener also postulated that an audience that is behind one causes nervousness. Coover (1913) collected reports from his observees, and categorised them as visual images or kinaesthetic sensations. Visual imagery included unbidden images of the observer looking directly at them. The kinaesthetic sensations associated with observation included feelings of: restlessness; discomfort; connection with, or nearness to, the observer; being criticised; and the desire to turn round. There was only one reported feeling of non-observation, and that was the feeling of being alone. The most frequently reported feeling in a large collection of anecdotal accounts of spontaneous remote observation detection is uneasiness, then restlessness (Sheldrake, 2003). Responses to a questionnaire on remote observation detection experiences showed that 78% students in a parapsychology class in Australia experienced a strong emotional reaction to being stared at, such as anxiety, sexual arousal, fear, or anger and 56% experienced a strong physical reaction, such as a pounding heart or blushing (Thalbourne & Evans, 1992). It seems strange that no positive feelings associated with remote observation detection have been reported from spontaneous cases. It is possible that, just as being observed by most people might be unsettling, there are some with whom there is a positive enough bond to create a positive feeling, like affiliation (Schachter, 1959). This could also be the case remotely; we could, perhaps, call it *remote affiliation*. The reported experiences are, however, mostly uneasiness and nervousness, which might originate from remote observation detection being an evolutionary product of being able to detect predators' gaze.

Lee et al. (2002) found differences in how people conceptualised being stared at depending on personality types. They devised their own questionnaire to measure "intuition" and "social awareness" traits. Those who scored high on intuition felt

watched in public places, which might just be because there are more chances of being seen. Those who scored high on social awareness felt watched when they felt “alone, isolated or vulnerable” (Lee et al., 2002, p. 408). This indicates that people feel watched in environments that cause them unease; this corroborates the spontaneous reports of remote observation being unsettling. It also warns researchers not to allow observees to confuse any uneasy feeling with the feeling of being observed.

6.3.3. Recent Remote Observation Detection Studies with Conscious Guess Measures and Physical Separation of Observer and Observee.

More recent studies into remote observation detection with conscious guess measures are reviewed below. These experiments improved on the methodology of the earlier remote observation detection studies by reducing the risk of sensory leakage and inadvertent cueing. This was achieved by physical separation of the observer and observee, and better techniques of randomising the trial observation conditions.

The earliest remote observation research with better controlled conditions was conducted by Peterson (1978, as cited in W. G. Braud et al., 1993b). Nine observer/observee pairs each took part in four 6-minute experimental sessions. The observer and observee were in separate, adjacent rooms with a one-way mirror between them, which allowed the observer to see the observee, but not vice versa. The risk of noise cues was reduced by the observee listening to white noise through headphones. Prior to the trials there was a training period, in which the observee was given feedback, to help them to learn how it feels to be observed. There was no feedback in the test trials. The observer watched, or did not watch, the observee through the mirror according to a randomised schedule. The observee pushed a button whenever he or she felt observed. The time and number of button presses were recorded, and later compared to whether they had occurred in observation or

non-observation trials. There were significantly more button presses in the observation trials. This experiment therefore supported the detection of remote observation under conditions in which there were very few risks of sensory leakage or cueing.

Williams (1983) investigated remote observation detection along with the effect of belief in psi. The observees were 14 believers in psi and 14 disbelievers, selected from a large group of students screened with a 10-item belief in psi scale. There was very good control against sensory leakage; the observer and observee were in separate rooms 60 feet apart. The observer watched an image of the observee on a monitor relayed from a closed-circuit television link (CCTV). The monitor displayed 12 seconds of the image of the observee, randomly interspersed with 12 seconds of blank screen, to denote the observation and non-observation trials. A tone signalled the start of each trial. The observee pressed a 'yes' button to score trials that he or she thought were observation trials, and made no response for those that he or she thought were non-observation. There was an overall significant positive deviation from chance in remote observation detection, with a large effect size ($r = .31$); this was greater for the believer observees ($r = .49$). The disbelievers considered alone guessed at chance. This study, therefore, demonstrated significant detection of remote observation with randomised observation condition sequences and good controls against sensory leakage and cueing. This supports the hypothesis that remote observation detection is psi-mediated, and adds the finding that believers in psi might be better able to detect remote observation.

Sheldrake (2000) reported the results of some well-controlled experiments that supported remote observation detection. Three experiments were conducted at schools in London that adequately separated the observers and observees. Whole classes of primary school children participated at once, in observer and observee pairs. The observees were outside with their teacher, and they wore blindfolds. The observers were inside the school with Sheldrake, and could see the observees through

the classroom windows, which were closed. They were separated by a distance of between three and 100 metres. Only after the observees were in place did the observers receive their randomised lists of observation sequences, which were unique to each pair, obviating any incentive to copy from others. There were 20 trials of 10 seconds each. Sheldrake told the observers when to start, and made a tone sound outside for the observees. A teacher outside with the observees told them when the trial was over; subsequently, the observees noted their guess on paper. There was no feedback given to either the observers or the observees. The hit rates were significantly above chance overall.

Colwell et al. (2000) investigated the sequences of observation conditions that Sheldrake (1998) had provided to encourage the general public to replicate his studies. Participants, who believed in the ability to detect remote observation, undertook 60 remote observation detection trials without feedback, followed by 180 trials with trial-by-trial feedback. The observer was one of the authors (S. Shroder). Observer and observee were in adjacent rooms with a one-way mirror between them, which allowed the observer to view the observee during the observation trials. The observee's room was not sound-proofed, but the observer took care not to provide noise cues relating to observation. The start and end of each 20-second trial was cued by the words *trial* then *yes or no* displayed on computer monitors, one in each of the two experimental rooms. When feedback was provided, this also appeared on the monitors immediately after the observee registered his or her guess. Overall, participants performed significantly above chance in the observation trials, but not in the non-observation trials. They performed significantly better than chance in the trials with feedback, but not in those without feedback, and there was improvement over the trials. This could have been because the participants were learning to identify the feeling of being remotely observed over the course of the feedback trials. The observation condition sequences were, however, found to be non-random, with too few repetitions of the same condition. The significant finding could therefore also

be because the participants were implicitly learning a pattern to the observation condition sequences.

To test whether inadequate randomisation had led to artefactual findings a second experiment was run using genuinely random sequences from random number tables (Colwell, Schroder, & Sladen, 2000). This followed the same procedure as their first experiment, with the exception that there was a different observer (D. Sladen), and participants did 200 trials each, the first 20 of which had no feedback. There was no overall remote observation detection effect found, neither did participants improve over time. Colwell et al. (2000) attributed the positive effect in the first experiment to participants implicitly learning the sequences. There are, however, other differences between these two experiments which might have caused the discrepancies in the outcomes. These two experiments had different observers. The longer non-feedback condition that preceded the feedback condition in the first experiment could have acted as practice time, improving performance in the feedback condition. Therefore, the order of the observation conditions is not the only possible reason for the different results between these two experiments. However, this study highlights the importance of truly randomised observation sequences.

In all of the experiments considered so far, there has been only one observer and one observee at any one time. A different set-up was devised in which there were concurrently around 13 observers and two observees (Wiseman & Smith, 1994). Participants rotated roles and acted as both observers and observees. The observers and observees were in adjacent rooms, with a one-way mirror allowing the observers to see the observees. The two observees were divided by a screen, and one of the experimenters was also in the same room as them. Each observee pair undertook six trials, each of 20 seconds duration, and rated each trial on a seven-point scale for how observed they felt. There were no significant differences between the participants' ratings in observation and non-observation conditions. This set-up has ecological validity in that it is similar to many real life situations in which there

might be other people present, and more than one observer. There was also fairly good control against sensory leakage, and truly randomised observation condition sequences. One possible reason for the null findings might be because the observees were not alone, and so may not have felt unobserved, even in the non-observation condition.

A further series of experiments found no evidence for remote observation detection (Lobach & Bierman, 2004). Their first experiment compared the effect of the observer's belief in psi. Each observee was observed for half their trials by a believing observer, and for the other half by a sceptical observer. The observer and observee were in adjacent rooms with a one-way mirror in between them. The observer looked at the left side of the observee's face, or looked away and focussed her thoughts on something else, according to a unique, randomised schedule that had been prepared in advance. The trials were 15 seconds long, and were started by a tape recorded voice played in both rooms. Their second experiment used a combination of psychophysiological measures of remote observation detection and conscious guess measures, and will be reviewed fully in the section below on psychophysiological responses. In a brief outline of the conscious guess aspects of this second experiment, the observer watched the observee through a CCTV link from an adjacent room, or did not watch, for twenty 30-second trials according to a randomised schedule. The observee pressed a button to record his or her guess in a pause between each trial. In their third experiment observer and observee pairs who knew each other were compared to unacquainted pairs. Participants were recruited in pairs of friends, and each took the role of observer and observee with their friend, and with a member of another pair whom they did not know. The observer and observee were in adjacent rooms with a one-way mirror between them. The observees rated their guesses by button presses after each 10-second trial. The hit rates in all three experiments were non-significantly different from chance.

The experiments just reviewed with conscious guess as the measure of remote observation detection covered a wide range of different experimental set-ups. They tested selected and unselected participants with different numbers and durations of trials. These studies therefore acted as conceptual replications for each other: They addressed the same question, but with variations in the methodology. The advantages of conceptual replications over exact replications, in which an experiment is duplicated, include reduced risk of fraud from involving many different experimenters and participants, and reduced risk that a positive effect might be due to a hidden artefact in one experiment's methodology or an inaccuracy in one experiment's procedure (Schlitz & W. G. Braud, 1997). These studies' hit rates, the outcome of single means *t* tests comparing hit rate to MCE, and the effect sizes are summarised in Table 7.

Four of the seven studies presented are independently significant. There are two additional non-significant studies (Lobach & Bierman, 2004), for which the data were not available; thus the majority were non-significant. However, there is a greater proportion of significant studies than the five percent that would be expected by chance. Of the significant studies, three had large ($>.3$) effect sizes: Peterson (1978), Williams (1983), and Colwell et al. (2000) and one had a medium ($>.15$) effect size: Sheldrake (2000). These experiments had good controls against sensory leakage and cueing, and so, with the exception of the possible randomisation artefact in Colwell et al.'s (2000) first experiment, there were no known artefacts that would indicate that this effect was not due to psi. The two experiments with the best controls against sensory leakage: Peterson (1978) and Williams (1983), had the highest effect sizes, indicating that remote observation detection is not an artefact of sensory leakage. Two of the experiments with null findings were conducted by sceptics: Colwell et al. (2000) and Wiseman and Smith (1994), indicating that there might be an experimenter effect in this dataset. Overall, there is some evidence that people can detect remote observation.

Table 7: Outcomes of Well-Controlled Remote Observation Detection Studies with Conscious Guess Measures

Study	N observees	<i>t</i>	Significant at <i>p</i> <.05 *	<i>r</i>	Hit rate
Peterson (1978)	9	2.65	*	.41	54.86%
Williams (1983)	28	1.71	*	.31	53.32%
Sheldrake (2000)	155	2.14	*	.17	52.6%
Colwell, Schroder & Sladen (2000) 1 st experiment	12	N/A ^a	*	.46	N/A ^a
Colwell, Schroder & Sladen (2000) 2 nd experiment	12	N/A ^a	NS	.11	N/A ^a
Wiseman and Smith (1994)	65	.93	NS	.12	N/A ^b
Lobach and Bierman (2004) 2 nd experiment	45	.124	NS	.18	52.1%

^aColwell et al. (2000) reported their results in four blocks of trials per experiment. The effect sizes reported are the mean effect size for each experiment based on the results of the blocks. There were no *t* tests or hit rates reported for each experiment as a whole.

^bWiseman and Smith (1994) did not report a hit rate because their observees rated how much they felt observed on a Likert scale. Their results only deviated slightly from what would be expected by chance.

W. G. Braud, Shafer and Andrews (1990, 1993a) reviewed the findings of remote observation detection studies with conscious guess measures prior to 1985. They argued that the results supported remote observation detection, but with small effect sizes. Given that spontaneous reports of observation detection frequently include “unconscious behavioural and bodily changes...rich in physiological content...and automatic movements” (W. G. Braud et al., 1993a, pp. 376-377) and rarely involve higher cognitive functions, W. G. Braud et al. proposed that using a more unconscious measure of nervous system activity might be more sensitive to remote observation, and therefore better at detecting it. Measures of unconscious physiological responses would also be free of cognitive interferences such as guessing strategies and response biases. For example, Coover’s (1913) participants reported a strategy of alternating their *observation* and *non-observation* responses to

maintain an even balance overall. Sheldrake (1999) also argued that the effect sizes were small because remote observation detection is usually unconscious “under the artificial conditions of experiments, people are being asked to do consciously when they may usually do unconsciously. Self-consciousness may interfere with their sensitivity.” (p. 67). In this above quotation, Sheldrake might have meant that the process of remote observation detection is normally nonintentional. Therefore, nonintentional psi measures might increase the chances of finding an effect. However, in the majority of the studies reviewed below, observees knew that they would be remotely observed, just not when. Therefore, these are not truly nonintentional psi studies. If, as argued in Chapter 1, it is the case that psi information is first received unconsciously, and that only part of this information reaches consciousness, then unconscious measures would have greater potential to capture this effect.

6.3.4. Remote Observation Detection Studies with Unconscious Psychophysiological Measures.

In this section, we review remote observation detection studies with psychophysiological dependent variables. The psychophysiological measure that has been used for remote observation detection is electrodermal activity, also called skin conductance (Lobach & Bierman, 2004) or galvanic skin response (Wiseman & Smith, 1994). Electrodermal activity is measured by placing electrodes on the observee’s non-dominant hand. A small electric current is passed through these and conductivity is measured at regular intervals. Sweating increases the conductivity of the hand. Electrodermal activity is an indication of the activity in the sympathetic branch of the autonomic nervous system (sympathetic nervous system arousal). Increased sympathetic nervous system activity is associated with the body’s response to stress, and the fight-flight syndrome. It involves increased palmar sweating, pupil dilation, heart rate, blood pressure, bronchiole dilation, and release of adrenaline (D. U. Silverthorn, Garrison, Johnson, & A. C. Silverthorn, 2009). It is also associated

with feelings of fear, nervousness, and agitation. Decreased sympathetic nervous system response is associated with calming, and with increased activity in the antagonistic branch of the autonomic nervous system, the parasympathetic nervous system (D. U. Silverthorn et al., 2009).

In remote observation studies with electrodermal measures, the observees' skin conductance is measured throughout the observation and non-observation trials. There are different ways used to calculate the effect of observation. The difference in the electrodermal activity scores between the observation and non-observation sessions can be calculated as a ratio and compared to MCE by single means *t* tests (e.g., W. G. Braud et al., 1990); the conductivity scores can be deducted from each other and compared to zero by single means *t* tests (e.g., Wiseman, Smith, Freedman, Wasserman, & Hurst, 1995); or the conductivity scores in the observation and non-observation trials can be compared to each other by paired *t* tests (e.g., Wiseman & Smith, 1994).

What would the effect of remote observation on sympathetic nervous system activity be? The spontaneous reports and the descriptions of participants' feelings in the experiments reviewed above depicted a sensation of uneasiness and nervousness from remote observation. Therefore, the effect is likely to be sympathetic nervous system activation, as this is associated with nervousness and agitation. Some of the remote observation studies that measured electrodermal activity found that remote observation lead to activation, and others found it lead to calming. We review the studies that found activation first, then those that found calming, and then we will examine the studies for possible explanations for this difference.

The first study that found activation from remote observation was also the first study to use a psychophysiological dependent variable (W. G. Braud et al., 1990; 1993a). Sixteen unselected volunteer participants were the observees. The observer was D. Shafer. Observer and observee were located in windowless rooms separated by

several other rooms. There was no risk of sensory leakage. Remote observation was carried out via CCTV. There was a continuously active camera in the observee's room, and a video monitor in the observer's room. The observer first set up the observee with palmar electrodes and informed him or her that he or she would be watched only at random times and just to sit quietly. Then she went to the observation room, and only then opened a randomised, sealed list of observation conditions. Opening these after the contact with the observee is over obviates the risk of inadvertent cueing during the set-up. Each observee had his or her own unique sequence, which ruled out the risk of the sequences accidentally coinciding with popular guessing patterns. There was a significant increase in the activation of the observees' sympathetic nervous systems during remote observation trials. This difference was due to the remote observation, not to the camera, as the camera was running continuously in both the observation and non-observation trials. There was a larger effect size found than the conscious guess studies ($r = .57$), and W. G. Braud et al. (1990, 1993a) concluded that electrodermal activity was a more powerful method of detecting remote observation.

In another study in which remote observation was associated with increased electrodermal activity, the influence of observation was combined with the observers trying to activate the observees (Schlitz & LaBerge, 1994, 1997). This study follows not only from the work of W. G. Braud et al. (1990; 1993a,b) but also from healing analogue studies. In these healing analogue studies, remote influencers aimed to calm or activate the electrodermal responses of a distant participant, not through observation, but through intention and imagery (Schlitz & W. G. Braud, 1997). There was considerable experimental support for the ability to influence a participant's physiology remotely (Schlitz & W. G. Braud, 1997). Thus, in Schlitz and LaBerge (1997), the effect may have been a combined effect of observation and intention. The observer and observee discussed ways that each particular observee might be activated. The observer and observee were taken to separate rooms, 15 metres apart, with a CCTV link to enable remote observation. Blocks of four trials: observation,

non-observation, non-observation, observation, or vice versa (ABBA) were created to balance the observation conditions and control for the possibility of electrodermal activity decreasing over time. The randomised, counterbalanced sequence of observation/non-observation trials was controlled by the computer program. The observer watched the image of the observee or a black screen, and either tried to activate the observee or attended to other things, accordingly. There were thirty-two 30-second trials, interspersed with short breaks of randomised duration. The experimenter was blind to the observation sequences until after concluding all interactions with the observer and observee. There was very good control against artefacts in this experiment. Observees' skin conductance was significantly higher in the observation trials than in the non-observation trials. These findings add some support to the idea that remote observation can lead to electrodermal activation, with the caveat that this effect might have been caused by the intention to activate the observees.

Wiseman and Smith (1994) also found an increase in participants' electrodermal activation from remote observation by a group of observers. In a similar way to their experiment with conscious guess as the dependent variable (Wiseman and Smith 1994), there were two observees at a time, separated by screen. There was a video camera monitoring each observee. This differs from the other studies with psychophysiological measures reviewed here, in that the observees were not alone. Thirteen observers were located on a different floor of the laboratory that had two monitors, each of which displayed an image of one of the observees. Half of the observers were allocated to watch each observee. The observers watched the images, or looked away, according to instructions from the experimenter who followed a pre-prepared, randomised, counterbalanced sequence of observation trials. These had been created by rolling a die to determine random entry into a random number table, then assigning an observation trial followed by a non-observation trial to even numbers, and the reverse for odd numbers. There were six trials per pair of observees, each trial lasted 20 seconds. Participants rotated roles and acted as both

observers and observees. There was a significant difference between the electrodermal activity in the observation and non-observation conditions. The authors further investigated the randomised and paired sequences and found that pairing of observation trials before non-observation trials predominated. This could have caused an artefact if the observees' electrodermal activity reduced over the trials, and further investigation corroborated that this might have occurred. This artefact could also have masked a genuine psi effect if there was one. Using randomised ABBA counterbalanced blocks, as Schlitz and LaBerge (1997) did, overcomes this problem, and these were used by all the following experiments in this review, unless otherwise stated.

Due to the possible randomisation artefact in the previous study, another two studies were run that investigated electrodermal response to remote observation (Wiseman et al., 1995). In both these experiments observees took part individually. There were good controls against sensory leakage; the rooms that the observee and observer were in were in some cases about nine metres apart, and in others the observers were on a different floor and at the opposite side of the building. The randomisation of the observation trials was different in each of the experiments. In the first experiment entry into a random number table was determined by die roll, then the next 10 digits were used to create the sequences. An even number translated to an observation trial followed by a non-observation trial; an odd number, vice versa. For each sequence created from the random number tables a counterbalanced sheet was created, in which the exact reverse observation conditions were listed. In the second experiment the randomised ABBA sequences were used. There were 10 observation and 10 non-observation 30-second trials, with 30-second rest periods in between, for each observee. In both experiments there was no significant activation effect from remote observation. The non-significant findings in these experiments and in Wiseman and Smith's (1994) experiments might have been due to an experimenter effect.

A striking experimenter effect was found in a study that also found electrodermal activation from remote observation (Wiseman & Schlitz, 1997). This study set out to compare the effects of a sceptical observer (R. Wiseman), who found null results in previous remote observation studies (Wiseman & Smith, 1994; Wiseman et al., 1995), with a psi proponent (M. Schlitz), who found significant results in a previous study (Schlitz & LaBerge, 1997), to see if any light could be shed on the difference in results. All of the trials were run at the same location, in the University of Hertfordshire, where R. Wiseman worked, with the same apparatus, and all 32 observees were from the same pool. The observees were assigned opportunistically to one of the observers. The observer and observee were in different rooms, separated by a distance of 20 metres. The observer greeted the observee and set up the electrodermal monitoring equipment. The randomised and counterbalanced observation sequences had been prepared in advance. It was only after the observer had left the observee and returned to the observer's room that he or she saw the observation sequence. The observer watched the observee through a CCTV link with a camera in the observee's room and a monitor in the observer's room. There was very good control against sensory leakage, error, cueing, and fraud. After an initial 2-minute delay to allow the observee's electrodermal activity to settle to baseline levels, the observer either watched the image of the observee on the monitor, or looked away and diverted his or her attention. Each trial lasted 30 seconds, and there were 32 trials for each observee. There was significantly higher electrodermal activity during remote observation in the observees for whom M. Schlitz was the observer, but no difference between observation and non-observation in the observees for whom R. Wiseman was the observer. It is not possible to determine whether this difference between the observers is due to their social interactions with the observees, their psychic ability as observers, or other characteristics. There is also a risk that the non-random allocation of observees to observers resulted in a difference between the two groups that could account for the different effect of the two observers. This risk could have been avoided by comparing the two observers

within-subjects. This study demonstrated that remote observation can lead to electrodermal activation, and that experimenter effects might play a role in this.

In summary, these experiments reviewed above all found an increase in the observees' electrodermal activity from remote observation. This equates to an increase in the sympathetic nervous system arousal of the observees, and is associated with feelings of nervousness, unease, and agitation. This fits well with the descriptions of the spontaneous experiences of remote observation. Other studies, conversely, found a calming effect from remote observation on the observees' sympathetic nervous systems, and we review them next.

A series of experiments in which the observer and observees had participated together in connectedness exercises found that remote observation reduced the sympathetic nervous system activity (W. G. Braud et al., 1993a,b). The first of these experiments (W. G. Braud et al., 1993a) was carried out similarly in terms of the observation sequence randomisation, procedure, and physical separation of the observer and observee to the experiment described above that found electrodermal activation from remote staring (W. G. Braud et al., 1993a). The connectedness exercises involved the group sharing a present-time, unitary focus of attention while voicing what they were experiencing; this generated a strong feeling of interconnectedness (Andrews, 2004). The observees and the observer/experimenter (D. Shafer) participated in the connectedness exercises together, prior to the remote observation trials. The observees' electrodermal activity was significantly lower in the observation trials compared to the non-observation trials. The most likely reason for remote observation being calming was the connectedness training. This training was interpreted as having changed the "typical ... threatening" (W. G. Braud et al., 1993a, p. 387) stimulus of remote observation, to the observer and observee being more relaxed and comfortable with the idea of connection. The connectedness training did not increase or decrease the magnitude of the effect of remote observation on electrodermal activity; it just changed the direction of the influence.

Two further experiments by the same research team also found a calming response from remote observation (W. G. Braud et al., 1993b). The first experiment used three new observers, who were trained by D. Shafer. These new observers did not receive connectedness training, although it was explained to them in general terms. In the second experiment D. Shafer was the observer. The procedure was the same as in the first of this series of experiments (W. G. Braud et al., 1990; 1993a). The observees' electrodermal activity was significantly lower in the observation trials compared to the non-observation trials. This last experiment incorporated a check that these results were not due to the observees being coincidentally in synchrony with the randomised schedule. A "sham control" was set up in which sessions were called observation/non-observation, but in all of which there was no observation. In these trials there was no significant difference between participants' electrodermal activity in the "observation" and "non-observation" sessions. This check is a strong counter claim to any criticism that significant remote observation detection findings might be artefacts of natural rhythms coinciding with the order of observation conditions.

The different effects from observation before and after connectedness training were not predicted by the authors. W. G. Braud et al. (1993b) suggested that the change in direction from remote observation, from activating to calming, was due to a change in the observer's attitude. In the first experiment D. Shafer had been uneasy and nervous, but from the second experiment onwards she was relaxed and comfortable. This increased comfort was very likely a product of the connectedness training, but might have also been due to increased familiarity and confidence from having done the experiment before. Her comfort with remote observation was transferred to the additional observers she trained, who also exerted a calming influence. This indicates that the emotions of the observer could be transmitted through observation; it might, therefore, be difficult to have a pure observation influence. This series of experiments provides strong evidence for psychophysiological remote observation detection, and how connectedness training could change the remote observation influence from activating to calming.

Another study that found a calming effect from remote observation (Wiseman & Schlitz, 1998) was a replication of Wiseman and Schlitz's (1997) experimenter effect and remote observation study described above. In a similar procedure to their previous experiment, observees were opportunistically assigned to the two observers, who followed the same methods throughout. The experiment was conducted at the Institute for Noetic Sciences where M. Schlitz worked. The observer first took the observee to the experimental room, which contained a camera, the electrodermal monitoring equipment, and headphones for the observee that played white noise, reducing the risk of noise cues. The observer then went to the observation room, which was separated from the observee's room by a few meters, and started the computer program that randomised the sequence of observation conditions for the trials. The observer watched the observee's image on a computer monitor during the observation trials, and did not watch the image and paid attention to other matters during the non-observation trials. Each trial lasted 30 seconds. There was a significant calming of observees' electrodermal activity only in M. Schlitz's sessions. In their previous experiment M. Schlitz's sessions showed a significant activation effect from observation (Wiseman & Schlitz, 1997).

Unfortunately, Wiseman and Schlitz (1998) did not discuss why there was a calming effect in this experiment, but we can make some suggestions. If, as suggested by W. G. Braud et al. (1993b), the relaxation or nervousness of the observer can be transmitted through the observation process, then this calming effect might have occurred because M. Schlitz was more relaxed in the second experiment. There are plausible reasons why she might have been more relaxed. This experiment it was a replication, and so the procedures were more familiar to her. It also replicated an experiment in which she had already found a successful psi effect, and so she might have felt more confident in her abilities to produce an effect. Additionally, this experiment was conducted in M. Schlitz's workplace, presumably more familiar and comfortable to her than R. Wiseman's workplace where the first experiment was carried out. The other difference between the experimenters that might account for

this effect was their belief in psi. M. Schlitz was a psi proponent and R. Wiseman a sceptic.

Lastly, we consider a study that compared the effect of observation with and without informing the observees that they would be watched (Lobach & Bierman, 2004). This is the first study in this review to investigate the influence of a hidden observer, i.e., an observer that the observee does not know exists. Although in the previous remote observation detection studies with psychophysiological measures the measure itself was unconscious, the observees were aware that they would be remotely observed, just not when. Lobach & Bierman's experiment is the first to measure remote observation detection when the observees were not aware that they would be remotely observed, so it can be considered the first *nonintentional remote observation detection* study. This study also included simultaneous electrodermal and conscious guess measures.

Observees were ostensibly recruited for 'judging musical fragments'. Observees rated whether 20 musical pieces, each 30 seconds long, were relaxing, by pressing a button on a response box while electrodermal measures were recorded. Unbeknown to the observee, a hidden video camera relayed his or her image to a computer in the observer's room. The observer's room was adjacent to the observee's; this did not rule out all possible noise leakage, but occluded all visual cues. The observer did not meet the observee, and did not leave the observation room during the experiment. The randomised sequence of observation and non-observation trials was determined by selecting slips of paper with stare/non-stare written on them; a unique list was created for each observee. However, the order of observation conditions was not counterbalanced, which is important in electrodermal studies due to the tendency for electrodermal activity to reduce over time.

After the first 20 trials were completed, the experimenter (not the observer) entered the observee's room and explained that in the second half of the experiment the

observee would be remotely observed at random intervals. There were a further twenty 30-second trials, during which the observee rated, with the response box, whether or not they were being observed. Electrodermal measures were taken simultaneously. There was no significant difference between the observees' electrodermal activity between observation and non-observation trials in either half of the experiment, although there was an activating effect that fell just short of significance ($p = .054$ one-tailed) in the second half when participants were informed about the remote observation. Lobach and Bierman interpreted their findings as showing that observees' electrodermal activity only responded to remote observation when they were aware that it might occur, but there are some problems with this interpretation. Firstly, detection was non-significant in both conditions, so the difference could be random variation. Secondly, there is a confounding factor. In the first half of the experiment the observees were listening to relaxing music, which might have calmed their electrodermal responses and over-ridden any effect of remote observation. The musical pieces were all rated as relaxing, and no analyses were conducted to discover whether or how they had affected the observees' electrodermal responses (Lobach, 2010).

As noted above, this was the first remote observation study to compare conscious guess and psychophysiological measures simultaneously. Observees' electrodermal activity was, on average, 1% higher when they rated themselves as being observed than when they rated themselves as not observed: a significant difference. This is in line with the social facilitation findings presented above, which imparted that when people are proximately observed, they have been found to have higher arousal levels. This is therefore an indication that remote and proximate observation might exert a similar influence. This comes with the caveat that neither the conscious guess nor the electrodermal measures significantly differentiated between observation and non-observation conditions in this experiment. It does, however, indicate that the observees associated higher electrodermal activity with the feeling of being

observed. A summary of the findings from studies with electrodermal activity as a measure of remote observation detection is presented in Table 8.

Table 8: Remote Observation Detection Studies with Electrodermal Activity Measures

Study	<i>N</i> observees	<i>t</i>	Sig at <i>p</i> <.05 *	<i>r</i>	Direction
Braud et al. (1993a) (untrained)	16	-2.66	*	.57	Activate
Braud et al. (1993a) (trained)	16	2.15	*	.48	Calm
Braud et al. (1993b) (3 observers)	30	1.98	*	.34	Calm
Braud et al. (1993b) (Shafer)	16	1.08	*	.47	Calm
Schlitz and LaBerge (1997)	48	2.65	*	.36	Activate
Wiseman and Smith (1994)	30	1.45	NS	.26	Activate ^c
Wiseman et al. (1995) (exp 1)	22	.66	NS	.14	N/A
Wiseman et al. (1995) (exp 2)	20	.91	NS	.20	N/A
Wiseman and Schlitz (1997) (Wiseman's)	16	0.48	NS	.14	N/A
Wiseman and Schlitz (1997) (Schlitz's)	16	2.25	*	.50	Activate
Wiseman and Schlitz (1999) (Wiseman's)	35	-.39 ^a	NS	.07	N/A
Wiseman and Schlitz (1999) (Schlitz's)	35	1.93 ^a	*	.33	Calm
Lobach and Bierman (2004) (hidden observers)	45	-.61 ^b	NS	0.002	N/A
Lobach and Bierman (2004) (informed observees)	45	1.64 ^b	NS	0.009	Activate ^c

Note. ^aThese values are not from a *t* test, but are *z* from a Wilcoxon signed rank test. ^bThese values are from a paired *t* test. ^cThese two studies were almost significant, so the direction of the effect is given.

Half of the remote observation studies that used electrodermal measures are independently significant, whereas five percent would be expected by chance. The studies with optimal ABBA counterbalanced and randomised observation conditions were Wiseman et al.'s (1995) second experiment, Schlitz and LaBerge (1997),

Wiseman and Schlitz (1997), and Wiseman and Schlitz (1998). With the exception of the trials conducted by R. Wiseman, these studies all found large significant effects. The only studies with possible randomisation problems: Lobach and Bierman (2004) and Wiseman and Smith (1994), found borderline effects. Therefore, the possible artefact of poor randomisation cannot be raised against the other studies' significant findings. All the studies had good control against sensory leakage and cueing. In all the experiments, except Lobach and Bierman's (2004), the observer and observee were in non-adjacent, physically separated rooms. Sensory leakage is therefore unlikely to be an artefactual confound.

The electrodermal calming effects from remote observation were predominantly from two observers: D. Shafer and M. Schlitz (W. G. Braud et al., 1993a,b; Wiseman & Schlitz, 1999). (The other calming effects were from the three observers trained by D. Shafer (W. G. Braud et al., 1993b).) These two observers also have produced activating responses to remote observation (W. G. Braud et al., 1993a; Wiseman & Schlitz, 1997). The magnitudes of the effect sizes are similar, whether the outcome was activating or calming, and they were all large effect sizes. Thus it seems that these two observers achieve a substantial remote observation effect, regardless of the direction of the effect. The unknown factor that leads to the difference in the direction of the effect does not appear to affect its magnitude. This is contrasted with an observer such as R. Wiseman who does not seem to exert an influence when remotely observing. It appears, therefore, that an observer either has an effect, whether that be activating or calming, or does not have an effect, rather than that some observers always influence their observees in either a calming or an activating direction. As these observations are based on experiments involving only three observers these comments are tentative, but this pattern, if it is genuine, validates the tendency for researchers to count both increases and decreases in electrodermal activity as remote observation effects. The calming effects occurred in the follow-up experiments when there were plausible reasons for the observers themselves to feel calmer and more relaxed about observing, such as increased familiarity with and

confidence in the proceedings. W. G. Braud et al. (1993b) also noted that the observees who received the connectedness training found that remote observation became a pleasant and positive experience, which they were disappointed to discontinue. This could indicate a remote affiliation effect, but there is insufficient experimental information to draw any strong conclusions. Based on the prevailing description of remote observation as feeling “uneasy” (Sheldrake, 2003; Titchener, 1898), and the fact that both of the observers with remote calming effects had remote activating effects in their first experiments, it is likely that the effect of remote observation will usually be electrodermal activation, unless there is a reason for calming.

Two meta-analyses have been conducted to estimate the combined effect size of the remote observation and psychophysiology studies (Schlitz & W. G. Braud, 1997; Schmidt et al., 2004). There was a large overlap in the studies reviewed by both these meta-analyses, but different outcomes were found. The first meta-analysis reviewed 11 remote observation studies with 230 individual sessions with psychophysiological dependent variables (Schlitz & W. G. Braud, 1997). In all of these studies, the observer and observee were physically separated and video links enabled observation. Sixty-four percent of the experiments were independently significant, a greater rate than would be expected by chance (5%). A mean effect size for all these studies was calculated by taking a straightforward average. This resulted in a medium (J. Cohen, 1988), and highly significant, effect size ($r = .25$). Schlitz and W. G. Braud concluded that this was a considerable effect, and not due to artefacts such as sensory leakage, human error, or the stacking effect. Schlitz and W. G. Braud were also confident they had included all the relevant studies ever conducted, avoiding the risk of a spurious effect from selective data reporting. They also found the same mean effect size for studies in which a remote influencer aimed to raise or lower a remote person’s electrodermal activity, through intention/visualisation, rather than observation, corroborating both paradigms’ findings.

The later meta-analysis analysed 15 remote observation studies (Schmidt et al., 2004). All the same papers as the Schlitz and W. G. Braud (1997) meta-analysis were included, and, in addition, two student projects conducted at the University of Edinburgh. In their estimation of the mean effect size the studies were weighted by sample size. This resulted in a very small (J. Cohen, 1988), but significant, effect size estimate ($d = .28$, the equivalent of $r = .14$). Some of the remote observation studies had two-tailed analyses, and so counted either activation or calming as being evidence of remote observation detection. A correction was applied to the estimated mean effect size to take this into account. This resulted in a smaller effect size overall ($d = .13$, the equivalent of $r = .05$). This is a smaller estimate than that made by Schlitz and W. G. Braud (1997). Both meta-analyses found homogeneity of effect sizes across the remote observation studies that they considered, and a significant overall effect.

One limitation of the remote observation detection studies is that no experiments have used any psychophysiological measure other than electrodermal activity. The electrodermal measurements in remote observation studies were strongly criticised for not being up to current standards (Schmidt & Walach, 2000). These experiments reviewed might have therefore contained artefacts, or missed effects that could have been detected.

In the experiments reviewed so far there is considerable evidence that remote observation can be detected by both conscious guess and unconscious psychophysiological measures. As argued in Chapter 1, behavioural responses are influenced by both conscious and unconscious influences, hence behaviour should respond to remote observation. Spontaneous reports and descriptions of observees' experimental experiences of remote observation also implicate behavioural responses, such as turning to look at the observer (W. G. Braud et al., 1993a; Coover, 1913; Sheldrake, 2003; Titchener, 1898) and waking from sleep (Sheldrake, 2003).

Behaviour has been researched far less than either conscious guess or psychophysiology as an indicator of remote observation detection.

6.3.5. Remote Observation Studies with Behavioural Measures.

In this section we review the previous research into hidden and remote observers' effect on behaviour. One experiment investigated whether people turn to look at hidden observers (Sheldrake, 2003). There were six observers, concealed from the observees behind a one-way mirror. The observers were in an office overlooking the entrance to a BBC studio, where a large audience was waiting to be allowed in. The audience members had signed release forms allowing video images of themselves to be recorded, and had already been ostentatiously filmed. Sheldrake (2003) therefore assumed that at the time they were actually observed and covertly filmed for the experiment they were not expecting to be covertly and remotely observed or filmed. The audience were facing away from the one-way mirror. The observers all observed the audience together, or all looked away, according to a pre-prepared randomised schedule. In the office with the observers was a video camera that ran continuously, recording images of the audience waiting outside. The number of times that members of the audience turned to face the camera was rated from the video recording by a judge who was blind to when the observation and non-observation periods were. There were 27 turns in the observation periods and 12 in the non-observation periods, which Sheldrake reported as a significant difference, although no further details were given. If this difference represents a genuine psi effect, there was an effect from the hidden observers that was greater than any possible effect of "observation" by the camera or future observation by the judge. The audience members were later interviewed, and none of them expressed that they were aware of having turned around, nor that they felt as though they were being observed. This indicates that the observees who turned may have done so automatically rather than deliberately. This is the only previously reported experiment that tests the influence of remote observation on behaviour, without informing the observees that they might

be observed, i.e., it used hidden observers, and it supports the hypothesis that remote observation can affect behaviour.

One other study considered a behavioural measure of remote observation (Lee et al., 2002). The observees, who were informed that they would be observed at random intervals, performed a motor coordination task of passing a loop along a “wobbly wire”. If an observee made an error and touched the wire with the loop, this closed an electric circuit, made a warning tone, and recorded an error. The observees were observed through a CCTV link via a monitor in an adjoining room. The position of the camera was varied: It was either in front of or behind the observee. The number of observers was also varied: There were either three observers or one. There were no differences found in the error rate on the wobbly wire task from observation, camera position, or number of observers. Lee et al. (2002) did not consider the effect that normal (non-remote) observation would have on task performance (i.e., the social facilitation effect). Therefore, they could neither predict whether remote observation would be likely to have an effect on this task, nor what the direction of that effect would be.

Behaviour is under-researched as a measure of remote observation detection, compared to conscious guess and psychophysiology. Sheldrake’s (2003) findings are promising, and use a measure of turning to look at the observer, which is one of the most frequently reported behavioural responses to remote observation. Studies of performance tasks, such as Lee et al.’s (2002), would benefit from considering the effect that normal observation has on performance: the social facilitation effect. Research that combines the influence of remote observation and the social facilitation effect as a behavioural response would further the field of remote observation detection.

6.3.6. Summary of Remote Observation Detection Research.

In summary, parapsychological investigations into remote observation detection were inspired by a plethora of reported spontaneous experiences, and aimed to discover whether this is a genuine psi phenomenon. The field has developed from poorly-controlled early investigations that exclusively used conscious guess measures, to sophisticated well-controlled experiments with conscious guess and psychophysiological measures. These experiments have shown that there is an effect from remote observation detection, and that this effect is better captured by unconscious psychophysiological measures than conscious guess measures. As behaviour can respond to conscious and unconscious influences, behavioural responses are likely to be an effective way to detect remote observation, and, despite many reported behavioural reactions to remote observation, behavioural responses are under-researched. One criticism of the remote observation research carried out to date is that the normal (non-remote) effect of observation has not been considered, or compared to, the remote effect. As reviewed above, the social facilitation effect describes the effect of normal observation on task performance. We now consider why remote observation might elicit social facilitation, and methodological issues inherent in combining remote observation and social facilitation.

6.4. Remote Observation and Social Facilitation

6.4.1. Why might Remote Observation elicit a Social Facilitation Effect?

This section briefly encapsulates the reasons we might expect remote observation to manifest a social facilitation effect. Firstly, to return to the main working hypothesis of this thesis, we predict that, through psi, people will respond to extrasensory circumstances if they would respond to the same circumstances given sensory knowledge of them (Stanford, 1990). Therefore, if sensory knowledge of observation (proximate observation), leads to the social facilitation effect, it follows that remote

observation should also. Secondly, both remote observation detection and the social facilitation effect relate to arousal levels. As reviewed above, remote observation can affect an observee's sympathetic nervous system arousal (e.g., W. G. Braud et al., 1993a,b). As argued above, the most likely arousal effect is activation, unless there is some reason for a calming effect. The social facilitation effect might also be mediated by changes in arousal, and, although this is not universally accepted, it is still the leading explanation of the social facilitation effect (Mullen et al., 1997). Therefore, if remote observation leads to arousal increases, and these lead to the social facilitation effect, then remote observation would lead to the social facilitation effect. Incidentally, Stanford (1990) hypothesised that, through psi, changes in arousal would occur in response to relevant circumstances. One example of this might be an increase in arousal from remote observation, if such an increase would occur from proximate observation. The last reason why remote observation might elicit a social facilitation effect relates to the possibility of the observee becoming aware of being remotely observed. If, for example as Sheldrake (2003) argued, people can become aware of remote observation, it might influence their behaviour in the same way as a proximate observer.

6.4.2. Investigating a Social Facilitation Effect from Remote Observation.

This section outlines some of the important methodological considerations required in testing for a social facilitation effect from remote observation. Firstly, we briefly reiterate considerations important in psi experimentation discussed above and in Chapters 1 and 2. Secondly, we consider the optimal interaction between observer and participant. Lastly, we explain the importance of a *true alone condition*, and how this might be created.

In psi experiments it is important that there is no sensory leakage or inadvertent cueing, as any non-psi transfer of information to the participant invalidates support

for the psi hypothesis. For the same reason, the order of the observation conditions should be truly random, as should the allocation of participants to observation conditions.

There are two aspects concerning how the observer and participant might optimally interact to create a social facilitation effect. The first of these is based upon the limited findings relating to observer/participant familiarity: More familiar observers affect participants' behaviour differently (Guerin, 1993), and might exert less of a social facilitation effect (C. F. Bond & Titus, 1983). Although the arousal explanation for social facilitation is not unequivocal, the familiarity findings can be interpreted in line with the remote observation detection findings explained above. Namely, that remote observation usually increases sympathetic nervous system activation, but that interpersonal connectedness and comfort can reduce sympathetic nervous system activation (e.g., W. G. Braud et al., 1993a,b). This link is tentative, but implies that the observer should not create an affiliative bond with the participant, or, if one already exists, this should be controlled for.

The second aspect pertaining to the way the observer and participant interact concerns the behaviour and location of the observer during proximate observation. As we will be considering the effect of an observer, not a co-actor, the observer will not engage in any particular activity, but will just watch the participant. Research that compared continuous watching to intermittent watching found that continuous watching had a greater social facilitation effect (Huguet et al., 1999). The location of the observer has also been considered. Observers who stand diagonally behind the participant, looking over his or her shoulder, have been found to exert a greater social facilitation effect than an observer sitting in front of the participant (Huguet et al., 1999; Klauer, Herfordt, & Voss, 2008). Therefore, in Experiments 3 – 5 the observer will stand diagonally behind the participant, clearly and continuously watching.

The final methodological condition we will expound here relates to the importance of a true alone condition. In a true alone condition there is no presence, observation, evaluation apprehension, expectation of evaluation, or deception of observation. In other words, a true alone condition aims to eliminate, as far as possible, all the influences that could manifest the social facilitation effect. As noted above, the alone condition has been poorly controlled in the majority of social facilitation research (see Guerin (1993) for an exhaustive list of 140 studies with presence or observation in the condition called alone). For example, Travis (1925) compared an audience condition of four to eight students, and an alone condition with the experimenter present. Although Travis found a social facilitation effect from this audience, it does not mean that experimenter presence does not exert any social facilitation effect, merely that the audience exerted more. Moreover, Ekdahl (1929) found complex task inhibition from the experimenter observing, compared to a physically alone condition. This means that experiments with poorly controlled alone conditions might appear to find a weaker social facilitation effect (see, e.g., Uziel, 2007), or fail to find an effect entirely, due to the inadequate baseline for comparison. Poorly controlled alone conditions have led to discrepancies in the literature. For example, Cottrell et al. (1968) claimed that mere presence exerted no greater effect than alone, but, as noted above, the experimenter was listening in the alone condition, which constitutes an evaluative influence. However, experiments with improved alone conditions did find a social facilitation effect from mere presence (Markus, 1978; Schmitt et al., 1986). Remote observation is likely to be a weak influence compared to proximate observation, and it is therefore vital that a true alone condition is created as a baseline.

Guidelines for creating a true alone condition in social facilitation experiments were created by Markus (1978). These were designed for investigating mere presence, but are transferable to investigating remote observation because both require that a true alone condition be created as a baseline. We will assess their suitability for investigating remote observation. Markus recommended that simple and complex

tasks that can be measured or assessed are used. This is to ensure that both the effects of simple task facilitation and complex task inhibition be captured. She specified that these tasks should not, in and of themselves, elicit evaluation apprehension, and that neither the participant nor the observer should invoke a standard to compare the performance to. This is to reduce the possible effect of evaluation apprehension, or other forms of negative judging that can act as if they are a form of observation, such as negative self-evaluations (Baumeister, 1982; C. F. Bond, 1982). Likewise, Markus (1978) and Chapman (1974) recommended using tasks that lack clear criteria for good or bad performance. In contrast, almost all the tasks used in previous social facilitation research had clear criteria for success or failure, such as anagram tasks (e.g., Aiello & Svec, 1993) and many of the other tasks reviewed above. One other way to reduce evaluation apprehension is to have participants perform a task that they do not believe to be part of the experiment. Lastly, the participant should truly be alone. These criteria, of reducing evaluation apprehension as much as possible and physical aloneness, also apply to remote observation and social facilitation research. One extra criterion would be added for social facilitation research considering remote observation: There should not be any covert observation of the participant, as this is the remote observation stimulus to be tested.

Markus (1978) argued that no previous experiments had fulfilled her criteria. Her experiment was outlined above when considering the mere observation effect, but certain aspects of the methodology regarding the alone condition will now be expanded upon below. Individual participants were told that they were to participate in a group experiment, for which they all had to be dressed the same. Each participant was asked to prepare for this by taking off his own shoes, putting on socks provided by the experimenter over his own socks and putting on large trainers and a large lab coat. The experimenter subsequently explained that the other participants had not arrived and the experiment could not proceed, so the participant could change back into his own clothes. Although the participants were led to believe that the experiment had not happened, the act of changing in and out of their own

clothes was the simple task, and changing in and out of the unfamiliar clothes was the complex task. This procedure is likely to have reduced evaluation apprehension, because the task of getting dressed should not evoke evaluation apprehension, and because the participants were led to believe that this was an incidental part of the proceedings, and not part of the experiment itself.

Each participant got changed under one of three observation conditions, to which they were randomly assigned. In the proximate observation condition the participant was watched by a confederate sitting in the testing room. In the mere presence condition there was a person present in the testing room with his back turned to the participant, busy mending machinery. In the alone condition participants were physically alone in the testing room, but in all conditions they were timed by a confederate watching through a gap in the curtains that covered a one-way mirror. As noted above, the social facilitation effect was found, with a clear increase in the time taken to change unfamiliar clothing from alone to mere presence, and mere presence to proximately observed conditions, and a clear decrease for the familiar clothing. The alone condition was clearly an adequate baseline to compare mere presence to. It was not, however, truly alone in the way that an alone condition in a remote observation experiment would require to be. The use of a covert, remote observer in the alone condition in Markus's (1978) experiment indicates that Markus did not consider that this form of observation could exert any influence. If the possibility of social facilitation from remote observation is to be tested, however, an alone condition without remote or covert observation has to be used as the baseline.

The importance of the covert nature of tasks in creating an alone condition was further emphasised by research that investigated the conditions under which participants feel monitored (Griffin, 2001; Griffin & Kent, 1998). Griffin and Kent predicted that participants to whom a task was given would assume that they were being monitored, and that if no obvious method of monitoring them were apparent, they would imagine one. They randomly allocated participants to one of three

conditions: alone without a task; alone with a card sorting task; and alone with a card sorting task, self-timed with a stopwatch. Participants with a task felt significantly more judged, concerned about doing well, and monitored than did participants without a task. In a follow-up study, Griffin (2001) compared the same three alone conditions as Griffin and Kent (1998) and a proximate observation condition. The participants felt most monitored with the audience, and more monitored in the task conditions than the alone without a task condition. Thus, participants engaged in an experimental task will infer that they are being monitored, even without apparent means of monitoring, and will therefore experience evaluation apprehension, despite being physically alone. Therefore, physical aloneness does not guarantee phenomenological aloneness, and the nature of the task has to be considered when creating a true alone condition. Specifically, the participants should not realise that they are performing a task: The task should be covert.

There is one previous social facilitation experiment that used a true alone condition and covert tasks. Schmitt et al. (1986) intended to improve upon Markus's (1978) experiment. Schmitt et al. criticised the task of changing into large and ill-fitting clothing for potentially causing anxiety about one's appearance. Instead, they used typing tasks, which they did not think would cause embarrassment or evaluation apprehension. Participants were asked to type their name (the simple task) and then their code for the experiment, comprised of their name spelled backwards, interspersing ascending digits (the complex task). These tasks were presented as gathering background information prior to the start of the experiment, which was ostensibly about sensory deprivation. Participants were randomly assigned to one of three observation conditions: proximate observation, mere presence, or alone. In the proximate observation condition the experimenter stood in the room with the participant, looking over his or her shoulder. In the mere presence condition a confederate who was blindfolded and wearing head-phones, unable to see or hear, was in the room with the participant. In the alone condition the participant was physically alone and unobserved. Unbeknown to the participants, typing speed was

recorded by the computer, circumventing the need for any human observation of the participant in order to measure performance. No participants suspected that they were being timed, or that these tasks were part of the experiment. There was a significant interaction found between task complexity and observation condition (as described above). This is, to my knowledge, the only prior social facilitation experiment with an alone condition adequate for investigating a social facilitation effect from remote observation.

The tasks described above, of typing the name and a code, fulfil the criteria set out by Markus (1978) for tasks that could be used with a true alone condition. They are comparable simple and complex tasks, which did not cause participants to infer that they were being observed or evaluated. It was possible to present them as an incidental part of the experiment set-up without participants suspecting that they were being observed or recorded (Schmitt et al., 1986). The time taken to type can be recorded automatically by the computer. These characteristics make these tasks the most suitable of any that have been researched before for use in a social facilitation experiment that uses a remote observation condition. The use of deception of the participant, into believing that the tasks are an incidental part of the experimental procedure, and tasks that are amenable to this, might be crucial in reducing evaluation apprehension enough to create an alone condition that remote observation can be compared to.

6.5. Summary

This literature review integrated previous empirical findings relating to the effects of being observed from the fields of social facilitation and remote observation detection. Many forms of social presence, including proximate observation, have been found to lead to the social facilitation effect. Arguably, mere observation also elicits the social facilitation effect, and testing for the social facilitation effect from remote observation would examine this claim further. It would also explore whether

covert observation can justifiably be used as if it makes no difference to participants' task performance. Remote observation detection research has found some evidence that people can detect psi-mediated observation, but behavioural measures of remote observation detection are currently under-researched and inconclusive. Considering whether remote observation could lead to the social facilitation effect would extend both these fields, by combining the psi influence with the well-researched behavioural effect from proximate observation. In the following two chapters, we present three experiments that investigated whether there is a social facilitation effect from remote observation.

7. Experiments 3 and 4: Psi-Mediated Social Facilitation from Remote Observation in Covert Task Performance

7.1. Overview

This chapter presents two empirical studies. Experiment 3 investigated nonintentional behavioural responses to remote observation (covert observation that one can only detect through psi). Specifically, Experiment 3 asked whether remote observation can elicit the social facilitation effect (simple task facilitation and complex task inhibition). Participants completed simple and complex typing tasks under three observation conditions within-subjects: alone, remote observation, and proximate observation (overt observation from a physically present observer). The moderating effects of participants' extraversion, neuroticism, state anxiety, belief in psi, and familiarity to the observer were controlled for. This is, to our knowledge, the first investigation of a social facilitation effect from remote observation. Experiment 4 addressed limitations in the methodology of Experiment 3, and aimed to obtain the social facilitation effect comparing only proximate observation and alone conditions on covert task performance.

7.2. Introduction

The PMIR model states that, through psi, people respond nonintentionally to situations they do not have sensory knowledge of, if sensory knowledge of these situations would itself provoke a response (Stanford, 1990). As reviewed in Chapter 6, people respond to observation that they have sensory knowledge of with the social facilitation effect. The social facilitation effect is the facilitation of simple task performance and the inhibition of complex task performance when the participant is in the presence of an observer (proximate observation) compared to being alone (Zajonc, 1965). If the PMIR statement above is correct, then observation that participants cannot have sensory awareness of, and could only be influenced by

through psi (remote observation), would also have a social facilitation effect. Experiment 3 aims to investigate whether remote observation has a social facilitation effect by comparing performance on simple and complex tasks under remote observation, proximate observation, and a true alone condition.

Experiment 3, below, investigated whether remote observation can lead to the social facilitation effect by comparing it to both being alone and to proximate observation (observation from a present observer). Remote observation is covert observation by a hidden observer, who participants can only be influenced by through psi. Remote observation was achieved by the hidden observer watching the participant via a webcam link from another room, which isolated the observer and the participant from sensory contact. As argued in Chapter 6, it is important that the alone condition is a true alone condition, without presence, observation, covert observation, or evaluation apprehension (Markus, 1978). In addition, participants should not suspect that they are being observed, or that their task performance is being monitored (Markus, 1978). In order to create a true alone condition several steps were taken. Firstly, participants were alone in the room. Secondly, participants were not informed that any remote observation would occur. This differs from the remote observation influence usually used in parapsychology experiments, in which participants are aware that they would be observed remotely, just not when (e.g., W. G. Braud et al., 1993a, 1993b). Finally, participants were not engaged in a task on which they would expect their performance to be monitored. Participants infer that they are being monitored if they are given a task, even if there is no overt method of monitoring or observing them (Griffin, 2001). Therefore, participants did covert tasks.

Following Markus (1978) and Schmitt et al. (1986) we used covert tasks, which participants were told were an incidental part of the preparation for the experiment. The tasks were presented as being the log-in to another experiment, which had three rounds, justifying three log-ins (one for each observation condition). The tasks were

to type the participant's name (simple task) and an alphanumeric code (complex task), similarly to logging-in to a computer or an email account. As name and code or password log-ins are common we considered this a plausible ruse. These tasks could be measured covertly, and had shown a social facilitation effect in a previous experiment (Schmitt et al., 1986). Specifically, Schmitt et al. found that proximate observation increased participants' speed of typing their names and decreased the speed of typing their codes. Thus, these typing tasks demonstrated the social facilitation effect of simple task facilitation and complex task inhibition. In this experiment, proximate observation and remote observation were predicted to increase participants' speed of typing their names, and reduce participants' speed of typing their codes.

As reviewed in Chapter 6, there are personality and individual differences characteristics that can affect both task performance and how observation affects people's task performance. High neuroticism, for example, is associated with performance decrements from observation irrespective of task complexity (Uziel, 2007), and neuroticism and anxiety improve performance on easy tasks and impair performance on difficult tasks (Matthews & Deary, 1998). Extraversion (Uziel, 2007), neuroticism, and state anxiety could confound the relationship between observation and complexity and task performance. There are also other variables that can affect participants' responses to observation, such as the relationship between the observer and the participant (Guerin, 1993; F. G. Miller et al., 1979; Henchy & Glass, 1968). Finally, there are individual differences that affect task performance in general and/or psi task performance. These include age (Der & Deary, 2006), gender (Dalton, 1994), and belief in psi (Lawrence, 1993). To ensure that the effect of observation and complexity was investigated without the effect of these potential confounds, they were controlled for. Given the complicated nature of the possible effects of these individual differences on the social facilitation effect, directional hypotheses were not made.

In Experiment 3, below, participants typed their first name (simple task) and a code made up of the letters of their name backwards interspersed with ascending digits (complex task) three times, which they were told was the log-in to a subsequent experiment. Each time they did the typing tasks, the participants went on to complete a short parapsychology experiment that provided a plausible experiment for the participants to log-in to and provided time between the log-ins for the experimenter to change location as the observation conditions required. Each time the participants typed their name and code they were either alone, remotely observed, or proximately observed; the order of the observation conditions was randomised and counterbalanced. Firstly, we asked whether the normal social facilitation effect was replicated, by investigating whether there was an interaction between observation condition and task complexity, when only the alone and proximate observation conditions were considered. Secondly, we asked whether there had been a social facilitation effect from remote observation by investigating whether there was an interaction between observation condition and task complexity, considering all the observation conditions. Lastly, we controlled for possible confounding personality and individual differences variables.

7.2.1. Hypotheses.

1 There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex). Proximate observation will increase the speed at which participants type their name (the simple task) and decrease the speed at which they type the alphanumeric code (the complex task) in comparison to their typing speed when alone. In other words, proximate observation will elicit the social facilitation effect.

2 There will be an interaction between observation condition (proximate observation, remote observation, or alone) and task complexity (simple or complex). Both proximate observation and remote observation will increase the speed at which

participants type their name (the simple task) and decrease the speed at which they type the alphanumeric code (the complex task) in comparison to their typing speed when alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

7.3. Method

7.3.1. Design.

This experiment used a 3x2 within-subjects design. The independent variables were observation condition (alone, remote observation, or proximate observation) and task complexity (simple or complex).

7.3.2. Participants.

A convenience sample of 30 participants was chosen for this experiment. They comprised 14 men and 16 women; 15 were university students. They ranged in age from 17 to 61 years ($M = 29.37$ years, $SD = 11.56$ years).

Participants were ostensibly recruited for a “Colour and Precognition” experiment. They were asked to try to bring someone unknown to the experimenter. They were told that this was to aid recruitment, because people who did not know the experimenter would be less likely to volunteer. It was also to ensure that participants unknown to the experimenter would be included in the experiment. The participants were a mixture of friends or acquaintances of the experimenter and strangers, and a mixture of people she had either a peer or expert status with.

Experimenter and Observer.

The author took the role of experimenter and observer.

7.3.3. Ethical Considerations.

We identified two potential ethical issues in this experimental design. There was deliberate deception of the participants regarding the overall aim of the experiment and the exact function of the experimental tasks, and covert observation via webcam images relayed to a computer terminal. As discussed in Chapter 3, the deception issue was discharged by giving participants the right to terminate the experiment at any time and a full debrief following the experiment. The images of the participants were not recorded, and so this method of covert observation raised no data-protection issues. We anticipated that the covert observation would cause participants no psychological harm or stress. We concluded that giving participants the right to terminate the experiment at any time and a full debrief following the experiment would resolve this issue. The Psychology Research Ethics Committee granted ethical approval for this experiment to proceed.

7.3.4. Materials and Apparatus.

Program.

The typing tasks and the decoy (colour and precognition) task were combined in one program, written by the author in E Prime (Psychology Software Tools, 1996). This program ran on a PC.

E prime is software designed for presenting stimuli in psychology experiments and recording and timing responses. To time the letter strings a function called ‘ask box’ was used. This presents an input box on the screen into which text can be typed. It timed from the opening of the input box, triggered by the participant pressing the space bar in response to instructions on the previous screen, to the closing of the onscreen box, triggered by the participant pressing return after typing the name or code. It did not record the time taken to make each keystroke. This program recorded the text that was left in it, but it did not record letters that had been erased or

replaced. For example, whether Sam typed *Sam* or *s-backspace-Sam* the output was the same: *Sam*. Timing in E prime is potentially millisecond accurate, but this can be compromised by the power of the computer that it is run on (E Prime, 2002). Tests (Schneider, Eschman, & Zuccolotto, 2002) run on the experimental computers in the department of psychology by the computing supervisor demonstrated that the timing fell very slightly short of millisecond accuracy, but well above 1/100th of a second (centisecond) accuracy. As a conservative measure, the centisecond accuracy level was used in this experiment.

Webcam.

The webcam was a Creative Web Blaster III which used Microsoft Netmeeting Video Conferencing software.

Personality Questionnaires.

Participants filled out their contact information, age, and gender on a participant information form that included the 12-item belief in psi scale produced by the Koestler Parapsychology Unit (Appendix 1).

Participants also completed the EPI (Eysenck & Eysenck, 1964). This 57-item questionnaire is a validated, well-tested measure of extraversion and neuroticism with extensive experimental and theoretical support (Eysenck, 1967a). This questionnaire includes negatively phrased items. An example of a negatively phrased item for extraversion is “Do you prefer to have few but special friends?” An example of a positively phrased item for neuroticism is “Would you call yourself a nervous person?” This questionnaire includes a lie scale to check for socially desirable responding. No participants had high lie scale scores, and no prediction had been made about these, so nothing further is reported on them. Participants answer the questions on a binary response scale, choosing either *yes* or *no*. There is a maximum

score of 24 for extraversion and 24 for neuroticism. High scores indicate high extraversion, or high neuroticism.

To measure state anxiety, participants completed the 20-item state anxiety section of the STAI (Spielberger, 1983). Participants respond to each item on four-point Likert scales anchored at 1 (*not at all*) and 4 (*very much so*). The scores have a minimum of 20 and a maximum of 80; high scores indicate high state anxiety.

7.3.5. Procedure.

Randomisation of observation conditions.

A schedule of observation conditions was prepared in advance by a member of the University staff who was not otherwise involved in this experiment. She prepared a schedule for 30 participants, with five repetitions of each of the six possible combinations of the three observation conditions (e.g., remote observation first, alone second, and proximate observation third). The order that these combinations were listed in on the schedule was randomised in Visual Basic (Microsoft Corporation, 2003). The experimenter was not blind to the order of the observation sequences. Participants were assigned to the next line in this list as they finished their pre-test questionnaires, which avoided any selection bias in the assignment of participants to their order of observation conditions.

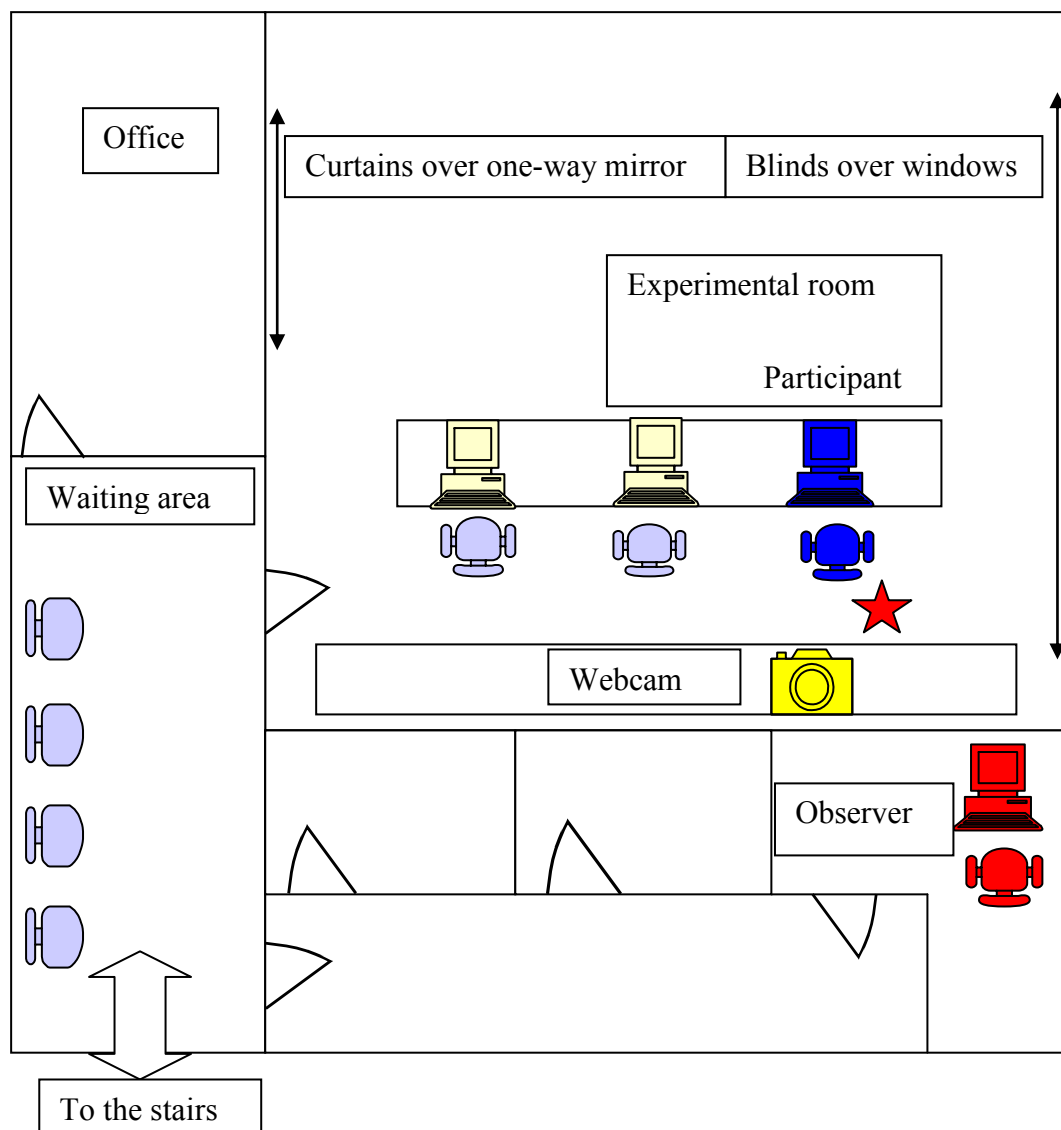
Session procedure.

All the sessions took place on the first floor in the Department of Psychology at the University of Edinburgh. Three rooms were used: an experimental room, a waiting area, and a remote observation room (see Figure 4).

Participants were directed to the waiting area from the front door of the psychology department by posters. The waiting area was the corridor just outside the computer laboratory. This linked the experimental room to an office, the small corridor to the

remote observation room, and the staircase. In the waiting area a sign requested that participants begin to fill out the questionnaires. There were seats and clipboards with pens and questionnaires provided. If the experimenter was free she waited here for the participants, and if she was busy with the experiment she checked the waiting area regularly.

All the participants filled out the belief in psi questionnaire and the EPI in the waiting area. Participants also signed a standard consent form agreeing to participate, and on the understanding that they could terminate the experiment at any time (no participants chose to). The first six participants also filled out the STAI in the waiting area. From the seventh participant onwards, the STAI was completed sitting in front of the experimental computer. This change was made because the state anxiety test is sensitive to the situation in which it is conducted (Spielberger, 1983). Participants had been arriving in small groups, and chatting to each other while they filled out the questionnaires. It would not have given an accurate estimation of their state anxiety during the experiment if participants filled out the questionnaire while conversing with their friends. In order to make the conditions under which each participant filled out the state anxiety questionnaire as similar as possible to the conditions under which the experiment would be conducted, the experimenter made sure she was in the room for part of the time, and out of the room for part of the time, although she made no attempt to remotely observe during this time.



The star marks the position for the observer in the proximate observation condition.

The word 'Observer' denotes the remote observation room.

Figure 4: Map of the Experimental Suite for Experiment 3 (not to scale).

Participants were tested individually. They were shown one at a time into the experimental room after completing the questionnaires. The experimental room was

a large computer laboratory with eight computers in total. All the monitors, except that of the experimental computer, were switched off. There was a one-way mirror in the wall between this room and the waiting area; this was covered by curtains, and there were blinds over the windows. No aspect of the room layout should have made participants feel overlooked.

Then the experimenter sat next to the participants at the experimental computer, and explained the procedure to them. She explained that they would do a colour and precognition task. The colour and precognition task maintained the covert nature of the name and code tasks by deceiving participants that they were only a log-in to the “real” experiment. As many of the participants knew that the experimenter was interested in parapsychology, we thought a psi task would be a convincing decoy task. The results from the colour and precognition task are not reported in this thesis. The colour and precognition task comprised three rounds, justifying participants logging in three times.

The experimenter told participants that in order to start each round they would log-in to the experiment by entering their first name and their experiment code. Participants were told this was so that the data would be logged under their name. The experimenter wrote both the name and code down for the participants on a piece of paper, explaining how the code was made up of their first name spelled backwards interspersed with ascending digits. We assumed that this verbal explanation and informal presentation of the task would make the log-in seem like an incidental part of the proceedings. Detailed onscreen instructions might have attracted too much attention to the importance of the log-in.

Whether or not participants asked if she would be staying in the room with them, the experimenter always said that she would be “in and out”. This was to prevent her arrivals and departures from being any more of a distraction than they needed to be. After an opportunity to ask questions, the participants completed three rounds of the

log-in and the colour and precognition tasks. Each round followed the same basic format. Instructions for each round of the colour and precognition task were presented, along with a reminder to log-in. The participants controlled the speed at which these screens scrolled by pressing the space bar. This allowed participants time to understand the instructions. It also ensured that their fingers were at the keyboard, ready for the typing tasks, so the time recorded for the typing tasks did not include the delay of participants moving their hands to the keyboard. The first round differed only in that the first screen of instructions remained on the monitor for 25 seconds before participants could scroll on. This allowed the experimenter time to leave the room, if she was required to be outside the room for the first round. It also ensured that participants did not accidentally scroll past the instructions (a problem revealed in the pilot testing stage). Participants typed their name into a text box on one screen, and their code into a text box on the consecutive screen. The colour and precognition task started automatically once participants had completed the log-in.

In each round the experimenter either observed participants from within the room, observed them remotely, or left them alone according to the observation condition schedule. In the proximate observation condition the experimenter was in the experimental room. She watched participants typing at close proximity, standing just behind them looking over their shoulders. She could see the participants, their hands on the keyboard, and their computer screen. She focussed her attention on the participants the whole time and blocked out other thoughts by mentally rehearsing “I am watching you. You are being watched.”

In the remote observation condition participants were physically alone in the experimental room. The experimenter observed his or her image onscreen via a webcam link from the observation room. The remote observation room was a small room containing one computer. The two rooms shared one common wall, but neither could be seen from the other room. The webcam was situated two metres behind the participants, a little to the right, so that the view was of the participants from the

waist up and the screen in front of them. The webcam was hidden by a pile of wires, a mouse, and a mouse mat that looked like general office hardware. The webcam was running throughout the whole experiment (in the alone and proximate observation conditions the camera was on and images of participants were sent to the remote observation computer, but no-one was watching the images). The images were not recorded. In the remote observation sessions participants' images were watched continuously. The observer focussed her attention on the images the whole time and blocked out other thoughts by mentally rehearsing "I am watching you. You are being watched."

In the alone condition participants were physically alone in the experimental room. The experimenter was outside the room, and occupied her mind with other thoughts. There was a sign on the door to inform people that there was an experiment in progress and not to go in. The door could only be opened by a swipe card, which only staff and senior students had, and the room was booked on the Department of Psychology's website. These measures prevented other people from entering the experimental room during the trials. Therefore, there was no-one watching the participant, and no-one other than the participant in the experimental room in the alone condition.

After the three rounds of the experiment were over, the experimenter re-joined the participant at the experimental computer. The experimenter verbally asked the following manipulation check questions and wrote down participants' answers.

- 1 "Honestly, did you realise the typing tasks were part of the experiment?"
- 2 "How often do you type your name on an average working day?"
- 3 "Have you ever typed your name backwards with numbers interspersed, like the code?"
- 4 "Did you notice any difference between the three times you typed your name and code?" (This question was discussed and the experimenter

prompted the participants to comment on their level of nervousness, and the speed and ease of the typing tasks, if they did not volunteer this information.)

5 “Did you see the webcam?”

All the participants were asked questions 1 – 4 above. It would have been inappropriate to ask all of them whether they had seen the webcam, because many of the participants knew each other, and might tell others who were in the experiment later. Only five participants known to be discreet were asked whether they had seen the webcam. In doing this the experimenter made two assumptions: that the camera was adequately hidden, and that any participants who saw the camera would ask her about it.

Participants were thanked for participating, and could take a sweet from a ‘lucky dip box’ as a gesture of thanks before they were shown back to the waiting room. Only after all the participants were tested and the results were analysed were the participants debriefed by email.

After all the participants had completed the experiment, but before the data were analysed, the experimenter coded participants by how well she knew them. Participants were categorised into one of five groups on an ordinal scale: 1 (*total stranger never met before the experiment*), 2 (*knew by sight*), 3 (*colleague or casual acquaintance*), 4 (*friend*), and 5 (*good friend*). This rating was called *familiarity*.

7.3.6. Analyses.

Analyses were conducted in SPSS version 14 and Microsoft Excel.

7.4. Results.

Manipulation check 1: Did the participants believe that the typing tasks were not part of the experiment?

Twenty-three participants (77%) did not realise that the typing tasks had anything to do with the experiment; many were surprised even to think that they had. Seven participants (23%) thought that the typing task might have had something to do with the experiment. This was expressed as an “inkling”; no-one guessed the purpose of these tasks. The name and code were, therefore, on the whole, convincingly presented as an incidental part of the experiment.

Manipulation check 2: Was the typing the name a familiar task?

Twenty-five participants (83%) typed their name once or more on an average working day. Five participants (17%) reported typing their name less than once an average working day, four of whom reported typing it once a week or more, and one participant once a month. Typing the name was therefore confirmed as a simple task in that it was well practiced.

Manipulation check 3: Was typing the code a novel task?

No participant had ever typed their name backwards interspersing ascending digits. The code was therefore a complex task in that it was novel.

Manipulation check 4: Were there any differences between the rounds in participants' self-reported nervousness and the speed and ease of doing the tasks?

Seventeen participants (57%) reported no difference in their level of nervousness between the three rounds. Ten participants (33%) reported feeling less nervous as the experiment proceeded. This reduction in nervousness over time could have been

because the participants became more comfortable with the experiment as it progressed. Two participants (7%) reported that they were more nervous in the proximate observation condition because they were aware of being watched.

Eighteen participants (60%) reported an increase in the speed of the typing tasks, twelve (40%) did not. Twenty-two participants (73%) reported an increase in the ease of the typing tasks, one that they became harder, and seven (23%) reported no difference. Except for the participant who reported that the tasks became harder, these differences are attributable to practice effects. One participant commented that he had memorised the code by the final round.

Manipulation check 5: Did anyone see the webcam?

None of the five participants who were asked had seen it.

Exploratory data analyses for Hypotheses 1 and 2.

The normality of the data was assessed by examining z -scores for the skew and kurtosis, and by visual inspection of histograms. The criterion for normality of the z -score of skew and kurtosis was ≤ 2.58 (Field, 2005). Two of the six cells (for the name and code in the alone condition) were not normally distributed, they were positively skewed, and the rest were normally distributed. Deleting outliers from one cell would have led to either dropping that participants' data from all cells, or having unequal numbers between cells. ANOVA are robust against some non-normality as long as cell sizes are equal (Field, 2005), and so the data were retained. As part of the exploratory analyses the data were subjected to a \log_{10} transformation to normalise the data, and ANOVA were run on the transformed data. This found the same outcome in the results as the ANOVA on the non-transformed data. As most of the cells were normal, and the transformation did not reveal any further effect that the non-transformed data had not shown, the non-transformed data is used for all the presented results.

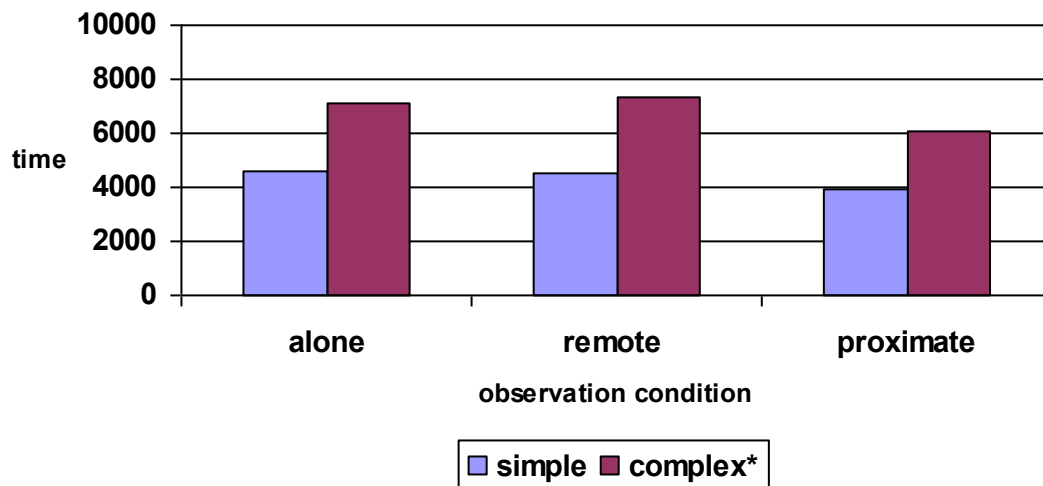
Exploratory analyses conducted on these data found that the complex task took longer to type. As the complex typing task (code) involved more keystrokes than the simple task (name), the code should take longer to type, irrespective of whether it were a more complex task. In order to counteract the effect of the code being longer, the times for the complex task were halved. For names under 10 letters long the code was exactly twice the number of keystrokes as the name, so halving was perfectly accurate for these participant's data. There was only one name of 10 letters in length, which had a code of 21 keystrokes, not 20. As there was only one name that would be inaccurate by only one digit in this analysis, halving was a reasonable method of comparing the time taken to do these tasks more directly. Any further effect of complexity found would reflect the different nature of the tasks, rather than the different number of keystrokes. All presented analyses use the halved times for the complex task. It is not necessary to calculate the times per letter because the within-subjects design ensures that the lengths of the names are constant across the groups. It was not possible to calculate the time per actual keystroke, because the E Prime program did not record changes or deletions. Table 9 displays the means, with the halved times for the complex task. These data are also displayed in Figure 5.

Table 9: Time taken in milliseconds (rounded to centiseconds) to complete Simple and Complex Typing Tasks by Observation Condition

Task	Observation condition						Row Means
	Alone		Remote		Proximate		
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	
Simple:	4610	2690	4520	3520	3930	1980	4360
Complex ^a	7140	3570	7350	4350	6090	3060	6860
Column Means	5880		5940		5010		5610

Note. *N* = 30. The values in were first created from the raw values from E Prime, and then rounded up to centiseconds. The row, column and grand totals may not match the means that would be created from if the rounded data in the cells were averaged

^aThese values are halved.



* These values are halved

Figure 5: Mean Time in milliseconds (rounded to centiseconds) to complete Simple and Complex Typing Tasks by Observation Condition

Table 9 and Figure 5 display the mean times by task complexity and observation condition. From visual inspection of these data, the simple task was completed faster under proximate observation than remote observation, and faster under remote observation than when alone, as predicted. The complex task was completed slowest under remote observation and fastest under proximate observation; this is not the predicted pattern.

Hypothesis 1: There will be an interaction between observation (proximate observation or alone) and task complexity (simple or complex). Proximate observation will increase the speed at which participants type their name and decrease the speed at which they type the code in comparison to their typing speed when alone.

In order to test Hypothesis 1, and discover whether there was a normal social facilitation effect in Experiment 3, a two-way ANOVA was calculated with typing

speed as the dependent variable. Observation condition (proximate observation or alone) and task complexity (simple or complex) were the within-subjects variables. Remote observation was omitted from this analysis. The outputs are in Table 10.

Table 10: Outcome of the 2x2 Analysis of Variance for Typing Times

Variable Main Effects	<i>df</i>	Error <i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	29	5.31*	.16	.03	.61
Complexity	1	29	23.97**	.45	<.01	>.99
Interaction						
Observation x Complexity	1	29	0.57	.02	.46	.11

Note. *N* = 30.

* *p* < .05; ** *p* < .01

As shown in Table 10, there was a significant effect of task complexity, despite halving the times for the code. This shows that the complex task was completed more slowly than the simple task, and that this is due to a difference in the nature of the tasks, not to the longer length of the complex task. There was also a significant effect of observation. Both the simple and complex tasks were completed faster when participants were proximately observed. There was no significant interaction between observation and complexity. Therefore, Hypothesis 1 was not supported.

There was a significant main effect of observation: Participants typed faster when they were proximately observed than when they were alone. As the figures in Table 9 indicated that this effect might be more pronounced in the code, a post-hoc Wilcoxon test was run to assess whether the code was typed significantly faster in the proximate observation condition than in the alone condition. It was: $T = -2.38$, $p < .01$ (one-tailed), with a small effect size: $r = .04$. Therefore, typing the code was facilitated by observation.

Hypothesis 2: There will be an interaction between observation condition (proximate observation, remote observation, or alone) and task complexity (simple or complex). Both proximate

observation and remote observation will increase the speed at which participants type their name (the simple task) and decrease the speed at which they type the alphanumeric code (the complex task) in comparison to their typing speed when alone.

In order to test Hypothesis 2, and discover whether there was a social facilitation effect from remote observation, a two-way ANOVA was calculated with typing speed as the dependent variable. Observation condition (proximate observation, remote observation, or alone) and task complexity (simple or complex) were the within-subjects variables. Mauchly's test indicated that the assumption of sphericity had been violated for the main effect of observation: $\chi^2(2) = 9.76, p = .01$, and the interaction between complexity and observation: $\chi^2(2) = 7.87, p = .02$. The Huynh-Feldt correction is the appropriate adjustment to use when $\epsilon > .75$ (Field, 2005). The degrees of freedom were corrected using the Huynh-Feldt estimates of sphericity ($\epsilon = .81$ for observation and $\epsilon = .84$ for the observation and complexity interaction). The adjusted figures are reported in Table 11.

Table 11: Outcome of the 2x3 Analysis of Variance for Typing Times

Variable Main Effects	<i>df</i>	Error <i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1.61	44.85	2.07	.07	.15	.36
Complexity	1	29	21.28**	.42	>.01	.99
Interaction						
Observation x Complexity	1.69	48.90	0.50	.17	.49	.11

Note. *N* = 30.

* $p < .05$; ** $p < .01$

There was a significant effect of task complexity, even with the times of the complex task halved. The complex task was completed more slowly than the simple task, and this is due to a difference in the nature of the tasks, not to the longer length of the complex task. There was neither a significant effect of observation, nor a significant interaction between observation and complexity. Therefore, this hypothesis was not

supported. Given that there had been a main effect of observation when proximate observation and alone were considered without remote observation, two further post-hoc tests were run. The first, a Wilcoxon test, investigated whether the code was typed significantly faster under the remote observation condition than the alone condition. It was not: $T = -.15, p > .05$ (one-tailed). The second, a t test, investigated whether the code was typed faster under proximate observation than remote observation. It was: $t(29) = 2.40, p = .02$ (one-tailed), with a small effect size, $r = .14$. Therefore, the speed of typing the code was non-significantly different between the alone and remote observation conditions, but significantly different between proximate observation and alone, and proximate observation and remote observation (or marginally significant if alpha is reduced to .16 to account for the three post-hoc analyses).

The non-significant results for Hypothesis 2 might have been due to confounding personality variables, such as extraversion and neuroticism, or other individual differences, such as state anxiety, belief in psi, familiarity, age, and gender. In order to increase the sensitivity of the analysis, the personality and individual differences variables that might have introduced variance were controlled for. Participants' responses to the personality questionnaires are summarised in Table 12.

Table 12: Descriptive Statistics for the Personality Variables

Variable	<i>M</i>	<i>SD</i>	Minimum	Maximum
Extraversion	12.10	3.81	4	21
State Anxiety	32.63	7.24	30	56
Neuroticism	11.70	4.41	3	18
Belief in psi	45.13	16.79	16	72

Note. $N = 30$.

The familiarity variable was ordinal level data. The five categories of familiarity were forced onto a quasi-normal distribution, so the data has a normal distribution (hence they are not reported in Table 12).

In order to rule out multicollinearity, the inter-correlations between all these variables were assessed by two-tailed Pearson correlation coefficients. A coefficient of over .8 indicates problematic multicollinearity (Field, 2005). If two variables correlate over .8 they should be amalgamated, or one of them should be dropped. All the correlations are presented in Table 13, none of which were over .8

Table 13: Individual Differences variables Correlation Coefficients

Variables	Neuroticism	State Anxiety	Belief in psi	Familiarity	Gender	Age
Extraversion	.04	.25	.11	.22	.15	.04
Neuroticism		.48**	.20	-.39*	-.22	-.22
State Anxiety			.46**	.18	-.04	-.30
Belief in psi				.07	.16	-.12
Familiarity					.15	-.38*
Gender						-.09

Note. $N = 30$.

* $p < .05$; ** $p < .01$

The covariates were entered into an ANCOVA (general linear model) with typing speed as the dependent variable. Observation condition (proximate observation, remote observation, or alone) and task complexity (simple or complex) were the within-subjects variables. The results are reported in Table 14.

Table 14: Main Effects and Interactions from the Personality Covariates on Typing Speed

Main Effects from Variables	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	2	1.60	.07	.21	.32
Complexity	1	0.36	.16	.55	.89
Extraversion	1	0.70	.03	.41	.13
Neuroticism	1	0.38	.02	.54	.09
State Anxiety	1	1.97	.08	.18	.27
Belief in psi	1	0.33	.02	.57	.09
Familiarity	1	2.44	.10	.13	.32
Gender	1	1.70	.07	.21	.24
Age	1	1.03	.05	.32	.16
Error	22	(23393650.37)			
Interactions					
Observation x Complexity	2	1.57	.07	.22	.32
Extraversion x Observation x Complexity	2	0.38	.02	.69	.11
Neuroticism x Observation x Complexity	2	1.20	.05	.31	.25
State Anxiety x Observation x Complexity	2	0.13	.01	.88	.07
Belief in psi x Observation x Complexity	2	0.86	.04	.43	.18
Familiarity x Observation x Complexity	2	0.43	.02	.65	.12
Gender x Observation x Complexity	2	0.42	.02	.66	.11
Age x Observation x Complexity	2	1.01	.04	.37	.22
Error	44	(7727256.04)			

Note. *N* = 30.

Values enclosed in parentheses represent mean square errors.

Mauchly's test indicated that the assumption of sphericity had been violated for the main effect of observation, $\chi^2(2) = 6.90, p = .03$. Therefore, degrees of freedom were corrected using the Huynh-Feldt estimates of sphericity ($\epsilon = 1.00$) (in this case the adjusted figures were identical to the sphericity-assumed figures).

The figures in the top half of Table 14 denote the main effects. The main effects of observation and complexity in Table 14 report only the variance that is uniquely attributable to these variables, with the effects of the covariates controlled for. The main effects of observation and complexity are non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render these main effects significant, and so the non-significant results of the ANOVA in Hypothesis 2 were not due to confounding variables.

The main effects of the personality variables (in the top half of the table) show the amount of the variance in the dependent variable, typing speed, that each personality variable explains. Partial η^2 gives the effect size for that variable, not influenced by the effect of other variables included in the analysis. None of the personality variables, age, gender, or belief in psi had a significant main effect on typing speed.

The second half of the table displays the interactions for each of these predictors and observation and complexity; lower order interactions were included in the model. This shows the effect that each predictor had on the social facilitation effect (i.e., the interaction between a personality variable and observation and complexity). None of these interactions were significant: Neither extraversion, neuroticism, state anxiety, belief in psi, familiarity, gender, nor age had any significant effect on the social facilitation effect. The interaction between observation and complexity was non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render this interaction significant, and so the non-significant interaction in the ANOVA in Hypothesis 2 was not due to confounding variables.

Post-hoc analysis into task repetition, a potential confound.

As the differences in the time taken to type the simple and complex tasks did not vary significantly between the observation conditions, we ran a post-hoc analysis to investigate whether task repetition had any confounding effect. We had expected that

three repetitions would not be enough to make participants familiar with the complex task (the code). However, as one of the participants did report having memorised the code by the last round, it is possible that the repetitions reduced the complexity of the code, by making it more familiar and better practised, and therefore not such a complex task. The complex task should be both difficult and novel. As the main results were presented by observation condition, and these were randomised across the rounds, the effect of the rounds is masked. If the code was complex in the first round, but behaved as if it were a simple task by the final round, the overall effect could be null. In order to investigate whether this happened, the data for each round were separated and are presented in Table 15.

Table 15: Time taken for the Typing Tasks by Observation Condition, Complexity and Round in milliseconds (rounded to centiseconds)

		Observation condition					
Round	Task	Alone		Remote		Proximate	
		<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
1	Simple	7330	2910	7000	5040	4790	1640
1	Complex ^a	8360	4510	8240	4580	7760	3220
2	Simple	2620	550	3770	1500	3930	2610
2	Complex ^a	6040	2350	7700	2990	5520	3300
3	Simple	3880	1110	2790	1240	3090	1260
3	Complex ^a	7040	3490	6120	5330	4990	2050

Note. *N* = 10 in each round.

^aThese values are halved.

A visual inspection of the data in Table 15 reveals that there are differences across the rounds. In the alone and remote observation conditions the name is completed much faster in the second and third rounds compared to the first. This could be the result of practice effects. The timing program, as noted above, timed from when the input box appeared on the screen, not from when the participants started typing into it. It timed until the participants pressed return, not when they stopped typing, so there was a risk of noise, especially in the first round and in the name, as it was the

first task. The faster typing speed for the name under proximate observation in the first round suggests that proximate observation made participants begin typing sooner. In the proximate observation condition the complex task gets faster across the rounds to a greater extent than in the other observation conditions. In the third and final round the complex task is completed fastest under proximate observation: the pattern that is expected for a simple task.

To assess whether the repetition across the rounds affected the results, a three-way ANOVA was calculated with typing speed as the dependent variable. Observation condition (proximate observation, remote observation, or alone), and round (first, second, or third) were the between-subjects variables and task complexity (simple or complex) was the within-subjects variable. The results are presented in Table 16.

Table 16: Outcome of the 3x2x2 Analysis of Variance for Typing Times

Variable Main Effects	<i>df</i>	Error <i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	2	81	1.30	.03	.28	.27
Complexity	1	81	42.00**	.34	<.01	1.00
Round	2	81	9.83**	.20	<.01	.98
Interactions						
Observation x Complexity	2	81	0.26	.01	.77	.09
Complexity x Round	2	81	1.00	.02	.37	.22
Observation x Round	4	81	.79	.04	.54	.24
Observation x Complexity x Round	4	81	1.08	.05	.37	.33

Note. *N* = 10 in each round.

* *p* < .05; ** *p* < .01

According to the results in Table 16, there were main effects for complexity and for round. The main effect for complexity is consistent with the findings so far: Participants typed the simple task (name) faster than the complex task (code). The main effect for round indicates that participants typed faster as the rounds progressed. This might have been due to practice effects, if participants improved at the typing task with practice. The non-significant interactions between round and

observation, round and complexity, and all three combined indicate that, although participants typed faster as the rounds progressed, there was no difference in their relative speed between observation conditions and task complexities.

7.5. Discussion

This experiment aimed to investigate whether people nonintentionally respond to psi-mediated observation in the same way that they do to proximate observation that they have sensory knowledge about. In other words, Experiment 3 investigated whether there is a social facilitation effect from remote observation. The participants completed typing tasks that were presented as being the log-in to another experiment, and so should not have elicited evaluation apprehension. The participants typed both a simple task (their first name) and a complex task (an alphanumeric code) three times, under a different observation condition each time: alone, remotely observed, or proximately observed. We assessed whether there was simple task facilitation and complex task inhibition from proximate observation compared to being alone, in other words, whether there was a normal social facilitation effect. There was no significant interaction between observation condition and task complexity, and so the first hypothesis was not supported. The design of Experiment 3 was based upon a previous study that found a significant social facilitation effect (Schmitt et al., 1986). Experiment 3 differed from Schmitt et al.'s experiment in that it had a remote observation condition instead of mere presence, and a within-subjects design. On further investigation, more differences between Experiment 3 and Schmitt et al.'s experiment came to light. These differences might explain the null findings.

The name and code tasks in Experiment 3 were intended to replicate the tasks used in Schmitt et al.'s (1986) experiment. The tasks in Schmitt et al.'s experiment took longer (the name took between seven and 15 seconds, and the code between 52 and 73 seconds, compared to between four and five seconds for the name and around seven seconds for the code in Experiment 3). This indicated that the tasks in the

previous experiment were longer. Schmitt et al. had not reported whether they used forenames or full names in their typing tasks. Goore confirmed that they had used full names, and excluded names less than 10 letters long (Goore, 2002). Six of the names in Experiment 3 were very short, just three letters long, and only one name, the longest, was 10 letters long. Thus there was very likely a ceiling effect in the simple typing task, from participants not being able to type their names any faster, which would limit how much of an effect observation could have. With such short times for these tasks, there was probably also inadequate lability for any observation effects to make a difference. Participants in Experiment 3 typed the code considerably faster than the participants in Schmitt et al.'s experiment. While this was no doubt partially due to the code being shorter, it might also have been an inadequately complex task. There is additional evidence for the code acting as a simple task, rather than a complex task, from the main effect of observation in the two-way ANOVA comparing only proximate observation and alone in the analysis for Hypothesis 1. The post-hoc test revealed that the speed of typing the code was increased by proximate observation. If it were acting as a complex task, we would expect observation to decrease the speed of typing the code.

We had assumed that the code, being a complex task, would be inhibited by proximate observation, as it had in Schmitt et al.'s (1986) experiment, but there were differences in the way the code was used in Experiment 3 that might have reduced its complexity. Importantly, as discussed above, the tasks were shorter, which would have made the code easier. The other pertinent difference between Experiment 3 and Schmitt et al.'s experiment was the repetition of the tasks. Participants had some practice with the code by the final round, and one criterion of a simple task is that it is well-practised (Zajonc, 1965). A complex task should be novel or difficult (Schmitt et al., 1986). Although the manipulation check revealed that none of the participants had typed the code prior to the experiment, they might have become familiar with it by the end of the experiment. For example, one participant reported having memorised the code by the final round. Also, most of the participants also

mentioned that the both the typing tasks became easier and faster throughout the experiment. Accordingly, participants got significantly faster from the first to the third round. Although we had thought that three repetitions would not be enough to make participants familiar with the code task, it may have been a much simpler task by the third round. Future experiments should not repeat tasks that are intended to be complex.

The last difference between Experiment 3 and Schmitt et al.'s (1986) experiment was that the experimenter wrote the code down for the participants. The code was, therefore, visible throughout the experiment. This changed the task from having to work out the code and type it, to just copying it. A participant in the pilot test gave feedback that the fact that the code was copied meant that it was not a particularly difficult task; unfortunately, this feedback was not given until after all the experiment trials had been run. We had elected to use an informal way of presenting the code, a verbal explanation and a hand-written reminder, to make this task appear to be an incidental part of the set-up of the colour and precognition task. Schmitt et al. had not specified how the code instructions were given to their participants. This was clarified by Goore (2002) "The first time they received the 'code word' instructions was during the experiment. The instructions were presented on screen, and the instructions also contained an example... We made sure there were no paper or pencils around so the task would have to be completed in one's head." We had assumed that the code was a complex task in itself, irrespective of how was presented. This assumption was incorrect. It might be important that participants have to work out the code: This would increase the cognitive load of the task (G. A. Miller, 1956) and the amount of working memory capacity it used. This might make it a more difficult task. In all, it appears that the code may not have been difficult enough to function as a complex task. Task difficulty may be an important aspect of complex tasks. For example, in a previous experiment that found the social facilitation effect, participants rated the complex task as significantly more difficult than the simple task (Feinberg & Aiello, 2006). On the other hand, Markus (1978)

found significant inhibition effects from proximate observation when participants were dressing and undressing in unfamiliar clothes, which is not difficult, only unfamiliar. Zajonc (1965) categorised tasks by whether they were well-learned (familiar, i.e., simple) or still being learned (novel, i.e., complex). The critical factor distinguishing these categories was, he proposed, that the participants' dominant responses (the responses people are most likely to make) were incorrect for complex tasks. This could be achieved by not repeating the code, and making it more difficult. Future experiments should ensure that the complex task is more difficult than the code in Experiment 3, and check that the participants find it difficult. Future experiments with these tasks should also replicate more closely the conditions in Schmitt et al. (1986).

The observation influence in Experiment 3 might have differed from Schmitt et al.'s (1986) experiment, because in Schmitt et al.'s experiment the observation conditions were between-subjects. In Experiment 3 the observation conditions were within-subjects, so all participants experienced proximate observation, remote observation, and alone conditions. This changing between conditions might have "contaminated" the alone condition. There is a potential precedent for this. Guerin (1993) criticised Sanna & Shotland's (1990) experiment, which did not find a performance effect from mere presence, and in which the experimenter entered the room frequently to administer the tasks, for having a contaminated alone condition. In Experiment 3 we minimised the amount that the experimenter had to enter the room by designing a fully automated program for the tasks, and therefore avoided that problem, but the within-subjects design might still have affected the observation conditions. It is not known how long the influence of observation continues after observation ceases (Aiello & Douthitt, 2001; Guerin, 1993). If the influence of one observation condition lingers, this could have reduced the differences between the observation conditions.

So far, the comparisons between Experiment 3 and Schmitt et al. (1986) have been negative, focussed on the failure to replicate the social facilitation effect, but Experiment 3 merits positive appraisal for creating a true alone condition. The criteria for a true alone condition were that participants should be physically alone, the tasks they perform should not elicit feelings of being evaluated or of self-evaluation (Markus, 1978), and there should not be covert observation in the alone condition. All of these criteria were fulfilled. Although some of the participants said they suspected that the name and code may have had more to do with the experiment than just being the log-in, the way that the question was phrased also told them that this was the case. When asking this question, the experimenter apprehended that some participants wanted to give the impression that they had realised that the name and the code were involved, rather than that they had actually thought this at the time. This is a limitation of this manipulation check, and future experiments should not use leading questions.

The way that the name and code were timed may also have been problematic. The E Prime program could not time each keystroke. It timed from the participants pressing the space bar, in order to open the screen into which they typed their name or code, to when they typed return at the end of the name or code. Thus the times recorded include any delay before starting to type, any time for corrections, and any delay before pressing return, in addition to the time taken to type. These added noise to the data, which could not be separated out. The E Prime program also only recorded the final product of the name and code typing, not any of the corrections. This means that there might be noise from corrections that we could not separate out. When she was observing, the experimenter noticed that some participants paused between finishing typing and pressing return. This would have happened more in the first round, as in the later rounds the participants would have known what to do. Although the first round was equally spread across the observation conditions, the extra time added by these delays may have obscured any social facilitation effect. The delays would have had relatively less of an effect, the longer the name and code were. To

overcome this limitation more accurate task recording and timing software should be used.

Experiment 3 failed to replicate the normal social facilitation effect of simple task facilitation and complex task inhibition from proximate observation. This effect was intended to be a benchmark, against which the effect of remote observation would be compared. We thought it likely that the effect of remote observation would be similar in direction, but lesser in magnitude, to the effect of proximate observation. Alternatively, remote observation could have produced a completely different effect. There was, however, no social facilitation effect from remote observation; Hypothesis 2 was not supported. As the normal social facilitation effect was not found, it is unlikely that Experiment 3 had any chance of detecting a remote observation effect. Even when the possible confounding individual differences measures were controlled for there was no effect. Future experiments investigating remote observation and social facilitation should improve upon the limitations identified here to improve the chances of finding the social facilitation effect.

In summary, the lack of a social facilitation effect between the alone and proximate observation conditions prevented there being a baseline with which to conceptually calibrate the effect of remote observation. A variety of limitations in the experiment, notably the repetition of the rounds, length and ease of the tasks, and inaccuracies in the timing program were identified as possible reasons for the null results and should be improved.

7.6. Summary of Experiment 3 and Rationale for Experiment 4

In summary, Experiment 3 investigated whether there was a social facilitation effect from remote observation compared to alone and proximate observation conditions. The results were inconclusive, as the social facilitation effect of simple task facilitation and complex task inhibition was not found between the alone condition

and the proximate observation condition. The lack of the social facilitation effect between the alone and proximate observation conditions in Experiment 3 prevented there being a baseline with which to compare the effect of remote observation. A variety of limitations were identified, and Experiment 4 aimed to address each of these, in order to test whether the normal social facilitation effect (between proximate observation and alone) could be replicated with covert typing tasks.

Experiment 3 did not replicate the procedure of Schmitt et al.'s (1986) experiment, specifically in terms of task length, repetition, and presentation. In Experiment 4 participants used their full names in the name and code log-in tasks. This not only made the typing tasks longer, increasing the chance of finding a social facilitation effect, but also aimed to make them more difficult, as they might have been too easy in Experiment 3. In Experiment 4 the participants did not repeat the name and code log-in tasks, because practice effects and increased familiarity from repeating the tasks in Experiment 3 may have interfered with the social facilitation effect. Lastly, the code in Experiment 4 was not visible throughout the experiment and could not be copied: Participants had to work the code out in their heads. To check that these manipulations worked, participants were asked to rate how difficult and familiar these tasks were.

Experiment 3 also differed from Schmitt et al.'s (1986) procedure in terms of the observation conditions. In Experiment 3 these were within-subjects, and the change from one condition to the other might have interfered with any observation condition exerting an influence. In Experiment 4, as in Schmitt et al.'s experiment, the observation conditions were between-subjects. With these changes to the task length, repetition, presentation and design, Experiment 4 should closely replicate the procedure that obtained a social facilitation effect in Schmitt et al.'s experiment.

One limitation of the apparatus used in Experiment 3 was the inaccuracy in the timing. There was a risk of noise in the timing data. In Experiment 4, a bespoke

timing program was used that timed individual keystrokes with millisecond accuracy (see Materials and Apparatus below). A second limitation in the apparatus of Experiment 3 was that the individual keystrokes made by participants were not recorded, only the final product. Therefore, a thorough assessment of accuracy could not be made, as corrected errors were not recorded. In Experiment 4, every keystroke made by the participants was recorded. This allowed the accuracy of the typing tasks to be assessed as well as the speed. It is advantageous to assess both speed and accuracy in social facilitation experiments because speed is the most reliable measure for simple tasks, and accuracy for complex tasks (C. F. Bond & Titus, 1983). Measuring both speed and accuracy maximises the chances of capturing the social facilitation effect.

In addition to accuracy, *perceived difficulty* was used as an indicator of the social facilitation effect in Experiment 4. It is possible that observation, as well as affecting the performance of tasks, might make them seem more difficult or easier to do (Bradner & Mark, 2001). Bradner and Mark compared participants' performance on a complex maths task between two observation conditions. These were alone, and observation by two-way video links in which the observer and participant could see each other's image on computer screens in separate rooms. The maths task was performed more slowly and rated as being more difficult in the two-way video observation condition compared to alone. Therefore, from this limited previous research, it appears that complex tasks are not only performed as if they are more difficult (more slowly) with observation, but are also perceived to be more difficult. This makes participants' ratings of task difficulty an indicator of whether there is an effect from observation. It is not known whether or how this effect would apply to simple tasks, i.e., whether they would be rated as easier when observed, but this seemed logical. The way perceived difficulty responds to observation in both simple and complex tasks is investigated in Experiment 4.

One more indicator of the effect of observation was introduced in Experiment 4. As reviewed in Chapter 6, the social facilitation effect is thought to be mediated by observation leading to an increase in the participants' arousal (Moore & Baron, 1983). State anxiety can be used as a self-report measure for arousal level (Mullen et al., 1997). In a change from Experiment 3, in which state anxiety was used as an individual difference that might interact with the social facilitation effect, in Experiment 4 state anxiety was used as an indicator of the influence of observation on arousal. This was achieved by asking participants retrospectively to rate their state anxiety during the experiment. The state anxiety section of the STAI is valid if used to refer to the present moment, or to a recent time in the past (Spielberger, 1983). Therefore, it is valid as a retrospective measure. State anxiety was expected to increase under proximate observation.

In Experiment 3, personality characteristics and individual differences that can affect both task performance and how observation affects people's task performance were measured. As a conservative measure, the potential effect of these individual differences was controlled for when assessing whether a social facilitation effect was found. Extraversion, neuroticism, gender, and age were retained for Experiment 4. Belief in psi was not used in Experiment 4, as there was no remote observation condition. The indicator of the relationship between the participants and the observer (familiarity) was also not used. Instead, the relationship between the participants and the observer was kept constant by using participants that the observer did not know well, or did not know at all. The relationship between the participants and the observer was also kept constant by ensuring that the observer had expert status to the participants. Both unfamiliar and expert observers should have a greater social facilitation effect than familiar (Guerin, 1993) or peer observers (Henchy & Glass, 1968; F. G. Miller et al., 1979).

In Experiment 4 (below) participants typed their full name (simple task) and a code made up of the letters of their name backwards interspersed with ascending digits

(complex task) once, which they were told was the log-in to a subsequent experiment. Following the typing tasks, the participants then completed a “Perceptual Judgement” experiment that was a decoy, intended to deceive the participants that the name and code were only a log-in to the “real” experiment. (The results of the perceptual judgement task were reported in Chapter 3). Participants were either alone or proximately observed when they typed the name and code. The order of the observation conditions was randomised and counterbalanced. Firstly, we asked whether the normal social facilitation effect was found, by investigating whether there was an interaction between observation condition and task complexity on typing speed. We controlled for possible confounding personality and individual differences variables. Secondly, we investigated whether there was an interaction between observation condition and task complexity on typing accuracy. Thirdly, we investigated whether there was an interaction between observation condition and task complexity on perceived task difficulty, rated by asking participants how difficult the tasks were. Lastly, we investigated whether there was an effect of observation on state anxiety.

7.6.1. Hypotheses.

1 There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of typing speed. Participants in the proximate observation condition will type their names (the simple task) faster than participants in the alone condition. Participants in the proximate observation condition will type their alphanumeric code (the complex task) more slowly than participants in the alone condition. In other words, proximate observation will elicit the social facilitation effect.

2 There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of typing accuracy. Participants in the proximate observation condition will type their names

(the simple task) more accurately than participants in the alone condition. Participants in the proximate observation condition will type the code (the complex task) less accurately than participants in the alone condition. In other words, proximate observation will elicit the social facilitation effect.

3 There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of perceived task difficulty. Participants in the proximate observation condition will rate typing their names (the simple task) as easier, and typing the code (the complex task) as more difficult, than participants in the alone condition will rate these same tasks.

4 Participants in the proximate observation condition will rate their state anxiety higher than participants in the alone condition.

7.7. Method

7.7.1. Design.

The design was a mixed model with two independent variables: observation condition (alone or proximate observation) between-subjects, and task complexity (simple or complex) within-subjects. The dependent variables were typing speed, typing accuracy, perceived task difficulty ratings, and state anxiety scores.

7.7.2. Participants.

Fifty participants, ranging from 17 to 45 years of age ($M = 23.33$, $SD 5.11$ years), 28 of whom were female and 22 male, volunteered. None of the participants from Experiment 3 were included, neither was anyone who might know about the aim of the experiment. Participants were recruited from tutorial groups and lecture theatres, ostensibly for an experiment in perceptual judgement. The participants were predominantly 1st and 2nd year psychology students, who had not yet studied social facilitation or parapsychology. Some were taught by the experimenter. They were

recruited in the first tutorial and so did not know her well, and knew her as a tutorial teacher, thus in a position of authority. Therefore, the participants and the experimenter/observer were unfamiliar to each other, and the observer was an expert rather than a peer in status.

Experimenter and Observer.

The author took the role of experimenter and observer.

7.7.3. Ethical Considerations.

We identified one potential ethical issue, deliberate deception of the participants, similarly to Experiments 1, 2, and 3 (see above and Chapters 3 and 4). We addressed this by giving participants the right to terminate the experiment at any time and a full debrief following the experiment. The Psychology Research Ethics Committee at the University of Edinburgh granted ethical approval for this experiment to proceed.

7.7.4. Materials and Apparatus.

Program.

The experiment was programmed in Microsoft Excel XP and Shockwave by the author in conjunction with a computing expert at the University of Edinburgh. The Excel program presented the instructions for the perceptual judgement task and the name and code. The time at which the participant pressed each key (*key down*) and the time of releasing each key (*key up*) were logged, along with each letter or digit typed. In order to assess how accurately the custom-made Excel program measured timing, the processing speed of the computer and the accuracy of recording the responses were considered. The processing speed of all modern computers is fast enough to ensure millisecond accuracy in timing (Wikipedia Clock Rate, 2002). The relative accuracy of the keyboard and mouse were compared against E Prime's

benchmarking program (E Prime, 2002) and were found to be accurate to beyond 100th of a millisecond.

Personality Questionnaires.

The 240-item NEO PI-R (Costa & McCrae, 1992a) was used to score extraversion and neuroticism. High scores indicate high levels of that trait, e.g., high extraversion, and the maximum score for a trait was 192.

To measure state anxiety participants completed the 20-item state anxiety section of the STAI (Spielberger, 1983). This scale has a minimum score of 20 and a maximum score of 80; high scores indicate high state anxiety.

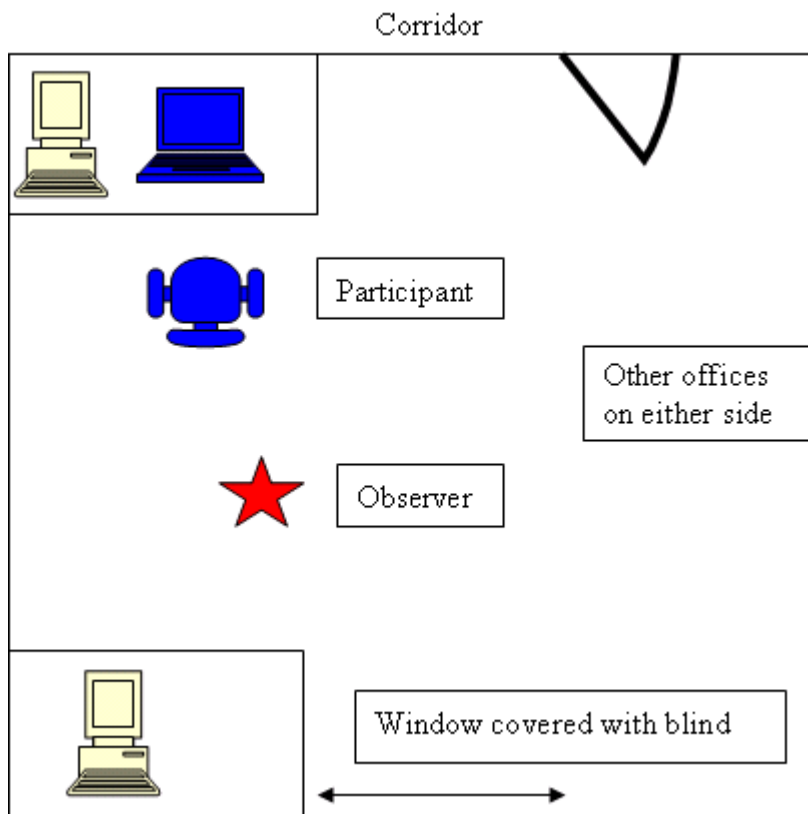
7.7.5. Procedure.

Randomisation of observation conditions.

The order of the observation conditions had been determined in advance of all trials using random entry into random number tables (RAND, 1966). The entry point was taken from the random number function on a hand held Casio calculator. The first two digits determined the page number, and the third digit the line at which the sequence began. Starting from this random entry point, the next 25 digits to the right were converted into observation conditions as follows: an even number became observe one participant, leave the next alone; an odd number became leave one participant alone, observe the next. This created a randomised, counterbalanced order of observation conditions. These observation conditions were printed on a list. Participants were assigned to the next observation condition in the order in which they arrived to do the experiment. The experimenter was not blind to the list of observation conditions.

Session procedure.

Participants were tested individually. Participants were greeted at the door to the Department of Psychology at the University of Edinburgh, and shown to a waiting room in which they completed the NEO PI-R, which took between 20 and 35 minutes. The waiting room was the hospitality room for the parapsychology suite, just along the corridor from the experimental room. Participants also signed a standard consent form agreeing to participate, and on the understanding that they could terminate the experiment at any time (none chose to). The experimenter then showed participants into the experimental room. One office in the Department of Psychology was used for testing. This office had two desks, each with one desktop computer on it. There was a dormer window, which was covered by a blackout blind for the experiment. The light was always switched on in the room to maintain a constant level of light across sessions. Participants sat at the desk with the experimental computer, a Sony VAIO Laptop, and another desktop computer, which always had the monitor switched off (see Figure 6).



The star marks the position of the observer in the proximate observation condition.

Figure 6: Map of the Experimental Room for Experiment 4 (not to scale)

The experimenter explained the instructions for the perceptual judgement task to the participants, and explained that they would also have to log-in to the experiment, but that the instructions for that were in the program. That was all that was said about the name and code. An example name and code was given in the instructions, but was not visible on the screen when the code was inputted. The example was: “Name: Bilbo Baggins, Code: s1n2i3g4g5a6b7o8b9110i11b12”. The instructions requested participants use just lower case letters in the code. We chose a fantasy name to reduce the chances of recruiting anyone with the same name, which might have given them an advantage in typing their code. In a change from Experiment 3 the

name and code were entered on the same screen, into two input boxes similar in appearance to an email log-in. Pilot tests had confirmed this was more naturalistic, an important consideration as participants were not supposed to realise that this was part of the experiment or feel evaluated at this point. The experimenter asked participants to fetch her from the waiting room at the end of the experiment.

According to the observation condition the experimenter stayed in the room up to the end of the inputting of the name and code (proximate observation), or left before the participants began the name and code (alone). In the proximate observation condition the experimenter stood in the experimental room with the participants. She stood about a metre behind them, and slightly to one side. She could see the participants, their hands on the keyboard, and the screen in front of them. In the alone condition participants were alone in this room, with the door closed. No-one else entered the room. The experimenter occupied her mind with other matters during this time. After the participant completed the log-in (name and code), the perceptual judgement task started automatically, and the participants went on to generate data that is not analysed in this chapter (see Chapter 3). At the end of the perceptual judgement task the participants fetched the experimenter, who was in the waiting room. The experimenter asked the following questions verbally to check that experimental manipulations had worked, and wrote down the participants' answers.

1 “How did you find it?”

2 “At the time of doing the name and code log-in, did you think that they were part of the experiment?” (If participants needed clarification “part of the experiment” was further defined as being an experimental task. This question was introduced after the first 20 participants had done the experiment, prior to which participants were just invited to comment on the name and code but no scripted question was asked.)

3 Participants were asked to rate the difficulty of the name and code tasks on a nine-point Likert scale anchored at 1 (*easiest*) and 9 (*most difficult*).

4 “How often do you type your full name?”

5 “Have you ever typed the code before?”

6 Participants who had been alone were asked “What was the effect of being alone on how you did the name and code?” Participants who had been observed were asked “What was the effect of being observed on how you did the name and code?”

Then participants completed the STAI and another questionnaire relevant to the perceptual judgement task. The STAI was completed first to ensure that the way they had felt in the experiment was fresh in their minds. They were asked to fill this out for “how you felt at the beginning of the experiment, before the lines task, around the time of doing the log-in”. Participants were thanked for participating, and were offered a sweet from a lucky dip box as a gesture of thanks before they were shown out of the experimental room. Only after all participants were tested and the results were analysed were the participants debriefed by email.

7.7.6. Analyses.

Analyses were conducted in SPSS version 14 and Microsoft Excel.

7.8. Results

Manipulation check 1: Did participants believe that the typing tasks were not part of the experiment?

As soon as the experimenter returned to the room after the experiment was over she asked “How did you find it?” This informal question was part of the manipulation check to see what participants referred to as having been the experiment (the name and code log-in or the perceptual judgement task). There was feedback recorded for

45 of the 50 participants, for this and all the other questions. Five of the response sheets (anonymised) were lost prior to coding due to a break-in.

Forty-one participants (89%) referred only to the perceptual judgement task. One referred to the log-in task, and one to both tasks. Two others commented on other aspects of the experiment. Therefore, the majority of the participants interpreted the experiment as being the perceptual judgement task only, and not the log-in, which indicated that they did not think that the log-in procedure was part of the experiment. This would mean that the perceptual judgement task functioned well as a ruse, and the participants should not have felt evaluation apprehension (an arousal-increasing influence from the expectation of observation/judgement) when typing the name and code.

From participant 20 onwards, participants were also asked “At the time of doing the name and code log-in, did you think that they were part of the experiment?” Of the 30 participants who were asked this question, 18 (60%) did suspect the code of being something to do with the experiment, with varying degrees of certainty, including just thinking there was a slight possibility. Twelve participants (40%) did not suspect at all. Only one participant commented “I didn’t think. I just did it, so used to logging in”, which was how we had hoped participants would react. Six of the participants (20%), who suspected the tasks were part of the experiment, guessed that they were being timed. Seven of the participants (23%) reported suspecting the log-in tasks because the code was “complicated” or “weird”.

Participants who were not asked this question had a chance to comment on the code, and four (20%) reported suspicion: one because this task was complicated, one guessed it was being timed, one thought it was being observed (he was in the observed condition), and one was only suspicious because he was being observed. In summary, the majority of the participants did think that the typing tasks were, or could be, part of the experiment. This means that the cover story of it being the log-

in to the psi task did not work as effectively as in Experiment 3, and there might have been a risk of evaluation apprehension from participants assuming that their typing was being monitored, or realising that it was being timed (Griffin, 2001; Griffin & Kent, 1998). Thus the alone condition in Experiment 4 may have been contaminated by evaluation apprehension, and therefore would not have been a true alone condition (Markus, 1978). This finding had not emerged from the answers to the first manipulation check question.

Manipulation check 2: Was the name easy and the code difficult?

Participants rated the difficulty of the typing tasks on a nine-point Likert scale anchored at 1 (*easiest*) and 9 (*most difficult*). Three participants did the code incorrectly; one did not use numbers, and just typed her name backwards; one used only the number 1, repeatedly. One other did the numbers correctly, but did not reverse the order of the letters of her name in the code. These three participants' ratings were excluded from the comparison. Five participants typed the code in a different way, which was not actually incorrect, but was not how we had intended they would type it. Instead of typing it sequentially (the last letter of their surname first, then the number 1...) they typed their name backwards, then side-spaced through the name adding the numbers. This might have made the task easier, reducing its complexity. Their ratings are given separately in Table 17 and are called *shortcut*, because they took a shortcut on the code.

Table 17: Perceived Difficulty of Simple and Complex Tasks.

Task	<i>N</i>	<i>M</i>	<i>SD</i>
Simple	45	1.24	0.61
Complex	37	6.09	1.48
Shortcut	5	5.00	2.00

As the figures in Table 17 indicate, the code was rated as being more difficult than the name. To test whether this difference was significant we ran a Wilcoxon signed ranks test (data for the name were highly positively skewed), which showed that the name was significantly easier than the code: $z = -5.40, p < .01$. The ratings for the name are towards the far end of the easy side of the scale ($M = 1.24$), but the code ratings are nearer the midpoint than the far end of the difficult side ($M = 6.09$). Therefore, although the code was more difficult than the name, the code was not a very difficult task in absolute terms.

The shortcut of the code was not significantly easier than the code: $t(41) = -1.49, p = .29$. The non-significant difference between the difficulty of the code and the shortcut of the code is important for Experiment 3. In Experiment 3 the final output was the only record of the typing tasks. It was not known how many people had taken shortcuts with the code. If, for example, the shortcut was much easier than the code, and if many participants had used the shortcut in Experiment 3, that might have reduced the complexity of the code in Experiment 3. As the difference between the ratings in Experiment 4 is non-significant, it is likely that any shortcuts in Experiment 3 did not significantly alter the difficulty of the code task. The participants who did the shortcut were excluded from further analysis in Experiment 4 because we had decided in advance to exclude all those who did not strictly adhere to the way the code was supposed to be typed.

Manipulation check 3: Was the name familiar and the code novel?

Participants were asked “How often do you type your full name?” Thirty-three of the 45 participants (73%) typed their full name at least once a day and all the other participants typed their full name at least once a week. The name was therefore a familiar, well-practiced task. Participants were also asked “Have you ever typed the code before?” None of the participants had typed the code before, although one

participant reported that she knew how to type her first name backwards, and another that he typed backwards as a child. Therefore, the code was a novel task.

Hypothesis 1: There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of typing speed. Participants in the proximate observation condition will type their names (the simple task) faster than participants in the alone condition. Participants in the proximate observation condition will type their alphanumeric code (the complex task) more slowly than participants in the alone condition.

Typing speed is the mean speed of typing each letter or digit. Typing speed was calculated as the *inter-stroke interval*, the average time elapsed between pressing one key (letter or digit) to pressing the next key. This used the *key down* times, not the *key up* times. The time taken to start typing was excluded, as was any time taken between finishing the name and moving on to the code, as these were often long delays that could misleadingly decrease the mean speed. The time of pressing the shift key to capitalise the first letter of the name was also excluded as not all participants had used capitals, and the very short duration between pressing shift and the first letter of the name would have increased the mean speed misleadingly. Time taken to make and correct errors was also included in the calculation of mean inter-stroke interval, in line with the following examples. If Sam typed *samm-backspace* each of those keystrokes were included in the calculation. If, on the other hand, Sam typed *stam watson* then rapidly side-spaced (using the left arrow key) back through the name to remove the *t*, then the rapid left arrow presses were not included as they were not part of the typing of the name, and would have misleadingly increased the mean speed.

Participants' responses were excluded if they had used the shortcut of the code, typed the code wrong, or if they had outlying data points with some justification for removal. (Five participants typed much slower, two were dyslexic, and three spoke English as a second language.) Data used in the analysis were normally distributed. The descriptive statistics are shown in Table 18 and displayed graphically in Figure 7.

Table 18: Time take to Type Simple and Complex tasks by Observation Condition

Task	Observation Condition				Row Means
	Alone <i>N</i> =17		Proximate <i>N</i> =20		
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	
Simple	362.47	151.29	320.9	104.53	522.92
Complex	1505.06	269.69	1363.4	224.73	2186.76
Column Means	933.77		842.15		1354.84

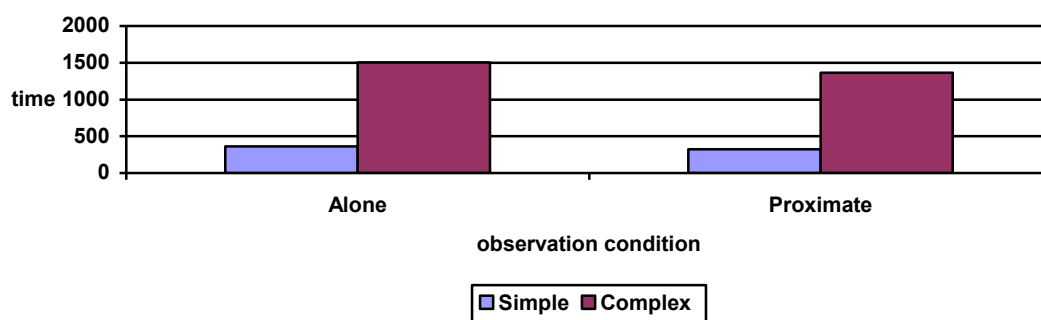


Figure 7: Mean Typing Speed in milliseconds by Task Complexity and Observation Condition

From visual inspection of the figures in Table 18 and Figure 7 it would appear that the speed of both the name and code were increased by observation, and that this effect was more pronounced in the code. In order to test Hypothesis 1, a two-way

ANOVA was calculated with typing speed as the dependent variable. Observation condition (proximate observation or alone) was the between-subjects variable and task complexity (simple or complex) was the within-subjects variable. The ANOVA results are displayed in Table 19.

Table 19: Analysis of Variance for Typing Times

Variable Main Effects	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	3.55	.09	.07	.45
Complexity	1	652.93**	.95	<.01	1.00
Error	35	(455.19)			
Interaction					
Observation x Complexity	1	1.37	.04	.25	.21

Note. $N = 37$.

Values enclosed in parentheses represent mean square errors.

* $p < .05$. ** $p < .01$.

There was a significant main effect of complexity: The complex task was completed more slowly than the simple task. As the mean inter-stroke intervals, which are unaffected by the length of the task, were used in this analysis this main effect was due to differences in complexity, not the differences in the length of the task. There was no significant main effect of observation, although this was approaching significance. There was no significant interaction. Hypothesis 1, that observation would increase the speed of the simple task and decrease the speed of the complex task, was not supported. There was a slight trend instead for observation to increase the speed of both tasks, but this was non-significant.

As there was a trend towards observation increasing the speed at which both tasks were typed, and because the figures in Table 18 indicated that this effect might be more pronounced in the code, a post-hoc *t* test was run to assess whether the code was typed significantly faster in the observation condition. It was: $t(35) = 1.74$, $p < .05$ (one-tailed), with a small effect size, $r = .08$. Therefore, typing the code was facilitated by observation.

The non-significant results for Hypothesis 1 might have been due to confounding personality variables, such as extraversion or neuroticism, or other individual differences, such as gender or age. In order to increase the sensitivity of the analysis, the personality and individual differences variables that might have introduced variance were controlled for. Participants' responses to the personality questionnaires are summarised in Table 20. A sub-set of personality questionnaires ($N=29$ for the NEO PI-R, $N=22$ for state anxiety) were lost prior to coding due to a break-in, and after it was no longer practical to ask participants to repeat them. The remaining data are shown below.

Table 20: Descriptive statistics for the Personality and Individual Differences variables

Variable	<i>N</i>	<i>M</i>	<i>SD</i>	Minimum	Maximum
Extraversion	21	119.78	19.81	79	154
State Anxiety	28	40.17	8.34	27	56
Neuroticism	21	103.22	24.76	59	147

In order to rule out multicollinearity, the inter-correlations between all these variables were assessed by two-tailed Pearson correlation coefficients. A coefficient of over .8 indicates problematic multicollinearity (Field, 2005). If two variables correlate over .8 they should be amalgamated, or one of them should be dropped. All the correlations are presented in Table 21, none of which were over .8.

Table 21: Correlation Coefficients for Individual Differences variables

Variable		Neuroticism	Gender	Age
Extraversion	$N = 21$	0.02	0.11	-0.28
Neuroticism	$N = 21$		0.29	0.09
Gender	$N = 48$			0.02

The covariates were entered into an ANCOVA (general linear model) with typing speed as the dependent variable. Observation condition (proximate observation or

alone) was the between-subjects variable and task complexity (simple or complex) was the within-subjects variable. The results are reported in Table 22.

Table 22: Main effects and Interactions from the Individual Differences predictors on Typing Times

Main Effects from Variables	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	2.68	.31	.15	.28
Complexity	1	3.06	.34	.13	.31
Extraversion	1	0.12	.19	.74	.06
Neuroticism	1	0.19	.03	.68	.07
Gender	1	1.36	.19	.29	.17
Age	1	1.52	.03	.71	.06
Error	6 (44396.86)				
Interactions					
Observation x Complexity	1	0.14	.02	.72	.06
Extraversion x Observation x Complexity	1	0.08	.01	.79	.06
Neuroticism x Observation x Complexity	1	0.12	.02	.74	.06
Gender x Observation x Complexity	1	0.02	<.01	.64	.07
Age x Observation x Complexity	1	1.26	.17	.30	.16

Note. $N = 19$.

Values enclosed in parentheses represent mean square errors.

The figures in the top half of Table 22 denote the main effects. The main effects of observation and complexity in Table 22 report only the variance that is uniquely attributable to these variables, with the effects of the covariates controlled for. The main effects of observation and complexity are non-significant. Controlling for the variance attributable to the personality and individual differences measures rendered the main effect of complexity, significant in Table 19 above, non-significant. (It is more likely that this non-significant finding is due to the low power in this analysis than that the finding in Table 19 was artefactual.)

The main effects of the personality variables (in the top half of the table) show the amount of the variance in the dependent variable, the typing speed, that each

personality variable explains. Partial η^2 gives the effect size for that variable, not influenced by the effect of the other variables included in the analysis. Neither extraversion, neuroticism, gender, or age had a significant main effect on typing speed.

The second half of the table displays the interactions for each of these predictors and observation and complexity; lower order interactions were included in the model. This shows the effect that each predictor had on the social facilitation effect (i.e., the interaction between a personality variable and observation and complexity). None of these interactions are significant: None of the personality or individual differences variables had any significant interaction with the social facilitation effect. The interaction between observation and complexity was non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render this interaction significant, and so the non-significant interaction in the ANOVA in Hypothesis 1 was not due to confounding variables.

Hypothesis 2: There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of typing accuracy. Participants in the proximate observation condition will type their names (the simple task) more accurately than participants in the alone condition. Participants in the proximate observation condition will type the code (the complex task) less accurately than participants in the alone condition.

Errors were counted in two ways: as the number of incorrect letters or digits that remained and the number of incorrect letters or digits that had been corrected. For example, if Sam typed *Sams Wattson* then side-spaced with the left arrow and corrected the double *t*, he scored 1 for remaining errors (the *s* on Sam) and 1 for corrected errors (the double *t*): a total error score of 2. Participants who used the

shortcut in the code or typed the code incorrectly were excluded. The descriptive statistics are shown in Table 23.

Table 23: Total Errors by Observation Condition and Task Complexity

Task	Observation Condition				Row Means
	Alone <i>N</i> = 19		Proximate <i>N</i> = 22		
	Mean	SD	Mean	SD	
Simple	0.26	0.73	0.18	0.4	0.22
Complex	1.26	1.85	0.86	1.21	1.06
Column Means	0.76		0.52		0.64

In order to test Hypothesis 2, a two-way ANOVA was calculated with typing errors as the dependent variable. Observation condition (proximate observation or alone) was the between-subjects variable and task complexity (simple or complex) was the within-subjects variable. The data for the errors were very positively skewed, because the majority of the people had very few errors. There is no non-parametric equivalent to a two-way ANOVA, and as ANOVA is very robust (Field, 2005) it was deemed to be suitable. The ANOVA results are in Table 24.

Table 24: Analysis of Variance for Errors

Variable Main Effects	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	0.89	.02	.35	.15
Complexity	1	10.53**	.21	<.01	.89
Error	39	(1.33)			
Interaction					
Observation x Complexity	1	0.38	.01	.54	.09

Note. *N* = 41.

Values enclosed in parentheses represent mean square errors.

There was a significant effect of complexity, no significant effect of observation, and no significant interaction. Participants made more errors in the complex task than the simple task, but the complex task was longer and the error scores did not take that

into account. Hypothesis 2, that observation would decrease the errors in the name and increase the errors in the code, was not supported. Due to the data being highly skewed, further investigations with the errors and the personality and individual differences variables were not made.

Hypothesis 3: There will be an interaction between observation condition (proximate observation or alone) and task complexity (simple or complex) in terms of perceived task difficulty. Participants in the proximate observation condition will rate typing their names (the simple task) as easier, and typing the code (the complex task) as more difficult, than participants in the alone condition will rate these same tasks.

To test Hypothesis 3, the data from the same ratings as those used in Manipulation Check 2 were used, but they were analysed by observation condition. The Likert scale was anchored at 1 (*easiest*) and 9 (*most difficult*). Participants who used the shortcut in the code or typed the code incorrectly were excluded. The descriptive statistics are shown in Table 25.

Table 25: Perceived Difficulty of Simple and Complex Tasks by Observation Condition

Task	Observation Condition				
	Alone <i>N</i> = 19		Proximate <i>N</i> = 22		Row Means
	Mean	SD	Mean	SD	
Simple	1.22	0.55	1.15	0.37	1.19
Complex	6.11	1.71	6.08	1.28	6.10
Column Means	3.67		3.62		3.64

In order to test Hypothesis 3, a two-way ANOVA was calculated with perceived difficulty as the dependent variable. Observation condition (proximate observation or alone) was the between-subjects variable and task complexity (simple or complex)

was the within-subjects variable. Although these data originate from a Likert scale, and are therefore not interval data, they can be used for ANOVA (Field, 2005). The ANOVA results are in Table 26.

Table 26: Analysis of Variance for Perceived Difficulty

Variable Main Effects	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	0.04	.00	.84	.06
Complexity	1	382.33**	.91	<.01	1.00
Error	36	(1.27)			
Interaction					
Observation x Complexity	1	.01	.00	.94	.05

Note. *N* = 38.

Values enclosed in parentheses represent mean square errors.

As the outputs in Table 26 suggest, there was a significant effect of complexity: The name was rated as being easier than the code. There was no significant effect of observation, nor any interaction. Hypothesis 3, that observation would decrease the perceived difficulty of the simple task and increase the perceived difficulty of the complex task, was not supported. The data for the perceived difficulty of the name were highly positively skewed. For this reason, further investigations controlling for the personality and individual differences variables were not made.

Post-hoc analysis: Correlation of errors and speed.

There might have been a trade-off between speed and accuracy, such that people either type faster, or more accurately (Meyer, Smith, Kornblum, Abrams, & Wright, 1990; Plamondon & Alimi, 1997). This could compromise any effect of observation on speed. This effect would be seen as a correlation between the remaining errors, or the total errors, and faster speed. Alternatively, it might take participants longer to correct errors. This would be seen as a correlation between the corrected errors, or the total errors, and slower speed. Conversely, correcting errors might make the mean inter-stroke interval faster, especially if participants are pressing delete a

couple of times in quick succession. This would be seen as a correlation between the corrected errors, or the total errors, and faster speed. In order to check for any of these effects, correlations were run between remaining errors, corrected errors, or total errors and speed within task complexity.

All the correlations were extremely small and non-significant. The calculation of speed as the mean inter-stroke interval already predominantly took into account the extra keystrokes involved in correcting errors. The findings for Hypotheses 1 and 3 were not affected by a trade-off between speed and accuracy.

Hypothesis 4: Participants in the proximate observation condition will rate their state anxiety higher than participants in the alone condition.

As participants' state anxiety is likely to be influenced by their neuroticism, the effect of neuroticism was controlled for. Table 27 displays the descriptive statistics for state anxiety levels, corrected for neuroticism.

Table 27: State Anxiety by Observation Condition, corrected for Neuroticism

Task	Observation Condition				Row Mean
	Alone <i>N</i> = 8		Proximate <i>N</i> = 11		
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	
State Anxiety Score	43.01	3.19	40.46	2.65	41.735

In order to test Hypothesis 4, neuroticism was included as a covariate in an ANCOVA with state anxiety as the dependent variable. Observation condition (proximate observation or alone) was the between-subjects variable.

Table 28: ANCOVA of Observation on State Anxiety, corrected for Neuroticism

Variable	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	1	0.29	.02	.60	.08
Neuroticism (covariate)	1	2.30	.13	.15	.30
Error	16	(69.93)			

Note. *N* = 19.

Values enclosed in parentheses represent mean square errors.

The outputs in Table 28 show that there was not a significant main effect of observation condition on state anxiety, when corrected for neuroticism. Neuroticism also did not significantly predict the state anxiety scores. Hypothesis 4 was not supported.

7.9. Discussion

Experiment 4 aimed to obtain a social facilitation effect using improved methodology and apparatus. The improvements and changes aimed to overcome the problems found in Experiment 3, and to replicate a previous experiment that found a social facilitation effect with typing tasks (Schmitt et al., 1986). Experiment 4 aimed to obtain the normal (non-psi-mediated) social facilitation effect, of facilitation of simple task performance and inhibition of complex task performance from proximate observation compared to being alone. We intended to find the normal social facilitation effect to establish the validity of our methods and apparatus. If this methodology obtained the normal social facilitation effect, we could then use it to investigate whether there is a social facilitation effect from remote observation. The participants completed typing tasks that were presented as being the log-in to another experiment, and so should not have elicited evaluation apprehension. Each participant typed both his or her names (simple task) and a code (complex task) once, either under proximate observation or while alone. We measured two indicators of task performance: speed and accuracy, along with two further indicators of the effect

of observation: perceived task difficulty and state anxiety. The social facilitation effect, the interaction between observation and task complexity, was not found using any of these measures. Each of these measures will be considered in turn below.

We hypothesised that proximate observation would increase the speed of typing the name (simple task) and decrease the speed of typing the code (complex task). This effect was not found, even when potentially confounding individual differences were controlled for. There was, however, a trend towards observation increasing the speed of both tasks. A post-hoc test found that the speed of typing the code was significantly increased by proximate observation. This is the opposite of the expected effect, and also the opposite of the effect found by Schmitt et al. (1986) with the same code. One possible reason for facilitation of complex task performance is that, when considering speed, complex task inhibition is an unreliable effect (C. F. Bond & Titus, 1983). Over many studies, 42.6% of investigations into the effect of observation on the speed of complex tasks found a facilitatory effect (C. F. Bond & Titus, 1983). As argued in Chapter 6, this could have been because simple and complex tasks are poorly differentiated, but it could also simply mean that the inhibition of complex task speed from observation is unreliable, and is therefore a difficult effect to find. If that is the case, a facilitatory effect of observation on complex task performance is in accordance with the previous literature. Alternatively, the code might have been facilitated by observation because it acted as if it were a simple task in this experiment.

One possible reason for this difference in the direction of the effect of observation on the code between Experiment 4 and Schmitt et al.'s (1986) experiment is that people's skill level at typing, and familiarity with logging-in, might have increased considerably since 1986. Zajonc's (1965) original distinction between facilitated and inhibited tasks was that the facilitated ones were well-learned. The code might have been a complex task previously, but typing might now be such a well-learned skill that it might be difficult to create a complex typing task. If, as some authors have

argued, tasks that are inhibited by observation should be difficult (e.g., Aiello and Svec, 1993), then the fact that the participants rated the code as only just above the midpoint on the difficulty rating indicates that the code was perhaps not an adequately complex task.

The name, on the other hand, was confirmed as being well-learned and easy by the manipulation check questions. The facilitation of the speed of simple tasks is a reliable effect (C. F. Bond & Titus, 1983). There are three potential explanations of the failure to replicate the facilitation of simple task speed. Firstly: a ceiling effect, as the name might have been so easy that it could not have been done any faster. Secondly, there might not have been enough of a difference between the proximate observation and alone conditions (limitations of the observation condition manipulation will be considered below). Lastly, the names might have been too short, despite participants using their full names, to show the social facilitation effect. Thus, if in Experiment 4 the name and code were both responding as if they were simple tasks, the code might have shown a facilitatory effect better because it was longer and more labile. As the code took a longer time overall, changes in the time taken due to observation were more apparent. Future investigations into social facilitation with typing tasks might benefit from using longer tasks.

The second hypothesis asked whether proximate observation would have a social facilitation effect on the accuracy of the typing tasks. Specifically, it investigated whether observation would increase the accuracy of the name (simple task) and decrease the accuracy of the code (complex task). This was assessed by looking at the total number of errors made by the participants, both those corrected, and those that remained. There was no significant social facilitation effect found on task accuracy. When considering how task accuracy responds to observation, C.F. Bond and Titus (1983) found that simple task facilitation had low reliability. Therefore the failure of Experiment 4 to find an increase in the accuracy of typing the name might not contradict the previous literature. The inhibition of complex task accuracy,

however, was the most reliable effect found, more reliable than either the facilitation of the speed of simple tasks or the inhibition of the speed of complex tasks (C. F. Bond & Titus, 1983). Proximate observation has previously been found to decrease complex task accuracy in anagram tasks (Aiello & Svec, 1993), and in learning word pairs (Berkey & Hoppe, 1972). Decreased complex task accuracy from observation was also found in a task that did not manifest a decrease in complex task speed (Stanton & Sarkar-Barney, 2003). It is possible that observation did not decrease the accuracy of typing the code in Experiment 4 because, as argued above, the code was inadequately complex.

The third hypothesis asked whether observation would affect participants' perception of how difficult the tasks were. Specifically, it asked whether observation would decrease the perceived difficulty of the name, and increase the perceived difficulty of the code. This hypothesis was not supported: There was no significant difference in perceived task difficulty between observation conditions. Perceived difficulty of a complex maths task was increased by observation in a previous experiment (Bradner & Mark, 2001). However, Bradner and Mark's study also found a social facilitation effect: Observation impaired complex task performance. Thus Bradner and Mark's findings were in accordance with each other. When observed, participants both experienced the complex tasks to be more difficult, and performed them as if they were more difficult. Experiment 4 failed to replicate the finding that observation alters the perceived difficulty of the tasks, but also failed to replicate the social facilitation effect. The findings from Experiment 4 are therefore also in accordance with each other; observation did not affect either task performance or the perception of task difficulty. The question of whether perceived difficulty routinely responds to observation should be investigated in further social facilitation studies, preferably ones that find a social facilitation effect.

The last hypothesis asked whether observation would affect participants' state anxiety. Specifically, it asked whether participants who were observed had higher

state anxiety during the experiment than participants who were alone. There was no effect of proximate observation on state anxiety, with individual differences in neuroticism controlled for; this hypothesis was not supported. State anxiety in Experiment 4 was used as a self-report measure of participants' arousal levels. There is little precedent for this in social facilitation research, but *stress* is a similar concept to state anxiety and was found to be affected by observation (Aiello & Kolb, 1995). The direction of the effect (whether stress was increased or decreased by observation) was mediated by participants' locus of control (internal or external), and so the effect of observation on state anxiety might also be mediated by individual differences that were not considered in Experiment 4. The null effect of observation on state anxiety is in accordance with the null effect of observation on accuracy and perceived difficulty found so far in this experiment, all of which point to a lack of an effect from the observation manipulation. The discussion will now turn to possible reasons that observation did not appear to exert much influence in Experiment 4.

To remind the reader, Experiment 4 aimed to replicate the proximate observation and alone influences on name and code task performance that showed a social facilitation effect in a previous study (Schmitt et al., 1986). The changes made after Experiment 3, using full names in the typing tasks and a between-subjects design, meant that Experiment 4 was a close replication of Schmitt et al.'s study, albeit without a mere presence condition. Schmitt et al. found a significant main effect of complexity, and a significant interaction between task complexity and observation condition. Experiment 4 found significant main effects of complexity for task speed, accuracy, and perceived difficulty, but no interactions with observation. One possible reason for this is that observation did not exert enough influence, although there was a trend for observation to increase the speed of both tasks. One reason the observation condition manipulation in Experiment 4 might have been weak is the possible contamination of the alone condition. The experimenter, who was also the observer, was in the room with the alone participants until just before they typed their name and code. If there is a lingering effect of presence (see, e.g., Dumas, Huguet, &

Ayme, 2005), this may have been an insufficiently alone condition. Another possible reason is that participants realised the typing tasks were being monitored. In Experiment 4, 60% of the participants suspected the typing tasks of being part of the experiment, and six of them guessed that they were timed. Some (seven) of the participants said that their attention was drawn to the code because it was “weird”. Therefore, the performance measurement was not covert. Apart from the increased length, the difference between the codes in Experiments 4 and 3, in which participants did not suspect this was a task, was that they had to work the code out mentally, not copy it. The most likely reason that more participants suspected the code in Experiment 4 was because they had to work it out for themselves. In contrast, no participants even suspected the typing tasks in the original experiment with these tasks (Schmitt et al., 1986). It is important in creating a true alone condition that participants do not feel as though they are being monitored, evaluated or watched when alone (Markus, 1978). In Experiment 4 this criterion of a true alone condition was not fulfilled. Thus, the alone condition may have been compromised by evaluation apprehension. Evaluation apprehension can lead to the social facilitation effect (Henchy & Glass, 1968), meaning that the alone condition in Experiment 4 was not the influence-free baseline it aimed to be. This may have lessened the difference between the observation conditions, and reduced the relative impact of proximate observation.

Another possible reason that observation might not have exerted enough influence in Experiment 4 is the short duration of the observation period. Participants were only observed for around one minute. Although it is not known how long it takes for the effect of observation to start, nor how long the effect of observation lasts (Guerin, 1993), it might be the case that such a short duration is not sufficient. In comparison, assuming that the participants in Schmitt et al.’s (1986) experiment were observed for the duration of the tasks, they were observed for between seven seconds and just over one minute. These are similar to, if not shorter than, the observation durations in Experiment 4, but then again, it is not specified whether the participants were

observed before and after the typing tasks in Schmitt et al.'s experiment. If participants were observed before and after they would have been observed for longer. Likewise, the tasks in Markus's (1978) experiment, which revealed a social facilitation effect, only took a minute or two, but the participant was left in their observation condition for 10 minutes (most of which was after completing the tasks). Longer durations have also been used: Aiello and Svec's (1993) anagram tasks lasted 10 minutes, and participants were in their observation conditions for this whole time. A social facilitation effect was also found on simple data entry tasks that took 15 minutes (Aiello & Kolb, 1995). Stanton and Sarkar-Barney (2003) found a social facilitation effect in a complex database checking exercise that lasted 35 minutes, and the participants were in their observation conditions this whole time. Proximate observation also facilitated simple data entry in two sessions of 45 minutes each (Griffith, 1993). So a wide range of observation durations, most, but not all of which, were longer than in Experiment 4, have elicited a social facilitation effect. Future research could investigate how long a duration of observation is optimal, as too long an observation duration could possibly lead to extinction of the arousal-increasing response when the participants become accustomed to the observer (Wapner & Alper, 1952). Attempts to improve upon the design of this experiment should include a longer observation duration.

In summary, Experiment 4 failed to find a social facilitation effect. The speed of typing the code was, unexpectedly, increased by proximate observation, indicating that it behaved as if it were a simple task, not a complex task as intended. We tentatively suggested that the code demonstrated a facilitatory effect, while the name did not, due to the longer length of the code task. The code, however, also compromised the covert nature of both typing tasks, and the "aloneness" of the alone condition, by attracting participants' attention and leading them to suspect that these tasks were being timed. Therefore, this code is not a suitable complex task for future studies involving a remote observation condition. The failure to replicate the social facilitation effect found by Schmitt et al. (1986) might be because Schmitt et al.'s

finding was itself spurious or artefactual. Experiments 3 and 4 were the first replication attempts, and both found null results. The methodological problems in Experiment 3 should have been overcome by the changes in Experiment 4. In Experiment 4 the null findings might have been because the alone condition was compromised, and the duration of observation may have been too short. One final possible reason for the failure to replicate the social facilitation effect was the low power. Many participants' data were excluded for typing the code differently, or to normalise the data. The between-subjects design also reduced the amount of data collected from each participant. Therefore, larger sample sizes might be needed.

7.10. Conclusions

The social facilitation effect of simple task facilitation and complex task inhibition on typing speed, typing accuracy, and perceived task difficulty was not found. There was also no effect of observation on state anxiety. There were trends towards observation facilitating the speed of both the simple and the complex tasks, but overall the effect of observation seemed weak, and increased observation durations might be required to elicit social facilitation effects. In future experimentation the code should be omitted, as it seems to behave as a simple task is expected to, and it compromised the alone condition. However, as the code showed some facilitatory effects, which might have been due to its longer length, longer typing tasks might better capture a social facilitation effect. However, as the typing tasks might be less effective than previous research indicated, further research might benefit from using different tasks.

8. Experiment 5: Psi-Mediated Social Facilitation from Remote Observation in Simple Task Performance

8.1. Overview

This chapter presents an empirical study, Experiment 5. This study aimed to further investigate whether there is a social facilitation effect from remote observation (observation that participants can only be aware of through psi). As in Experiment 3, we used three observation conditions: alone (physically alone and not observed, even covertly); remote observation; and proximate observation (observation from a physically present observer). We investigated the effect of remote observation and proximate observation with a new set of tasks. In Experiments 3 and 4, typing the code (intended to be a complex task) was actually marginally facilitated by proximate observation, as if it were a simple task, but typing the name (a simple task) was not. As previously argued, the code might have shown an effect from observation, while the name did not, because it was a longer task. Consequently, in Experiment 5, the typing task was modified to be an even longer simple task while still being covert. As the only effect from observation in Experiments 3 and 4 was facilitatory, the following study only used simple tasks, aiming to find a facilitatory effect from remote and proximate observation. The new tasks were a simple maths task, and a Stroop task, neither of which were covert. In prior literature, maths and Stroop tasks have shown social facilitation effects, and we review this below. In Experiment 5, we asked whether performance on these tasks was facilitated by remote and proximate observation compared to performance when participants were alone. The moderating effects of participants' extraversion, neuroticism, state anxiety, belief in psi, gender, and age on the social facilitation effect were controlled for.

8.2. Introduction

According to the PMIR model, people can respond to circumstances beyond the reach of their senses, similarly to the way they would respond if they had sensory knowledge of them (Stanford, 1990). In other words, through psi, people can respond to sensorially remote stimuli as if they had sensory contact with them. As reviewed in Chapter 6, when people are in the physical presence of an observer (proximate observation) they usually perform simple tasks better and more accurately compared to their performance when alone. Experiment 5 explored whether a similar simple task performance facilitation occurs when people are observed in such a way that they cannot have sensory knowledge that they are being observed (remote observation), and could only be affected by the observation via psi.

To recapitulate briefly, in Experiments 3 and 4, name and code typing tasks were presented as if they were an incidental part of the experiment set-up. In other words, they were intended to be covert tasks. This manipulation was important in limiting participants' evaluation apprehension. As noted in Chapter 6, evaluation apprehension is an influence with an effect similar to observation, which results from participants' awareness or concern that their performance is being assessed (Henchy & Glass, 1968). Covert tasks are important in creating a true alone condition, in which participants are not only physically alone and unobserved, but do not feel monitored, observed, or evaluated (Markus, 1978). The typing tasks, especially in Experiment 4, were not successfully presented as covert tasks, predominantly because of the code task. It is difficult to create a covert task on which performance can be assessed. The typing tasks aimed to replicate the only successful covert task in the previous social facilitation literature (Schmitt et al., 1986), but failed to do so. Therefore, we decided to introduce other tasks in Experiment 5 that have shown social facilitation effects in previous research, in addition to a modified typing task.

In Experiment 5, we measured participants' speed and accuracy in three different tasks. The first of these was a typing task. As observation had increased the speed at which participants in Experiments 3 and 4 typed the code, the longer of the two typing tasks in those studies, we used an even longer typing task in Experiment 5 to increase the chances of finding the social facilitation effect. Participants each typed their full name and email address, misinformed that this was an incidental part of the experiment set-up. This was the only task that aimed to be covert in Experiment 5. As it was a simple typing task, being both easy and well-practised, we predicted that observation would facilitate performance.

The first new task that was introduced in Experiment 5 was a colour Stroop. In this section, we describe social facilitation effects previously found with the Stroop task, and, therefore, the effects remote observation might lead to. In a Stroop task participants name, or respond to, the ink colour that words or control stimuli are presented in. People are faster at naming, for example, the green colour of the ink for either control strings (e.g., +++) or the word GREEN written in green ink, than for the word RED written in green ink (Stroop, 1935). This is a robust effect called *Stroop interference* (Stroop, 1935). Interference is measured as the difference between the time taken to name the colour of the *incongruent colour* words (i.e., words written in a different colour ink to the colour word they spell) and the time taken to name the ink colour of the control items. Stroop interference occurs because the meaning of the word interferes with the naming of the ink colour, and this slows people's responses to words written in incongruent colours (MacLeod, 1991). In the earliest research, the Stroop task was presented in a block design, with one card containing only incongruent colour words and another containing only control stimuli (Stroop, 1935). This design risks the potential disadvantage of anticipatory effects, as participants would expect the next item to be an incongruent colour word, if the last one was. With electronic presentation it is possible to intersperse control and incongruent colour items (e.g., +++ vs. RED respectively, in green ink), and this

eliminates the possibility of anticipation effects. Modern research more often uses electronic presentation of stimuli.

Stroop interference has responded to social influences in previous experiments. For instance, competition has been found to reduce Stroop interference (Dumas et al., 2005; Huguet, Dumas, & Monteil, 2004; MacKinnon, Geiselman, & Woodward, 1985). In MacKinnon et al.'s (1985) experiment, participants performed a Stroop task under one of two conditions; they were randomly assigned either to compete against each other in pairs for a prize, or to perform without competition. Competition reduced Stroop interference. In another study, Huguet et al. (2004) also compared participants' performance alone, or in the presence of a competing confederate. In a third study, Dumas et al. (2005) measured participants' performance alone, but directly following being in the presence of a competing confederate. The presence, or previous presence, of a competitor significantly reduced Stroop interference (Dumas et al., 2005; Huguet et al., 2004). In all of the above experiments, Stroop interference was reduced and participants' reaction times to both the control and incongruent colour items were faster. In sum, these studies revealed a reduction in Stroop interference from competition, a social influence that has a similar influence to observation (Triplett, 1898) and, therefore, observation might similarly reduce Stroop interference.

Huguet et al. (2004) and Dumas et al. (2005) attributed the reduced Stroop interference to changes in arousal. There is support for the idea that increased arousal reduces Stroop interference. For instance, Stroop interference was reduced by increased arousal from loud white noise (Booth & Sharma, 2009; Chajut & Algom, 2003; O'Mally & Gallas, 1977; O'Mally & Poplawsky, 1971). Arousal induced by administering electric shocks, and threatening participants with higher voltage shocks, also decreased reaction times to control items and incongruent colour words (Agnew & Agnew, 1963).

A similar effect of reduced Stroop interference and decreased reaction times to both control and incongruent colour items was found in response to observation (Huguet et al., 1999; Klauer et al., 2008). Participants in both these experiments were first trained in responding to colour stimuli (blue, green, red, or yellow) by pressing buttons on a standard computer keyboard. Once trained, participants were timed responding to the colour of control items (strings of 3 or 4 '+'s or colour-neutral words, e.g., chair) and incongruent colour words. Participants in Klauer et al.'s (2008) experiment completed electronically presented Stroop tasks whilst either alone or proximately observed, within-subjects. Observation reduced the Stroop interference effect, especially when the control items were strings rather than neutral words (Klauer et al., 2008). Participants' reaction times to all items were faster under proximate observation. Additionally, Huguet et al. (1999) compared two proximate observation conditions: an observer standing directly behind the participant, and an observer watching attentively from in front of the participant. Both proximate observation conditions were compared to a mere presence condition (the observer was reading) and to an alone condition. Both the proximate observation conditions reduced Stroop interference compared to the mere presence and alone conditions. Again, participants' reaction times to all items were decreased by proximate observation. In sum, Stroop interference and reaction times have been reduced by competition, arousal, and proximate observation. Therefore, in Experiment 5, proximate observation is predicted to reduce Stroop interference while decreasing reaction times to both control and incongruent colour items. If remote observation has a similar effect to proximate observation, then it too would reduce Stroop interference.

For the participants, the Stroop was overtly an experimental task. As it was completed on a computer, participants were likely to be aware that their responses were recorded. It was also presented as being the main task in Experiment 5; it was the task participants were ostensibly recruited for. It was therefore likely to elicit

evaluation apprehension. This might, on the other hand, have conveyed the advantage of distracting suspicion away from the typing task.

The other new task in Experiment 5 was multiplication. Maths tasks have manifested social facilitation effects in previous research. For instance, Grant and Dajee (2003) compared participants' performance on a simple multiplication task when participants were either alone or proximately observed, between-subjects. Participants completed 45 multiplication questions, which were made up of combinations of the digits 0 - 10 (e.g., 2×10) on paper while timing themselves with a stopwatch. The alone participants were physically alone in the testing room. The observed participants were watched quietly by another participant who was also in the testing room. The observed participants completed the maths task significantly faster than the alone participants. Accuracy was not investigated. In another previous experiment complex maths task inhibition was found when comparing performance under proximate observation to performance alone (Zanbaka et al., 2007). Participants decided whether simple (e.g., $5+2=7$) and complex (e.g., $(6/3)+8-(4 \times 2)=3$) equations were true or false, while alone or with proximate observation, within-subjects. Participants made significantly more errors and were significantly slower on the complex task under proximate observation compared to their performance alone. Zanbaka et al. noted that there was slight facilitation of the simple maths items, both in terms of speed and accuracy, and suggested this was non-significant due to ceiling effects, as the simple task was too easy. Considering these two experiments together, proximate observation has led to facilitation of simple maths task performance (Grant & Dajee, 2003) and inhibition of complex maths task performance (Zanbaka et al., 2007), demonstrating both aspects of the social facilitation effect.

Further examples of social facilitation effects in maths tasks involved co-action influences (Dashiell, 1930; Fletcher, 1985). Children produced more accurate decisions on a maths-based computer game when working in co-acting groups

compared to their performance when alone (Fletcher, 1985). Dashiell (1930) also found that co-acting groups, even when working in individual rooms, were faster at simple multiplication tasks than participants who were alone. Thus, maths tasks appear to be influenced by observation and other social influences and can manifest the social facilitation effect.

Experiment 5 used a simple maths task closely based on that used by Grant and Dajee (2003), although digits up to 12 were included in the multiplication problems to ensure that the task was not too easy and prone to ceiling effects. Observation was predicted to facilitate performance. Like the Stroop, the maths task was obviously an experimental task, and might therefore have provoked evaluation apprehension. To minimise this, we used a deception similar to that used by Huguet et al. (1999). Huguet et al. aimed to reduce evaluation apprehension in participants performing a Stroop, by telling them that due to technical problems they would not do the real task, but a different one (the Stroop), on a computer that was not yet programmed to record their responses. They did not report the success of this manipulation. In Experiment 5, we misinformed participants that the maths task on the computer had failed, and that they would do the task on paper, self-timed with a stopwatch, just so that they did a comparable procedure to the other participants. We hoped this would reduce evaluation apprehension.

In addition to speed and accuracy of task performance, we measured participants' perceived difficulty of the tasks. As reviewed in Chapter 7, it is possible that observation might make tasks seem easier or more difficult. This effect has been shown in complex tasks, which participants perceived to be more difficult when they were observed (Bradner & Mark, 2001). In Experiment 5, participants rated the perceived difficulty of the tasks on Likert scales. We predicted that the tasks would be rated significantly differently between the observation conditions.

As reviewed in Chapter 6, the social facilitation effect is thought to be mediated by observation leading to an increase in the participants' arousal (Moore & Baron, 1983). State anxiety can be used as a self-report measure for arousal level (Mullen et al., 1997). In Experiment 5, participants rated their current state anxiety during the experiment. We predicted that observation would increase state anxiety.

In Experiment 5, we measured personality characteristics and individual differences that can affect both task performance and how observation affects people's task performance. These were: extraversion, neuroticism, belief in psi, gender, and age. As a conservative measure, the potential effects of these individual differences were controlled for when assessing whether a social facilitation effect was found, although no directional hypotheses were made.

In summary, in Experiment 5 (below) participants typed their full names and email addresses once, misinformed that this was an incidental part of the experiment set-up. They were not told that their typing was timed. Participants then completed a Stroop task on a computer, a task that presumably provoked evaluation apprehension. Participants then timed themselves completing a maths task on paper, with an evaluation apprehension reducing manipulation (participants were misinformed that this was not the real maths task). Throughout the whole experiment, participants were either alone, remotely observed, or proximately observed. Firstly, we asked whether the normal social facilitation effect was found, by investigating whether all the tasks were performed faster and more accurately by participants who were proximately observed, compared to participants who were alone. Secondly, we asked whether there had been a psi-mediated social facilitation effect by investigating whether all the tasks were performed faster and more accurately by participants who were remotely observed, compared to participants who were alone. The direction and magnitude of the remote observation effect were also compared to the effect of proximate observation. As a conservative measure we controlled for possible confounding personality and individual difference variables. Thirdly, we investigated

whether there was an effect of observation on state anxiety. Lastly, we investigated whether there was an effect of observation on perceived task difficulty.

8.2.1. Hypotheses.

1 Participants who are observed, either proximately or remotely, will type their name and email address faster than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

2 Participants who are observed, either proximately or remotely, will type their name and email address more accurately (i.e., make fewer errors) than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

3 Participants who are observed, either proximately or remotely, will demonstrate reduced Stroop interference compared to participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

4 Participants who are observed, either proximately or remotely, will make fewer errors on the Stroop task than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

5 Participants who are observed, either proximately or remotely, will complete the maths task faster than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

6 Participants who are observed, either proximately or remotely, will make fewer errors on the maths task than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation. In other words, proximate observation and remote observation will both elicit the social facilitation effect, but we expect a weaker effect from remote observation.

7 Participants who are observed, either proximately or remotely, will have higher state anxiety ratings than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

8 Participants who are observed, either proximately or remotely, will rate the difficulty of the tasks (typing, Stroop, and maths) differently to the participants who are alone. We tentatively predict that proximate observation will have a greater effect in this than remote observation.

8.3. Method

8.3.1. Design.

This experiment used a between-subjects design. The independent variable was observation condition (alone, remote, or proximate observation).

8.3.2. Participants.

One-hundred participants volunteered for a “Stroop Gobbledegook” experiment. The participants had a mean age of 22.87 years; 38 were male and 62 female; all were

naïve to the purpose of the experiment. To maximise the chances of performance effects from observation, we recruited participants who were strangers to the observer and would perceive her to be an expert. This decision was based on previous research findings: Observers who are strangers or experts elicit a greater social facilitation effect (C. F. Bond & Titus, 1983; Guerin, 1993; F. G. Miller et al., 1979). The observer should have appeared to be an expert by virtue of her position as experimenter. Additionally, she was older and further up the educational hierarchy than most of the participants, who were predominantly undergraduates, increasing her expert status.

There is no previous research relating to the effect of social contact between the observer and participant prior to an experiment, nor whether affiliation between observer and participant would affect the social facilitation effect. However, to maintain the participants as strangers, we minimised contact between the experimenter and participants before the experiment. Sixty-five participants were recruited from the first year psychology student pool at the University of Edinburgh; they received course credit for participating. The students signed up online for experiments that were posted to the website, and most of them did not meet, or have any contact with, the experimenter prior to their arrival at the experimental room. There were few exceptions to this: Late students received a reminder by text, if they did not reply they also got a phone call, which very few of them answered. A few other participants e-mailed or texted in advance to ask for directions or to apologise for being late, and they all received a reply. Fourteen participants were recruited via other emailing lists, but had not met the experimenter prior to doing the experiment. A further 17 were recruited from one face-to-face contact before the experiment, but had no other previous contact with the experimenter. The remaining four participants were known to the experimenter, but only by sight.

None of these participants could have known from their personal knowledge of her that the experimenter was researching parapsychology. To minimise the risk of

participants finding this out in other ways, the experimenter deliberately had a misleading entry on the Psychology Department's website, which did not specify her true research interests. The experimenter did not discuss the purpose of the experiment with anyone other than her supervisors.

The sample size was calculated in G Power (Buchner, Erdfelder, & Faul, 1997; Faul, Erdfelder, Lang, & Buchner, 2007). With a one-way ANOVA the recommended sample size for power of .8 was around 100 participants. The effect sizes from Experiments 3 and 4 were used to calculate this.

Experimenter and Observer.

The author took the role of experimenter and observer.

8.3.3. Ethical Considerations.

We identified two potential ethical issues in this experimental design. There was deliberate deception of the participants regarding the overall aim of the experiment and the exact function of the experimental tasks, and covert observation via webcam images relayed to a computer terminal. As discussed in Chapter 7, we decided that giving participants the right to terminate the experiment at any time and a full debrief following the experiment would resolve these issues. The Psychology Research Ethics Committee at the University of Edinburgh granted ethical approval for this experiment to proceed.

8.3.4. Materials and Apparatus.

Typing task.

The program that timed the typing of each participant's name and email was an adaptation of the name and code timing program written in Microsoft Excel for Experiment 4 by the author in conjunction with a computer programmer at the

University of Edinburgh. It logged each keystroke with *key down* and *key up* times, accurate to the millisecond. Participants saw an input screen with two text boxes, one for their full name and one for their email address. There was no indication that typing speed was recorded. The program also made and presented a code on the screen which was made up of the first and third letters of the participant's name, and a random four digit number (e.g., Sam's code might be sm1576). This code was the participant's identification for the other experimental tasks.

Personality Questionnaires.

Extraversion and neuroticism were measured on scales from the International Personality Item Pool (IPIP) (Goldberg, 1999). Each scale has 10 items, such as "I feel comfortable around people" for extraversion. Some of the items are negatively phrased, such as "I am not easily bothered by things" for neuroticism. Participants respond on a five-point Likert scale anchored at 1 (*strongly disagree*) and 5 (*strongly agree*). Each personality scale is scored from a minimum of 10 to a maximum of 50 points. High scores indicate, for example, high extraversion. The IPIP measures the same five-factor personality constructs as the NEO PI-R (Goldberg, 1999), but is in the public domain, and is therefore free and editable. A bespoke version was constructed, omitting the scales for agreeableness and conscientiousness, saving experiment time. The full list of IPIP items are in Appendix 3.

State anxiety was measured on a six-item state anxiety questionnaire (Marteau & Bekker, 1992). Participants rate items, for example "I am tense" on a four-point Likert scale anchored at 1 (*not at all*) and 4 (*very much*). State anxiety is scored from a minimum of six to a maximum of 24 points; high scores indicate high state anxiety. The full list of STAI questions are in Appendix 3.

The final questionnaire was the Koestler Parapsychology Unit belief in psi 12-item questionnaire (Appendix 4). Participants respond on seven-point Likert scales

anchored at 1 (*no*) and 7 (*yes*). Belief in psi is scored from a minimum of 12 to a maximum of 84; high scores indicate a high belief in psi.

Stroop task.

Five colours were selected for the Stroop: red, green, blue, yellow, and white. Red, green, blue, and yellow have been used in previous published Stroop research (Dumas et al., 2005; Huguet et al., 2004; Huguet et al., 1999; Klauer et al., 2008). White was included because it had been used in several recent student projects that manifested a Stroop interference effect (Vigentini, 2007). Three formats of Stroop stimuli were used in Experiment 5: example sheets, practice stimuli, and experimental stimuli. Example sheet 1 had five congruent colour items (e.g., RED written in red) and 3 strings (e.g., ++++). Example sheet 2 had seven incongruent colour words (e.g., RED written in yellow). Both sheets were A4 with the words in capitals in Arial size 72 font on a black background (Appendix 5).

The practice stimuli consisted of JPEG images of coloured rectangles in the centre of a black screen. The coloured rectangles were the same height as capital letters in font size 72, and the same width as three-letter words. There were three presentation of each colour, making a total of 15 practice trials.

The experimental stimuli comprised a total of 105 items: 60 incongruent colour words, 15 congruent colour words, and 30 control strings. The 60 incongruent colour items consisted of each colour word presented in all the other colours three times each (e.g., 3 x RED written in green, 3 x RED written in yellow, and so on). Congruent colour items were included because, in previous research, a higher proportion of congruent colour items increased Stroop interference (Kane & Engle, 2003; Lindsay & Jacoby, 1994). Pilot testing for Experiment 5 likewise revealed that if only incongruent colour words are used, people anticipate that a colour word will require a response other than the colour that the word spells; including congruent colour words prevents this anticipation effect. Although we did not want the

incongruent colour items to become too difficult for the participants, as we expected observation to facilitate this task, a low proportion (20%) of congruent colour items were included to ensure that a Stroop interference effect was found. The 15 congruent colour items consisted of each colour word presented in the colour that the word spelled three times each. The congruent colour items were not used in the analysis of the Stroop interference. Strings of '+' s were used as controls, because, in previous research, a greater interference effect was found between '++++' and incongruent colour words than with colour-neutral words as controls (Klauer et al., 2008). In previous research, strings of three to six '+' s have been used (Booth & Sharma, 2009; Dumas et al., 2005; Huguet et al., 2004; Huguet et al., 1999; Klauer et al., 2008). The strings should be around the same length as the colour words, and in Experiment 5 we chose to use strings of four and six '+'s. The 30 control strings consisted of each string length presented in all the colours three times each. The stimuli were presented as JPEG images in Arial font size 72 on a black background.

The order of presentation of the targets was randomised using a random sequence generated in STATA (StataCorp, 2003, Stata Statistical Software: Release 8). Adjacent exact colour and word duplicates were removed. These targets were inputted into a Stroop presentation and timing program (vCeleris) written by a computer programmer at the University of Edinburgh. One limitation of this program was that the order of stimulus presentation could not be individually randomised, and all participants saw the same stimuli in the same order.

The answer keys were the buttons along the right side of a mini keyboard, (right arrow, End, PgDn, PgUp, Home). These were chosen because they were the most accessible keys, equally easy for left or right handed participants, because the orientation of a mini keyboard can be changed easily. The mini keyboard was connected by a USB cable to the computer, and so there was no delay in the registering of the timing of keystrokes, which is a risk with infra-red light remote keyboards. The keys were labelled with their colour word written in black ink on

white paper fixed to the keyboard. It is important not to label the answer buttons in colour, because this has been found to reverse the Stroop effect by participants bypassing verbal encoding and just looking at the colour of the word and the answer key (Durgin, 2000). The program recorded the time from the presentation of the target on the screen to the pressing of the answer button down with millisecond accuracy, and recorded which button was pressed.

Maths task.

The maths task was presented on paper. It was compiled by generating random integers from zero to twelve using STATA. These were then put together as 50 simple multiplication questions (e.g. $3 \times 5 = _$) (Appendix 2). Adjacent duplicate questions were removed. An ATECH SW210 stopwatch, accurate to centiseconds, was provided along with a clipboard and pen.

8.3.5. Procedure.

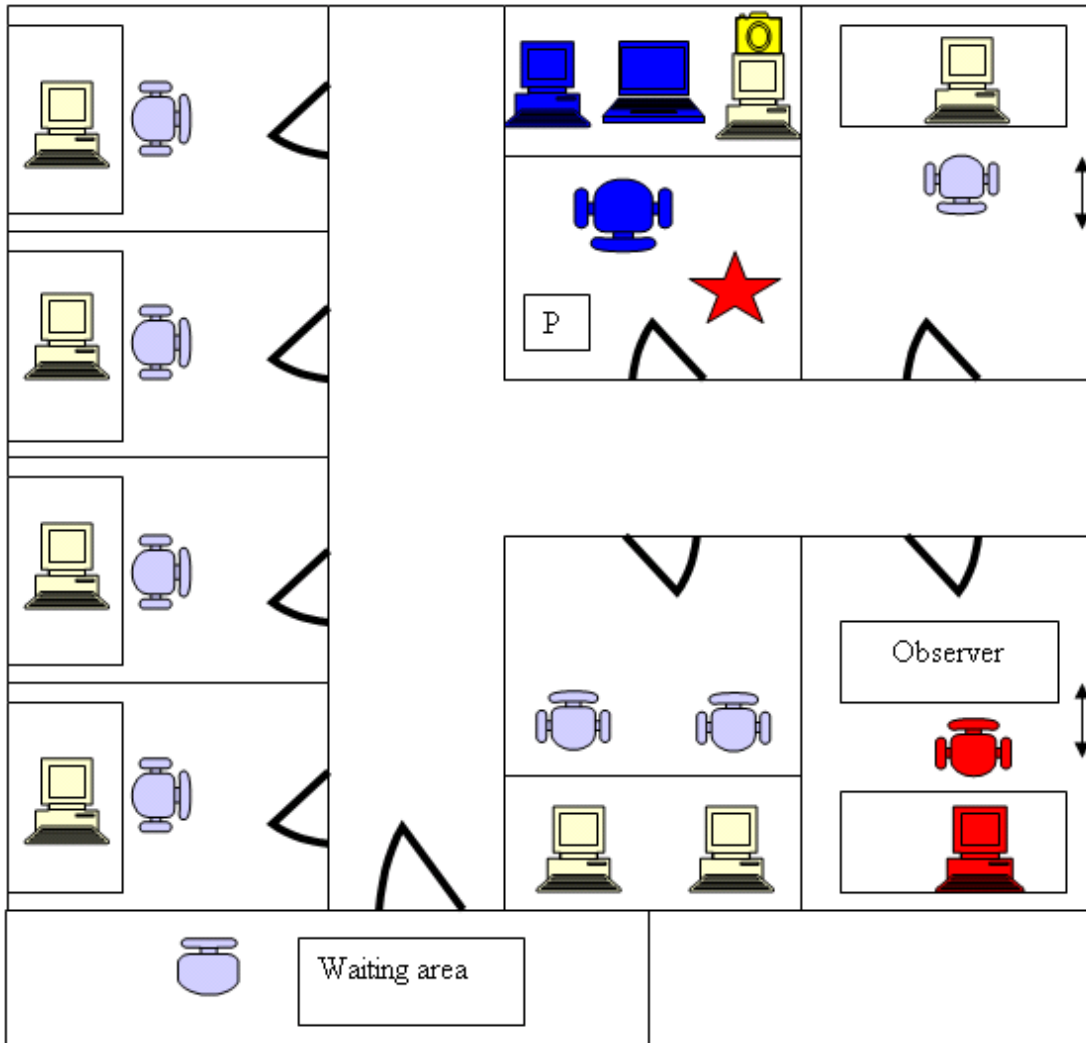
Randomisation of observation conditions.

The experimenter prepared a randomised, counterbalanced schedule of observation conditions prior to experimentation commencing. The order of the observation conditions was generated by a random function in STATA, manipulated so that over 102 trials each observation condition (alone, remote observation, or proximate observation) would be used 34 times. Two lists of observation conditions were created by the STATA program. *Sheet 1* was a list of trial numbers (1 – 102) and the words IN or OUT (meaning inside or outside the experimental room) for each trial. If the observation condition was proximate observation *Sheet 1* said “IN”, and if it was remote observation or alone it said “OUT”. *Sheet 2* was a list of the trial number and the observation conditions (alone, remote observation, or proximate observation) for each trial. Once this sheet was created, the experimenter instantly put it inside an opaque envelope without looking at it. The experimenter consulted *Sheet 2* only after

leaving participants who would be remotely observed or alone in the experimental room. When she checked the observation condition for each trial on Sheet 2, the experimenter used a method of occluding all subsequent trials. The experimenter was thus blind to the future observation conditions on Sheet 2. This blind was set up to prevent any inadvertent differences in the way that the experimenter treated participants between the alone and remote observation conditions.

Session procedure.

The experimental and observation rooms were two cubicles within the experimental laboratory in the basement of the Department of Psychology at the University of Edinburgh. The observation and experimental rooms were separated by a corridor and two doors (see Figure 8) reducing the risk of accidental noise cueing, but not eliminating it entirely, as the rooms were not soundproofed. The experimental room had no windows, and so the layout of the room should not have made participants feel as though they were being watched.



The position of the observer in proximate observation is shown by the star.
 The position of the observer in remote observation is denoted by 'Observer'.
 The participants' room is denoted by 'P'.
 The arrows represent windows.

Figure 8: Map of the Experimental Suite for Experiment 5 (not to scale)

Participants were tested individually. Prior to the participants' arrival, the experimenter checked Sheet 1. Participants were assigned to the next line in the randomised observation condition sequence. The experimenter prepared the

experimental room, ensuring that the participant's instruction sheets matched the observation condition. The instructions for participants who would be physically alone stipulated "When you have finished go and get the experimenter" at the end. This was the only difference in the instructions.

There were two PCs (Vision Master Pro 413) on either side of the testing desk, the Sony VIAO Laptop computer was placed between them. This laptop had a screen size of 23cm high by 30.5cm wide, the screen resolution was set to 1024 x 768 pixels. The Logitech Quickcam Messenger Webcam was hidden inside a lever arch folder, positioned so that the lens looked out through the small hole in the folder's spine. The lens aperture and the hole were the same size (2.5cm), so the view from the camera was not occluded by the folder. Only one participant saw the camera, and she was not allowed to take part in the experiment. The folder was placed on top of the PC on the right side of the testing desk. The monitor attached to this PC was switched off throughout the experiment, and it was not used for any other purpose. Remote observation was enabled through the *video call* function of Windows Live Messenger (Microsoft Corporation, 2009). To activate the video link, a video call was made from the computer in the experimental room. The image of the participant could only be seen after the call was answered from the observation computer in the observation room. The experimenter made the video call before the participant arrived, irrespective of the observation condition.

The experimenter greeted the participants in the waiting area, or, occasionally (if testing after hours when the door to the building was closed) by the door to the psychology building. Each participant was shown into the experimental room in the laboratory, and sat in front of the Laptop, about 60cm from the screen. The experimenter explained the procedure for all the tasks verbally, and gave the participant a printed list of the tasks. As a Stroop practice, the participant read out loud the colour of six congruent colour words, seven incongruent colour words and three strings from Example sheets 1 and 2. This identified problematic colour

blindness (one participant was eliminated from the Stroop task), and ensured that all of the participants understood the task. The experimenter apologised that the software for the maths task was not working, and asked the participants if they did not mind completing it on paper and timing it themselves, just so that they completed the same tasks as the others (the implication and deception being that the others had completed it on the computer, and that the paper task was not assessed). Participants practiced using the stopwatch. After an opportunity to ask questions, participants signed a standard consent form. This explained that they could terminate the experiment at any time (no participants chose to).

Once the participant was ready to start, the experimenter either left the room or remained to observe according to Sheet 1. If the experimenter left the experimental room, she then checked Sheet 2 for the current trial only. In the proximate observation condition, the experimenter remained in the experimental room throughout the entire experiment, without leaving, clearly watching the participant. She stood diagonally behind the participant to the right, with a clear view of the participant, and the task he or she was doing. In the remote observation condition, the experimenter accepted the video call from the observation room. She could see an image of the participant from the right, from a similar distance and angle to where she stood in the proximate observation condition. She watched the image of the participant with her full attention, until just before the participant came to get her, when she minimised the image window so that the participants would not see that they had been observed. In both observation conditions (proximate and remote) she mentally rehearsed “I am watching you. You are being watched.” to block out other thoughts. In the alone condition the experimenter declined the video call, occupied her attention with matters unrelated to the experiment, and remained in the observation room. No other people entered the experimental room.

Participants did the typing task first. They were prompted by instructions on the laptop which asked them to enter their full name and email address and to make a

note of the code that would be displayed onscreen. The rationale for the code was to protect the participant's identity when doing the online personality questionnaires. The rationale for typing their name and email was so that the experimenter knew who was identified by each code. (Both these reasons were true.)

Participants next completed the personality questionnaires and the STAI on the experimental PC (the PC on the left on the testing desk) in Lime Survey, an online survey host (Lime Survey, 2005). They used their experimental code to identify themselves. They then completed the Stroop task on the Laptop using the mini keyboard to enter their responses. Participants first entered their experimental code and read an onscreen reminder of the Stroop instructions, which emphasised speed and accuracy. Participants then completed the 15 practice trials. Then a screen appeared that said "That concludes the practice. Press any colour key to continue. The experiment will start at once." Participants then completed the 105 experimental Stroop trials. Each trial was preceded by a 500-millisecond presentation of a white fixation spot in the centre of a black screen, then the Stroop stimulus appeared. This remained on the screen until a valid response key was pressed, when the fixation spot re-appeared.

Participants then went on to the maths task. This was presented in an envelope, with instructions on the outside reiterating that the paper version was a substitute for a computer-based task that was broken. This was contrived to look like a last minute arrangement. Participants removed the maths task from the envelope and timed themselves with the stopwatch as they completed it. They wrote their answers onto the paper, and noted the time taken at the end.

At this point, participants who had been physically alone in the room came to get the experimenter, and they returned to the experimental room together. The participants then generated data for an experiment that is not reported in this chapter (see Chapter

4). Participants then did the post-experiment questionnaire online in Lime Survey on the experimental PC. This included the following manipulation check questions:

1 “Were you aware of being observed (looked at by a person) during the experiment?”

2 “At the time of typing your name and email did you suspect that it might be an experimental task?”

3 Participants were asked to rate the difficulty of each of the experimental tasks individually on a seven-point Likert scale anchored at 1 (*extremely easy*) and 7 (*extremely difficult*).

4 Participants were asked to rate their familiarity with each of the experimental tasks individually on a seven-point Likert scale anchored at 1 (*never done before*) and 7 (*do it all the time*).

The post-experiment questionnaire included the belief in psi scale, which was presented last to avoid indicating to participants that the experiment had anything to do with parapsychology while the experiment was still underway. Although these final questions were not an experimental task, the observation condition was kept constant. If the participant was being proximately observed, the experimenter stayed in the room and marked the maths task. If the participant was being remotely observed, the experimenter returned to the viewing cubicle and marked the maths task with intermittent glances at the participant’s image. If the participant was alone, the experimenter returned to the observation room without the maths task and kept her attention away from the experiment. The whole experiment took around 45 minutes per participant.

Once the participant had finished, he or she was thanked and shown out of the laboratory. Only once all the participants had completed the experiment and the

results were analysed did they receive emailed feedback debriefing them about the experiment.

8.3.6. Analyses.

Analyses were carried out in SPSS version 14 and Microsoft Excel.

8.4. Results

Manipulation check 1: Did more participants who were observed (proximately or remotely) report feeling observed than participants who were alone?

Participants were asked “Were you aware of being observed (looked at by a person) during the experiment?” and answered *yes* or *no*. Their responses are displayed in Table 29.

Table 29: Frequencies of Participants who did or did not feel Observed by Observation Condition

Felt observed?	Observation Condition		
	Alone N=33	Remote N=33	Proximate N=34
Yes	1	2	31
No	32	31	3

Note. With a total of 100 participants the cell counts are the percentage values.

A visual inspection of the data in Table 29 indicates a clear difference in the number of participants who answered that they felt observed between the observation conditions; a greater number of proximately observed participants felt observed. The difference in the frequencies of *yes* and *no* responses between the observation conditions was assessed by χ^2 analyses. The difference is significant: $\chi^2(2) = 75.12$, $p < .01$ with a large associated effect size, Cramer’s $V = .87$. This difference is only significant between proximate observation and the two physically alone conditions. The difference between alone and remote is non-significant: $\chi^2(1) = .35$, $p = .50$ with

an associated effect size, Cramer’s $V = .07$. Therefore, no more remotely observed participants felt observed than did alone participants, but most proximately observed participants did feel observed. If awareness of the feeling of being observed is crucial to performance effects from observation, we would expect that a social facilitation effect might manifest between proximate observation and the two physically alone conditions in Experiment 5, but not between remote observation and alone.

Manipulation check 2: Did participants suspect that typing the name and email address was an experimental task?

Participants were asked “At the time of typing your name and email did you suspect that it might be an experimental task?” and answered *yes* or *no*. Forty participants (40%) reported suspecting that typing the name and email address was an experimental task. This task was therefore not successfully presented as an incidental part of the experiment set-up. The participants who suspected it might have experienced evaluation apprehension in the alone condition, meaning that a true alone condition was not achieved, lessening the expected difference between the observation conditions and the alone condition.

Manipulation check 3: Were the tasks simple?

Participants rated the difficulty of each experimental task on a seven-point Likert scale anchored at 1 (*extremely easy*) and 7 (*extremely difficult*) and their familiarity with each task on a scale anchored at 1 (*never done before*) and 7 (*do it all the time*). The mean ratings are shown in Table 30.

Table 30: Task Difficulty and Familiarity, from seven-point Likert Scales

	Task			
	Typing	Stroop Control	Stroop Incongruent	Maths
Mean Difficulty	1.28	2.49	4.29	3.56
Mean Familiarity	6.66	2.31	2.43	4.24

Note. $N = 100$.

If a task is simple it should have low difficulty scores and/or high familiarity scores. Table 30 shows that participants found the typing very easy and were familiar with it. Both the Stroop tasks were unfamiliar, with the incongruent colour items rated as more difficult than the controls. The incongruent colour items were not rated as very difficult, however; they are at the midpoint of the scale. The maths task was around the midpoint for both familiarity and difficulty. The tasks were all easy, familiar, or in the middle of the scales and so are simple tasks. Therefore, if social facilitation effects manifest in Experiment 5, we would expect task facilitation from observation.

Hypothesis 1: Participants who are observed, either proximately or remotely, will type their name and email address faster than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

Typing speed was measured as the mean inter-stroke interval of each participant's name and email address combined. There were two extreme outliers in these data, both from participants who reported during the experiment that they were unfamiliar with typing. These data were removed. Table 31 shows the mean typing speed by observation condition.

Table 31: Time taken to Type in milliseconds by Observation Condition

	Observation Condition		
Typing speed	Alone $N=32$	Remote $N=33$	Proximate $N=33$
M (SD)	357.28 (122.59)	370.91 (136.35)	324.01 (91.01)

To test Hypothesis 1, we first checked for the normal social facilitation effect, namely performance facilitation from proximate observation. A one-way ANOVA was calculated with typing speed as the dependent variable. The independent variable was observation condition (proximate observation or alone), between-subjects. There was no significant effect of observation on typing speed: $F(1,63) =$

1.52, $p = .22$, $\eta^2 = 0.02$, observed power = .23. This could have been assessed by an independent t test, but an F test was used to maintain consistency with the other analyses.

Secondly, we investigated the effect of all the observation conditions on performance. A one-way ANOVA was calculated with typing speed as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The difference in typing speed between the three observation conditions was non-significant: $F(2,95) = 1.35$, $p = .26$, $\eta^2 = 0.03$, observed power = .29. This hypothesis was not supported; neither a normal nor a psi-mediated social facilitation effect was found.

The non-significant results for Hypothesis 1 might have been due to confounding personality variables, such as extraversion and neuroticism, or to other individual differences, such as age, gender, and belief in psi. In order to increase the sensitivity of the analysis, the personality and individual differences variables that might have introduced variance were controlled for. Participants' responses to the personality questionnaires are summarised in Table 32.

Table 32: Descriptive Statistics for the Personality Variables

Variable	N	Mean	SD	Minimum	Maximum
Extraversion	100	35.71	5.92	18	48
Neuroticism	100	25.47	6.03	12	42
Belief in psi	98	35.72	16.88	12	81

Note. Two participants refused to answer the belief in psi questionnaire because of strong feelings against parapsychology.

In order to rule out multicollinearity, the inter-correlations between all these variables were assessed by two-tailed Pearson correlation coefficients. A coefficient of over .8 indicates problematic multicollinearity (Field, 2005). If two variables correlate over .8 they should be amalgamated, or one of them should be dropped. All the correlations are presented in Table 33, none of which are over .8.

Table 33: Individual Differences Correlation Coefficients

Variable		Neuroticism	Belief in psi	Gender	Age
Extraversion	<i>N</i> = 100	-.38**	.35**	-0.07	-.30**
Neuroticism	<i>N</i> = 100		-.14	-.16	.18
Belief in psi	<i>N</i> = 98			-.15	.04
Gender	<i>N</i> = 100				.05

Note. * $p < .05$. ** $p < .01$.

The covariates were entered into an ANCOVA (general linear model) with typing speed as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The results are reported in Table 34.

Table 34: Main effects and Interactions of Individual Difference Covariates on time taken to Type

Main Effects from Variables	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	2	0.02	0.00	0.98	0.52
Extraversion	1	4.67*	0.06	0.03	0.57
Neuroticism	1	0.01	0.00	0.91	0.05
Belief in psi	1	0.06	0.00	0.80	0.06
Gender	1	0.16	0.00	0.67	0.07
Age	1	0.11	0.00	0.74	0.06
Error	79	(14185.56)			
Interactions					
Extraversion x Observation	2	0.13	0.00	0.88	0.07
Neuroticism x Observation	2	0.07	0.00	0.94	0.06
Belief in psi x Observation	2	0.75	0.02	0.48	0.17
Gender x Observation	2	2.16	0.05	0.12	0.43
Age x Observation	2	0.00	0.00	>0.99	0.05

Note. *N* = 93.

* $p < .05$. ** $p < .01$.

Values enclosed in parentheses represent mean square errors.

The figures in the top half of Table 34 denote the main effects. The main effects of observation in Table 34 report only the variance that is uniquely attributable to each

variable, with the effects of the covariates controlled for. The main effect of observation is non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render this main effect significant, and so the non-significant results of the ANOVA above were not due to confounding variables.

The main effects of the covariates (in the top half of the table) show the amount of the variance in the dependent variable, typing speed, that each individual difference covariate explains. Partial η^2 gives the effect size for that variable, not influenced by the effect of other variables included in the analysis. Extraversion had a significant association with typing speed, but, as noted above, even when extraversion was controlled for the effect of observation was non-significant. Neuroticism, belief in psi, gender, and age had non-significant main effects on typing speed. The second half of the table displays the interactions for each of these covariates and observation; lower order interactions were included in the model. None of these interactions were significant: None of the individual differences variables had any significant interaction with observation. The interaction between extraversion and observation was non-significant, showing that although extraversion had an association with typing speed, it did not interact with the social facilitation effect.

Hypothesis 2: Participants who are observed, either proximately or remotely, will type their name and email address more accurately (i.e., make fewer errors) than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

Errors were counted as both errors made and then corrected, and errors that remained in the participants' names and email addresses. Correct versions of the participants' email addresses were found via the online system that the students used to sign up for the experiment, or from consent forms, onto which participants had hand-written

their email addresses. The descriptive statistics for the errors in the typing task are displayed in Table 35.

Table 35: Error counts in the Typing Task by Observation Condition

	Observation Condition		
Number of errors	Alone <i>N</i> =33	Remote <i>N</i> =33	Proximate <i>N</i> =34
<i>M</i> (<i>SD</i>)	1.15 (0.28)	0.85 (0.19)	0.94 (0.25)

To test Hypothesis 2, we first checked for the normal social facilitation effect, namely performance facilitation from proximate observation. We used a non-parametric test because the data in Table 35 were not normally distributed. The Mann-Whitney U test is the non-parametric equivalent of an independent *t* test or one-by-two ANOVA (Field, 2005). This showed that there was a non-significant difference in the error rates: $U = 494.50, p = .37$ (two-tailed).

Secondly, we checked the effect of all the observation conditions on performance. We used a Kruskal-Wallis test, the non-parametric equivalent of a one-way independent ANOVA (Field, 2005). The difference in the frequency of participants with errors between observation condition was non-significant: $H(2) = .94, p = .63$. This hypothesis was not supported; neither a psi-mediated nor a normal social facilitation effect was found. As the data for typing errors was not normally distributed, further investigations with the individual differences variables were not made.

Hypothesis 3: Participants who are observed, either proximately or remotely, will demonstrate reduced Stroop interference compared to participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

Participants' reaction times in response to each item were recorded, and the mean reaction times for the incongruent colour items and the control items were calculated. Stroop interference was calculated as the mean incongruent colour item reaction time minus the mean control reaction time for each participant.

Exploratory data analysis revealed there were two participants with high outlying error scores. One had made many mistakes of answering with the colour that the word spelled, but otherwise completed the task correctly. It appeared that she had understood the task, but found it very difficult; her data were retained. The other high error scorer had done the task about 10 times faster than the norm and the errors seemed indiscriminate. We concluded that this participant had just pressed random buttons as fast as possible; his data were excluded. One participant did not attempt the Stroop due to colour blindness. Due to a technical error, the data for three participants were lost. Data for 95 participants were retained for analysis. One of these remaining data points was a very slow outlier on the Stroop. This was recoded to the mean (of all the other participants) plus two standard deviations (Field, 2005). The descriptive statistics of the analysis dataset are presented in Table 36.

Table 36: Reaction Times and Stroop Interference in milliseconds by Observation Condition and Item Type

Mean time (SD)	Observation Condition		
	Alone N=32	Remote N=32	Proximate N=31
Incongruent items	1,357 (253)	1,297 (248)	1,245 (174)
Control items	1,223 (246)	1,170 (212)	1,148 (198)
Interference	134 (123)	128 (100)	96 (108)

A visual inspection of the data in Table 36 reveals that the reaction times for the control and incongruent colour items and the Stroop interference decrease from alone to remote observation, and again from remote observation to proximate observation. A Stroop interference effect was found: Participants responded faster to the control items ($M = 1,180$, $SD = 220$) than to the incongruent colour items ($M = 1,300$, $SD = 231$), $t(94) = 10.54$, $p < .01$ (two-tailed).

To test Hypothesis 3, we first checked for the normal social facilitation effect, namely a reduction in Stroop interference in the proximate observation condition. A one-way ANOVA was calculated with Stroop interference as the dependent variable. The independent variable was observation condition (proximate observation or alone), between-subjects. The Welch correction for non-homogeneity of variances was applied. There was no significant effect of observation on typing speed: $F(1,56.25) = 2.45$, $p = .12$, $\eta^2 = 0.04$, observed power = .34. This could have been assessed by independent t test, but F was used to maintain consistency with the other analyses.

Secondly, we investigated the effect of all the observation conditions on performance. A one-way ANOVA was calculated with Stroop interference as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The Welch correction for non-homogeneity of variances was applied. The difference in Stroop interference between the three observation conditions was non-significant: $F(2,60) = 1.38$, $p = .26$, $\eta^2 = 0.03$, observed power = .30. This hypothesis was not supported; neither a normal nor a psi-mediated social facilitation effect was found.

The non-significant results for Hypothesis 3 might have been due to confounding personality variables, such as extraversion and neuroticism, or to individual differences, such as belief in psi, gender, and age. In order to increase the sensitivity of the analysis, the personality and individual differences variables that might have

introduced variance were controlled for. The covariates (see Table 33) were entered into an ANCOVA (general linear model) with Stroop interference as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The results are reported in Table 37.

Table 37: Main Effects and Interactions of Individual Difference covariates on Stroop Interference

Main Effects from Variables	<i>df</i>	<i>F</i>	Partial η^2	<i>P</i>	Power
Observation	2	0.60	0.16	0.55	0.15
Extraversion	1	0.18	0.00	0.89	0.05
Neuroticism	1	0.39	0.00	0.84	0.05
Belief in psi	1	0.25	0.00	0.62	0.08
Gender	1	0.24	0.00	0.62	0.07
Age	1	2.35	0.03	0.13	0.33
Error	75	(0.05)			
Interactions					
Extraversion x Observation	2	1.02	0.03	0.37	0.22
Neuroticism x Observation	2	0.88	0.00	0.92	0.06
Belief in psi x Observation	2	1.03	0.03	0.36	0.22
Gender x Observation	2	0.37	0.01	0.69	0.11
Age x Observation	2	1.37	0.04	0.26	0.29

Note. $N = 93$.

Values enclosed in parentheses represent mean square errors.

The figures in the top half of Table 37 denote the main effects. The main effects of observation in Table 37 report only the variance that is uniquely attributable to each variable, with the effects of the covariates controlled for. The main effect of observation is non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render this main effect significant, and so the non-significant results of the ANOVA in Hypothesis 3 were not due to confounding variables.

The main effects of the covariates (in the top half of the table) show the amount of the variance in the dependent variable, Stroop interference, that each individual difference covariate explains. Partial η^2 gives the effect size for that variable, not influenced by the effect of other variables included in the analysis. None of the individual differences variables had a significant main effect on typing speed. The second half of the table displays the interactions between each of the covariates and observation; lower order interactions were included in the model. None of these interactions are significant; none of the individual differences variables had any significant interaction with observation.

Hypothesis 4: Participants who are observed, either proximately or remotely, will make fewer errors on the Stroop task than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

An error in the Stroop task was defined as responding with any response key other than the one corresponding to the colour of the stimulus, and errors were summed for the control and incongruent colour items combined. The same five participants' data were omitted as in the analysis for Hypothesis 3, above. The descriptive statistics for the error data is displayed in Table 38.

Table 38: Means and standard deviations of errors in the Stroop task by observation condition

Number of errors	Observation Condition		
	Alone N=32	Remote N=32	Proximate N=31
<i>M (SD)</i>	4.06 (1.17)	3.13 (0.33)	2.26 (0.24)

Note. $N = 95$.

To test Hypothesis 3, we first checked for the normal social facilitation effect, namely performance facilitation from proximate observation. A Mann-Whitney U test was used because the data in Table 38 were not normally distributed. This

showed that there was a non-significant difference in the error rates: $U = 450.50$, $p = .52$ (two-tailed).

Secondly, we checked the effect of all the observation conditions on performance. The difference in the number of errors between the observation conditions was assessed by a Kruskal-Wallis test. There was no significant effect from observation: $H(2) = 4.29$, $p = .12$. This hypothesis was not supported; neither a psi-mediated nor a normal social facilitation effect was found. As the data for Stroop errors was not normally distributed, further investigations with the individual differences variables were not made.

Hypothesis 5: Participants who are observed, either proximately or remotely, will complete the maths task faster than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

The total time taken to complete the maths task was timed by the participants. The descriptive statistics for the time taken to complete the maths task are displayed in Table 39.

Table 39: Time in seconds to complete the Maths Task by Observation Condition

	Observation Condition		
Maths task time	Alone N=33	Remote N=33	Proximate N=34
$M(SD)$	184.13 (91.49)	230.40 (137.49)	176.78 (76.45)

To test Hypothesis 5, we first checked for the normal social facilitation effect, namely performance facilitation from proximate observation. A one-way ANOVA was calculated with time taken to complete the maths task as the dependent variable. The independent variable was observation condition (proximate observation or alone), between-subjects. There was no significant effect of observation on the time taken to complete the maths task: $F(1,65) = 0.13$, $p = .72$, $\eta^2 < 0.01$, observed power

= .06. This could have been assessed by an independent t test, but an F test was used to maintain consistency with the other analyses.

Secondly, we investigated the effect of all the observation conditions on performance. A one-way ANOVA was calculated with time taken to complete the maths task as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The Welch correction for non-homogeneity of variances was applied. There was no significant effect of observation conditions on times: $F(2,61.45) = 1.94$, $p = .15$, $\eta^2 = 0.05$, observed power = .50. This hypothesis was not supported; neither a normal nor a psi-mediated social facilitation effect was found.

The non-significant results for Hypothesis 5 might have been due to confounding personality variables, such as extraversion and neuroticism, or other individual differences, such as belief in psi, gender, and age. In order to increase the sensitivity of the analysis, the personality and individual differences variables that might have introduced variance were controlled for. The covariates (see Table 33) were entered into an ANCOVA (general linear model) with time taken to complete the maths task as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The results are reported in Table 40.

Table 40: Main Effects and Interactions of Individual Difference covariates on the time taken to complete the Maths Task

Main Effects from Variables	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	2	0.19	0.00	0.98	0.53
Extraversion	1	0.06	0.00	0.81	0.57
Neuroticism	1	0.11	0.00	0.75	0.06
Belief in psi	1	0.80	0.10	0.38	0.14
Gender	1	2.39	0.03	0.13	0.33
Age	1	0.05	0.00	0.82	0.06
Error	80	(10728.47)			
Interactions					
Extraversion x Observation	2	1.39	0.03	0.26	0.29
Neuroticism x Observation	2	1.32	0.03	0.27	0.28
Belief in psi x Observation	2	1.48	0.03	0.23	0.31
Gender x Observation	2	0.26	0.07	0.77	0.09
Age x Observation	2	1.15	0.03	0.32	0.25

Note. *N* = 98.

Values enclosed in parentheses represent mean square errors.

The figures in the top half of Table 40 denote the main effects. The main effects of observation in Table 40 report only the variance that is uniquely attributable to each variable, with the effects of the covariates controlled for. The main effect of observation is non-significant. Controlling for the variance attributable to the personality and individual differences measures did not render this main effect significant, and so the non-significant results of the ANOVA in Hypothesis 5 were not due to confounding variables.

The main effects of the covariates (in the top half of the table) show the amount of the variance in the dependent variable, the time taken to complete the maths task, that each individual difference covariate explains. Partial η^2 gives the effect size for that variable, not influenced by the effect of other variables included in the analysis. None of the individual differences variables had a significant main effect on maths

task speed. The second half of the table displays the interactions for each of these covariates and observation; lower order interactions were included in the model. None of these interactions are significant; none of the individual differences variables had any significant interaction with observation.

Hypothesis 6: Participants who are observed, either proximately or remotely, will make fewer errors on the maths task than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

Errors were counted as both errors that remained, and errors that participants had corrected. Table 41 displays the mean error rate on the maths task by observation condition.

Table 41: Errors in the Maths task by Observation Condition

	Observation Condition		
Number of errors	Alone $N=33$	Remote $N=33$	Proximate $N=34$
$M (SD)$	1.82 (0.36)	2.30 (0.56)	3.62 (0.97)

To test Hypothesis 6, we first checked for the normal social facilitation effect, of performance facilitation from proximate observation. As these data were not normally distributed, a Mann-Whitney U test was run. This showed that there was a non-significant difference in the error rates: $U = 473.00, p = .26$ (two-tailed).

Secondly, we investigated the effect of all the observation conditions on performance. The difference in the error rates between observation conditions was assessed by a Kruskal-Wallis test, and was found to be non-significant: $H(2) = 1.60, p = .45$. Observation did not significantly influence the accuracy of the maths task. This hypothesis was not supported; neither a normal nor a psi-mediated social facilitation effect was found. As the maths error data was not normally distributed, further investigations with the individual differences variables were not made.

Hypothesis 7: Participants who are observed, either proximately or remotely, will have higher state anxiety ratings than participants who are alone. We tentatively predict that proximate observation will have a greater effect than remote observation.

Participants self-rated their state anxiety levels on a short form of the STAI. The descriptive statistics for state anxiety scores are displayed in Table 42.

Table 42: State Anxiety by Observation Condition

	Observation Condition		
	Alone N=33	Remote N=33	Proximate N=34
<i>M (SD)</i>	9.97 (2.78)	10.94 (2.88)	9.59 (2.30)

To test Hypothesis 7, we first checked whether there was a difference in state anxiety ratings between participants in the proximate observation and alone conditions. A one-way ANOVA was calculated with state anxiety as the dependent variable. The independent variable was observation condition (proximate observation or alone), between-subjects. There was no significant effect of observation on state anxiety scores: $F(1,65) = 0.38, p = .54, \eta^2 < 0.01$, observed power = .09.

Secondly, we investigated the effect of all the observation conditions on state anxiety. A one-way ANOVA was calculated with state anxiety as the dependent variable. The independent variable was observation condition (proximate observation, remote observation, or alone), between-subjects. The difference in state anxiety between the three observation conditions was non-significant: $F(2,97) = 2.28, p = .11, \eta^2 = 0.05$, observed power = .45. This hypothesis was not supported.

As Experiment 5 used a between-subjects design, there might have been differences in participants' neuroticism levels. This would affect participants' propensity to become state anxious. The effect of observation could be masked by an effect from neuroticism. To control for neuroticism it was entered as a covariate into an

ANCOVA with observation (proximate observation, remote observation or alone) between-subjects, and state anxiety scores as the dependent variable.

Table 43: ANCOVA of Observation on State Anxiety, corrected for Neuroticism

Variable	<i>df</i>	<i>F</i>	Partial η^2	<i>p</i>	Power
Observation	2	1.28	.03	.28	.27
Neuroticism (covariate)	1	16.76**	.15	<.01	.98
Error	96	(6.10)			

Note. *N* = 100.

* *p* < .05. ** *p* < .01.

Values enclosed in parentheses represent mean square errors.

The results in Table 43 indicate that there was no significant main effect of observation condition on state anxiety, when corrected for neuroticism. Neuroticism significantly predicted the state anxiety scores. This hypothesis was not supported.

Hypothesis 5: Participants who are observed, either proximately or remotely, will rate the difficulty of the tasks (typing, Stroop, and maths) differently to the participants who are alone. We tentatively predict that proximate observation will have a greater effect in this than remote observation.

Participants rated all the tasks on seven-point Likert scales anchored at 1 (*extremely easy*) and 7 (*extremely difficult*). Table 44 displays the descriptive statistics for task difficulty by observation condition.

Table 44: Mean Perceived Task Difficulty by Task and Observation Condition

Task	Observation Condition		
	Alone <i>N</i> =33	Remote <i>N</i> =33	Proximate <i>N</i> =34
Typing	1.27	1.12	1.44
Control items	2.45	2.67	2.35
Incongruent items	4.21	4.42	4.24
Maths	3.15	3.67	3.85

To test Hypothesis 7, we first checked whether there was a difference in the perceived difficulty ratings between participants in the proximate observation and alone conditions. A Mann-Whitney U test was run on the typing data, which were highly skewed. One-way ANOVAs with observation condition (proximate observation or alone) as the independent variable, between-subjects, were run on the Stroop and maths task data. There were no significant differences in task difficulty by observation condition: typing: $U = 487.00$, $p = .23$ (two-tailed); control items: $F(1,65) = .12$, $p = .73$ $\eta^2 = 0.01$, observed power = .63; incongruent colour items: $F(1, 65) = .01$, $p = .95$ $\eta^2 = 0.01$, observed power = .50; or the maths task: $F(1, 65) = 2.87$, $p = .10$ $\eta^2 = 0.04$, observed power = .39.

Secondly, we investigated the effect of all the observation conditions on perceived task difficulty. A Kruskal-Wallis test was used on the typing data, which were highly skewed. One-way ANOVAs with observation condition (proximate observation, remote observation or alone) as the independent variable, between-subjects, were run on the other dependent variables. There were no significant differences in task difficulty by observation condition: typing: $H(2) = 2.64$, $p = .27$; control items: $F(2,97) = .57$, $p = .57$ $\eta^2 = 0.01$, observed power = .14; incongruent colour items: $F(2,97) = .22$, $p = .81$ $\eta^2 = 0.04$, observed power = .83; or the maths task: $F(2,97) =$

1.55, $p=.22$ $\eta^2 = 0.03$, observed power = .32. Therefore, this hypothesis was not supported.

8.5. Discussion

Experiment 5 aimed to discover whether people's behaviour nonintentionally responds in the same way to remote observation (observation one can only be affected by via psi) as it responds to proximate observation. In other words, it aimed to discover whether remote observation can lead to the social facilitation effect. In Experiment 5 all the tasks were simple, as confirmed by the manipulation checks. We predicted that observation would facilitate their performance, in terms of both speed and accuracy. Participants completed a simple typing task (typing their full name and email address), a colour Stroop, and a simple multiplication task under either proximate observation, remote observation, or alone conditions. We also investigated the possibility of observation increasing participants' state anxiety, or altering their perception of the difficulty of the tasks. The findings for each of these areas will be considered in turn.

Neither proximate observation nor remote observation increased participants' typing speed, even with potentially confounding individual differences controlled for. Neither observation influence increased participants' accuracy on this task either. This was unexpected, because in Experiment 4 there was a trend for proximate observation to increase the speed of participants typing their name and code, and a post-hoc analysis found that the code was significantly facilitated. In Experiment 3, also, the code was typed significantly faster when participants were proximately observed. The difference in findings could be due to the different tasks used: In Experiments 3 and 4 typing an alphanumeric code was facilitated, in Experiment 5 participants did not type this code, but their names and email addresses.

The typing tasks were based upon name and code tasks that elicited a social facilitation effect in previous research (Schmitt et al., 1986), but the experiments presented in this thesis failed to replicate the social facilitation effect with these tasks. This could be because Schmitt et al.'s findings were spurious. On the other hand, social facilitation effects have been found with data entry tasks, which are also simple typing tasks. For instance, proximate observation increased the speed (Aiello & Kolb, 1995) and accuracy (Aiello & Kolb, 1997) of a data entry task of copying strings of six digits. However, no performance effects were found comparing proximate observation to being alone on another data entry task (Griffith, 1993). Possibly, typing and data entry do not respond reliably to observation. It is also possible that there are other factors that affect whether typing responds to observation. For instance, Aiello and Kolb (1995) found that the effect of observation was mediated by participants' initial skill level; highly skilled participants' performance was facilitated by observation, and less skilled participants' performance was reduced. Therefore, a wide initial skill mix might produce an overall null effect that masks internal social facilitation effects. In Experiment 5, we did not check participants' skill at typing, so it is possible that a wide range of initial skill masked an observation effect. Future research with performance tasks should take initial skill level into account.

One reason for using these typing tasks was because they had successfully functioned as covert tasks (Schmitt et al., 1986), and this is important in creating a true alone condition (Markus, 1978). In Experiment 5, however, 40% of the participants suspected that the typing was an experimental task. Therefore, this manipulation failed, and this experiment lacked a true alone condition. It follows that participants might have felt evaluation apprehension in the alone condition; this is another possible explanation for the null results.

Neither proximate nor remote observation increased participants' speed of completing a maths task, even with potentially confounding individual differences

controlled for. Neither observation influence increased participants' accuracy on this task either. This was unexpected, as this task had been based on a maths task that demonstrated a social facilitation effect in a previous experiment (Grant & Dajee, 2003). It is possible that differences between the tasks used by Grant and Dajee (2003) and those in Experiment 5 led to the difference in outcomes. Grant and Dajee's (2003) maths task had 45 questions, and none of the digits in the multiplication tasks were over 10. In Experiment 5 there were 50 questions and the digits in the multiplication tasks went up to 12. Although these differences may seem minor, there is a possibility that they made the maths task in Experiment 5 more difficult than Grant and Dajee's (2003) task. Participants in Grant and Dajee's (2003) experiment performed their maths task faster when they were observed, in other words it behaved as a simple task. Participants in Experiment 5 rated the maths task as being just to the *easy* side of the midpoint when rating task difficulty. Although this means that the maths task was easy (as opposed to difficult), there is a possibility that it was not easy enough to be facilitated by observation. According to the optimal arousal curve (see Chapter 6) there is a region, between tasks that would be facilitated and tasks that would be inhibited, in which there is little or no effect from observation. Maybe the maths task was in this region. This suggestion comes with the caveat that participants' subjective ratings of task difficulty might not reliably indicate the direction in which observation affects task performance. There is also a further caveat in that Grant and Dajee (2003) did not take task difficulty ratings, so there is no objective basis for comparing task difficulty between these two experiments. One previous maths task did not find simple task facilitation from observation, although it did find complex task inhibition, of both speed and accuracy (Zanbaka et al., 2007). Zanbaka et al. attributed their failure to find simple task facilitation to ceiling effects. Ceiling effects are unlikely to be the reason for the null effect in Experiment 5, as the maths task was designed to be more difficult than that used by Grant and Dajee (2003). It would benefit future research to analyse ratings of perceived task difficulty and participants' familiarity with tasks. This could lead to a

greater understanding of just how easy, difficult, familiar, or novel tasks need to be to manifest facilitation or inhibition from observation.

There was another difference between Experiment 5 and Grant and Dajee's (2003) experiment that might have affected the results. In Experiment 5 the observer was the experimenter, and in Grant and Dajee's experiment the observer was another participant. In both experiments the observer was unfamiliar to the participant. In Experiment 5 the observer had expert status, and so should have exerted a greater social facilitation effect than the peer observer in Grant and Dajee's (2003) experiment (Guerin, 1993; Henchy & Glass, 1968; F. G. Miller et al., 1979). There is prior evidence that experimenters elicit the social facilitation effect when they observe (Ekdahl, 1929; Ferris & Rowland, 1980). Therefore, although the observer type differed between these two experiments, there was no reason indicated in previous literature for the observer in Experiment 5 not to induce a social facilitation effect.

Observation, whether proximate or remote, did not reduce Stroop interference, nor did it reduce the number of errors made. Considering firstly the effect on the error rates, previous studies have found low error rates (under 5%) (Dumas et al., 2005; Klauer et al., 2008), as did Experiment 5. In these previous studies, observation did not affect the error rate, which could be due to floor effects because the error rate was low. One previous study did find that arousal, induced by the threat of electric shocks, affected the error rate (Pallack, Pittman, Heller, & Munson, 1975). Although the effect of observation is thought to be mediated by arousal (Zajonc, 1965), the threat of electric shocks is likely to exert a stronger arousal-increasing influence than observation. Observation is possibly too weak an influence to manifest an effect on Stroop error rate.

Observation has been found to reduce Stroop interference, however, by reducing both the reaction time to control and incongruent colour items (Huguet et al., 1999;

Klauer et al., 2008). Experiment 5 found a Stroop interference effect, but did not find any effect from observation, even after controlling for potentially confounding individual differences measures. Experiment 5 was closely modelled on the experiments cited above, and so was expected to replicate their effects. In Experiment 5, the observer stood in the same relative position to the participant (for proximate observation) as did the observer in Klauer et al.'s (2008) experiment, and this was the most successful of all the observer locations compared by Huguet et al. (1999). The Stroop stimuli used, and the method of response (pressing buttons on a keyboard), were very similar to those used in previous research that found that observation reduced Stroop interference (Huguet et al., 1999; Klauer et al., 2008). One possible reason for the failure to replicate an observation effect on Stroop interference was the length of practice time given to the participants. Participants in Experiment 5 had a 15-trial practice. In comparison, participants in Dumas et al.'s (2004; 2005) experiments had a 100-trial practice, and these experiments found competition significantly reduced Stroop interference. Possibly as a result of the short practice time, participants in Experiment 5 had slightly longer reaction times (control and incongruent colour item means were all over one second) in comparison to previous studies (most means just under one second) (Dumas et al., 2005; Huguet et al., 2004; Huguet et al., 1999; Klauer et al., 2008). This slower reaction time could also have resulted from the five-colour choice that participants made in Experiment 5. Experiment 5 included five colours in the Stroop, instead of four as used previously (Dumas et al., 2005; Huguet et al., 2004; Huguet et al., 1999; Klauer et al., 2008). These differences could all have contributed to the null findings.

We also hypothesised that observation, both proximate and remote, would increase participants' state anxiety. This hypothesis was not supported; no significant difference in state anxiety was found between the observation conditions. State anxiety was measured to capture any effect from observation on participants' arousal during the experiment. Prior research has found that observation causes increases in stress, a similar construct to arousal (Aiello & Svec, 1993). Studies that investigated

physiological indicators of arousal found that proximate observation increased participants' arousal (Chapman, 1974; Mullen et al., 1997). However, there is more evidence that observation increased arousal when participants were performing complex tasks (Gendolla & Richter, 2006; Martens, 1969) or in conjunction with arousal-increasing stimuli (Rourke, Marshall-Goodell, Tassinary, & Baron, 1990). The null result in Experiment 5 might therefore be because the participants were performing simple tasks, not complex tasks. Alternatively, it might be because the observation influence in Experiment 5 did not increase arousal, which corroborates the null task performance findings discussed so far. If the social facilitation effect is mediated by arousal, and arousal was unaffected by observation in this experiment, it follows that performance would also be unaffected. The question of why observation seemed to exert such a weak effect remains.

We also investigated whether observation, both proximate and remote, would affect participants' ratings of task difficulty. There were no significant differences between the ratings of task difficulty due to observation condition; therefore, this hypothesis was not supported. Observation has been found to increase the perceived difficulty of complex tasks in one prior social facilitation study (Bradner & Mark, 2001). It is not known whether observation would affect the perceived difficulty of simple tasks. The null results found here could indicate that observation does not affect participants' perception of simple task difficulty. Alternatively, the null results could corroborate the findings relating to task performance and state anxiety: that there was no effect from observation in Experiment 5.

In summary of the findings so far, observation, whether proximate or remote, did not increase the speed or accuracy of any of the tasks, increase participants' state anxiety ratings, or alter their perception of the difficulty of the tasks. Perhaps this was because the observation influences did not exert any effect on the participants. We attempted to check whether the observation condition manipulation had been successful by asking participants whether they had been aware of being observed

during the experiment. Significantly more participants in the proximate observation condition reported awareness of being observed than in either of the physically alone conditions. In other words, the proximately observed participants were aware of being observed and still this did not significantly affect their behaviour, anxiety, or perception of the tasks. Therefore, awareness of observation, in Experiment 5 at least, was not a sufficient condition to manifest the social facilitation effect.

There was no significant difference between the numbers of participants who reported awareness of being observed between the remote observation and alone conditions. Almost all the participants in the physically alone conditions reported no awareness of being observed. This might have been because of the way the question was phrased; it specified that observed meant “looked at by a person”. Possibly, participants would be less likely to answer yes when there was no other person in the room. Therefore, this question was probably an inadequate check for the feeling of being remotely observed. The experimenter noticed that many participants checked the room for hidden ways of observing them when they read this question (they did not find the webcam), so it is likely that they interpreted it as asking whether they were observed rather than that they felt observed. It would be important for future experiments to check the manipulation of observation more carefully. It is also possible that in the confines of a small room with no windows people are unlikely to feel observed. If the ability to detect remote observation developed from an evolutionary advantage of being able to detect predators’ gaze, it is plausible that this ability is usually prompted only in situations where it might seem possible to be seen.

An alternative explanation for almost no participants being aware of being observed in the remote observation condition is that there was no psi-mediated remote observation effect in this experiment. In previous research, participants have demonstrated that they can consciously detect remote observation at above chance rates (e.g., Colwell et al., 2000; Sheldrake, 2000; Williams, 1983), but other studies have not found this effect (e.g., Wiseman & Smith, 1994). In those previous

experiments, participants were informed that they would be remotely observed, but not when. This is a different influence from that used in Experiment 5, when the participants were not informed that they would be remotely observed. Only two previous remote observation studies have used remote observation without informing the participants. Lobach and Bierman (2004) did not find that participants' physiology differentiated between remote observation and not being observed when participants were not informed about the remote observation. Sheldrake (2003) found that a higher proportion of people in a crowd turned to look in the direction of concealed observers when they were observing them remotely than when they were not observing them. Further research is required into people's ability to detect and respond to remote observation when they have not been informed that it will occur, but the findings from Experiment 5 do not provide support for either the detection of remote observation or the social facilitation effect.

8.6. Conclusions

Observation, whether proximate or remote, did not increase the speed or accuracy of participants' performance on simple typing or maths tasks, or reduce Stroop interference. Observation also did not increase participants' state anxiety or alter their perception of the difficulty of the tasks. There was no social facilitation effect found in Experiment 5. As we did not find a normal social facilitation effect (between proximate observation and alone), we could conclude that these tasks did not respond to observation influences. Alternatively, we could conclude that the observation influences in Experiment 5 were insufficient to produce the social facilitation effect, despite the fact that the proximately observed participants were aware that they were being observed. This lack of the normal social facilitation effect means that the possibility of a social facilitation effect from remote observation was not adequately tested in this experiment. If the normal facilitation of simple task performance had occurred, then there would have been a baseline from which to conceptually calibrate the effect of remote observation. Nonetheless, if remote

observation had produced any significant performance facilitation or inhibition compared to either of the other observation conditions, unexpected though that would have been, it would have shown that there was some effect from remote observation. With the null results found we can merely conclude that neither proximate observation nor remote observation affected participants' performance in this experiment.

9. General Discussion of Experiments 3, 4, and 5: Psi-Mediated Social Facilitation from Remote Observation

This section reviews the findings from Experiments 3, 4, and 5. We start by briefly restating the aims of these studies, and comparing similarities and differences across the findings of these studies. The null effect of remote observation from a hidden observer is interpreted in light of the predominantly null effect from a physically present observer. From the limitations discovered in Experiments 3, 4, and 5 we discuss how future research might establish the optimal conditions for investigating a social facilitation effect from remote observation. We suggest ways forward for further investigations into the possibility of psi-mediated social facilitation effects. Throughout this general discussion, we also address what implications our findings have for the wider literature.

Considered together, Experiments 3, 4, and 5 aimed to discover whether people's behaviour nonintentionally responds in the same way to remote observation (observation they can only be affected by via psi) as it responds to proximate observation. In other words, they aimed to discover whether remote observation can lead to the social facilitation effect. We hoped that these experiments would add to both of the fields of social facilitation and remote observation, and inform psychology methodology in general. Many previous social facilitation experiments used covert observation to assess performance in the condition they called alone (e.g., Markus, 1978; C. F. Bond & Titus, 1983) as if this observation, that the participants were not informed of and could not perceive sensorily, would make no cogent difference to their behaviour. The experiments reported in this thesis are the first to check whether covert observation (as remote observation by hidden observers) has any social facilitation effect. Thus, our experiments are the first to test whether or not that assumption is justified, and thus make an important contribution to the social facilitation literature. Our experiments also advanced the field of parapsychology. Prior remote observation research predominantly focussed upon

conscious guess and psychophysiological measures. Only one previous study considered the effect of remote observation on task performance (Lee et al., 2002), and this study did not take into account the effect of proximate observation on the behaviour under investigation. Ours are the first experiments to compare corresponding psi- and non-psi-mediated observation influences on behaviour. We hoped that the social facilitation effect would become a useful tool with which to investigate the effect of remote observation. Lastly, if the presence of, and observation by, the experimenter can alter participants' behaviour, this should be taken into consideration in all psychology experimental research designs; however, it rarely is (Huguet et al., 1999). If remote observation or attention from the experimenter can affect participants' task performance, then experiments are open to many more influences than are usually considered. These influences could help us understand the experimenter effect, an important consideration in psychology and parapsychology (Palmer, 1997). Experiments 3, 4, and 5 were a preliminary inquiry into whether these remote observation effects should be considered in experimental designs.

In Experiment 3, participants completed typing tasks that were presented as being the log-in to another experiment, and so should have been covert assessments of performance. The participants typed both their first name (simple task) and a code (complex task) three times, under different observation conditions each time: alone, remotely observed, or proximately observed. We predicted that observation (both remote and proximate) would increase the speed of typing the name, and decrease the speed of typing the code. The hypothesised interaction between observation and complexity was not found. Instead, the speed of typing the code was increased by proximate observation.

Certain limitations in the methodology and apparatus of Experiment 3 were identified, and Experiment 4 aimed to overcome these limitations. Experiment 4 aimed to obtain the normal (non-psi-mediated) social facilitation effect. We predicted

facilitation of simple task performance and inhibition of complex task performance from proximate observation compared to being alone. Participants completed typing tasks which were presented as being the log-in to another experiment. The participants typed both their full name (simple task) and a code (complex task) once, under either proximate observation or while alone. Two measures of task performance: speed and accuracy, were measured, in conjunction with two further indicators of the effect of observation: perceived task difficulty and state anxiety. We predicted that proximate observation would increase the speed and accuracy of typing the name, and decrease the speed and accuracy of typing the code. We also predicted that proximate observation would increase participants' state anxiety ratings, and affect their perception of the difficulty of the tasks.

None of these hypotheses were supported. The social facilitation effect, the interaction between observation and task complexity, was not found using any of these measures. Typing the code, however, which we intended to be the complex task, was facilitated by observation: Participants typed it faster when proximately observed. The code also made some participants suspect that they were being assessed on the typing tasks. This was a problem, because the typing tasks were supposed to be covert. Covert tasks are important in creating a true alone condition (Markus, 1978), which was not achieved in Experiment 4. It is important to create a true alone condition to create an influence-free a baseline as possible. The influence of remote observation was expected to have a small social facilitation effect. If there were any other social facilitation effect-inducing stimuli in the alone condition, the chances of finding a social facilitation effect from remote observation would have been reduced. The fact that the tasks were not covert compromised the alone condition in Experiment 4, as evaluation apprehension, caused by participant's awareness that their performance is being evaluated, can lead to the social facilitation effect (Henchy & Glass, 1968). This may have nullified any social facilitation effect from remote observation in this experiment.

As we had not found the normal social facilitation effect with the typing tasks, we used different tasks in Experiment 5. As the only observation effects we had found in Experiments 3 and 4 had been facilitatory (increased speed of typing the code), we decided to look for facilitation effects exclusively, and therefore only used simple tasks. Participants completed a simple typing task (typing their full name and email address), a colour Stroop, and a simple multiplication task under either proximate observation, remote observation, or alone conditions. We predicted that observation would facilitate task performance, both in terms of speed and accuracy, for the typing task and the maths task, and would reduce Stroop interference. We also investigated the possibility of observation increasing participants' state anxiety, and perception of the difficulty of the tasks. There were no observation effects found with any of these tasks or measures.

In Experiments 3, 4, and 5, an array of individual differences that might have confounded the social facilitation effect were controlled for. These were: extraversion, neuroticism, belief in psi, age, gender, and (in Experiment 3 only) state anxiety. While belief in psi was an exploratory variable, extraversion, neuroticism, and state anxiety were included based upon prior research showing that they interact with observation, task complexity, or both (see Chapter 6). In the process of controlling for these variables, any main effects or interactions with observation, complexity, or both would have emerged. None of the individual differences measures had significant main effects (even when considered individually at exploratory stages of the analysis), or interactions with observation and/or complexity. This could be for the simple reason that there was no social facilitation effect in these experiments, and so these variables had nothing to interact with. Importantly, controlling for the individual differences measures did not render any of the social facilitation effects significant.

A recent meta-analysis of the social facilitation and personality literature found that personality types might be more important than task complexity in determining the

direction of the effect observation has on performance. Uziel (2007) identified two personality types that relate to how one experiences social situations. The first was *positive-self-assured* people, characterised by extraversion and high self-esteem, whose task performance is facilitated by observation. The second personality type was *negative-apprehensive* people, characterised by neuroticism and low self-esteem, whose task performance is inhibited by observation. Experiments 3, 4, and 5 failed to confirm Uziel's hypotheses, as there were no significant interactions between observation and either extraversion or neuroticism. Again, this might simply be because there was a meagre effect from observation in our studies, rather than there being no effect of personality on how people react to observation. It is important, therefore, for future research into the social facilitation effect to consider the role of personality. This could be achieved by controlling for the effects of personality, as we did in the experiments presented in this thesis. Alternatively, as Uziel found that the personality types interacted with observation, not with observation and complexity, future research could focus on the effect of observation and personality on performance. This might increase the chances of finding an observation effect, and, thereby, increase the chances of finding an effect from remote observation, if there is one. Another suggestion for future research, if Uziel's proposals are correct, would be to use experimental samples of either positive-self-assured or negative-apprehensive participants so that the direction of the effect of observation on performance could be predicted.

One similarity between the findings of Experiments 3 and 4 was that proximate observation increased participants' typing speed on the code. Typing the code was originally intended to be a complex task, and we therefore expected that it would be slowed by observation (following Schmitt et al., 1986). However, all the evidence points to the code having behaved as if it were a simple task: Proximately observed participants typed it faster and participants rated it as quite easy. If, post-hoc, we can reconceptualise the code as being a simple task, then in both Experiments 3 and 4 there was some evidence of a social facilitation effect from proximate observation, in

that the speed of typing the code was increased. Experiment 4 did not include a remote observation condition, but Experiment 3 did. So, within Experiment 3, we can compare participants' speed of typing the code between alone, remote observation, and proximate observation conditions, in a task that demonstrated a social facilitation effect. The post-hoc tests conducted revealed that there was no significant difference between the alone and remote observation conditions in the speed of typing the code. There was, however, a significant difference between alone and proximate observation, and a marginally significant difference between remote observation and proximate observation in the speed of typing the code. All of these analyses are post-hoc, and assess an effect that went the opposite way to the hypothesis, so any conclusions drawn can only be tentative. With those caveats, these findings indicate that remote observation had a more similar effect to being alone than to being proximately observed. In other words, there was no social facilitation effect from remote observation.

This finding is in accordance with the most similar research in the previous literature. Lee et al. (2002) compared the number of errors that participants made on a wobbly wire task between remote observation and an alone condition. In both conditions a camera was present, and participants knew that they might be observed through it at any time. No significant difference in the number of errors made on the wobbly wire task between remote observation and the alone condition were found. Further research which finds the social facilitation effect between a true alone condition and a proximate observation condition is required to further explore the effect of remote observation on behaviour, and we will return to the types of tasks and observation that might be optimal for this after the overview of the experiments.

In Experiment 5, there were no significant effects from remote observation or proximate observation on any of the tasks or dependent measures. The hypothesis that there would be a social facilitation effect from remote observation was not supported. It is important to consider this in light of the fact that a social facilitation

effect between proximate observation and being alone was also not found. This effect was to be the baseline against which the effect of the new condition, remote observation, would be conceptually calibrated. The lack of the normal social facilitation effect as a baseline means that Experiment 5 did not adequately test for a remote observation effect. As the tasks used did not discriminate between participants' performance when alone and when proximately observed, we cannot claim that they would have been able to detect an effect from remote observation, if there is one. It is commendable that Experiments 3 and 5 included both an alone condition and a proximate observation condition as baseline comparators. If, for example, Experiment 5 had compared remote observation to proximate observation only, we might have concluded that remote observation exerted a similar influence to proximate observation. Conversely, if we had compared remote observation to being alone only, we might be certain that remote observation exerted no greater effect than being alone. The increased complexity in the design and analysis of having three observation conditions was justified by avoiding either of these errors. Future research into remote observation and social facilitation should therefore continue to use both proximate observation and being alone as baseline conditions.

The failure to replicate the social facilitation effect in our experiments might have been due to the tasks we chose. The typing task used in Experiment 5 was designed to capitalise on the facilitatory effect found in the code in Experiments 3 and 4. We thought that the code might have demonstrated a facilitatory effect while the name did not because the code was longer than the name typing task. Hence, an even longer task, typing one's full name and email address, was used in Experiment 5. This longer task, however, did not show any observation effects at all. One suggestion for future research, based on the findings of these experiments, is therefore to continue to use the code typing task, as it was the only task that responded to observation. One drawback of the code, however, was that it was difficult to present as a covert task. Following Markus (1978) we thought that it would be important to create a true alone condition. This involved the participant

being physically alone, undertaking a task that would not elicit evaluation apprehension, and not being remotely observed while alone. This imposed restrictions on the choice of tasks. The tasks had to be presented as if they were not assessed experimental tasks, but nonetheless it had to be possible to record performance speed and accuracy without the experimenter watching or listening. The typing tasks used in Experiments 3 and 4 were selected as they were the only example, to our knowledge, of tasks on which speed and accuracy could be measured, that were covert, and that had previously shown the social facilitation effect (Schmitt et al., 1986), but they might have been non-optimal.

The majority of social facilitation research has used cognitive tasks, but there is also a wealth of previous research into motor tasks (Strauss, 2002). Cognitive tasks, such as solving anagrams, would be very difficult to present in such a way that participants would not realise that their performance was being assessed, monitored, or recorded. Motor tasks, on the other hand, might be easier to present as being an incidental part of the experimental set-up. Based on a review of previous research findings, Strauss found that certain types of motor tasks are fairly reliably facilitated by observers. Motor tasks that require mostly strength or power, rather than co-ordination, such as running (e.g., Worringham & Messick, 1983) are facilitated by proximate observation. One possible task for future research could be to ask participants to lower blinds over a window, ostensibly to dim the lights prior to a visual perception task, and have the blinds connected to a device that timed their speed.

One cultural change that might make a difference to people's reactions to remote observation is the recent rise in technology that makes covert observation commonplace. We are under constant covert surveillance; video footage is taken of streets, shops, and busses. People also choose to communicate via web cams and surround themselves with a virtual community via email, blogs, and Facebook. This means that research into whether people are affected by CCTV and other remote viewing

technologies has ecological validity (Baker, 2005). However, if people are constantly exposed to arousal-increasing stimuli the arousal response reduces over time, a process called extinction (Pavlov, 1960). It is possible that in modern times people would have a lesser arousal response to remote observation than in the early days of remote observation detection experiments. Thus, it might be more difficult to detect an effect from remote observation that relies on arousal, and hence a social facilitation effect from remote observation (if that is mediated by arousal).

One pattern that occurred over all three experiments was that there was no significant difference between the observation conditions, when assessed by the planned analyses. This could have been because observation did not exert enough of an effect, which could have been due to aspects of the observer. One limitation of these three experiments is that they used the same observer. Many of the social facilitation studies have two observers, one male, one female, to balance any gender effects, (e.g., Dickerson, Mycek, & Zaldivar, 2008; Griffin & Kent, 1998), or used the same gender observer as the participant, (e.g., Huguet et al. 1999). Although there were no gender effects in our experiment, gender effects frequently occur in psychology, so gender-balancing observers is a wise precaution. Another limitation of using the same observer throughout is that her personality might not have been optimal for an observer. The effect of the observer's personality on the social facilitation effect has not been researched (Guerin, 1993). It is possible that the observers' characteristics play a role in how great an observation influence they exert. Very little investigation has been carried out into the characteristics of influential remote observers. However, some research suggests that remote observation from an observer who believes in psi might have a greater effect than remote observation from an observer who does not believe in psi (Wiseman & Schlitz, 1997; Wiseman & Schlitz, 1998). The observer in Experiments 3 – 5 did believe in psi, however, so that is not the reason for the null results here. One suggestion for future research is to investigate the characteristics of observers that affect whether they exert an effect on participants' performance; then optimal observers could be selected for future experiments.

One aspect that might affect observers' social facilitation influence is whether they calm or arouse the participants. In the parapsychology literature there were psychophysiological arousing effects found from remote observation (e.g., W. G. Braud et al. 1993a (untrained participants); Schlitz & LaBerge, 1997; Wiseman & Schlitz, 1997 (Schlitz's trials)). There were also sometimes calming effects found from remote observation (e.g., W. G. Braud et al., 1993a (trained participants); W. G. Braud et al. 1993b, Wiseman and Schlitz, 1998 (Schlitz's trials)). This calming effect could be called remote affiliation. A tentative explanation for the remote calming effect was the amount of experience and comfort that the observer had with remote observation (W. G. Braud et al., 1993a). As the social facilitation effect is thought to be mediated by arousal (Moore & Baron, 1983), too much affiliation might, in theory, reduce the chances of finding the social facilitation effect. So an optimal hidden observer in a social facilitation experiment should increase participants' arousal. Therefore, in the experiments reported in this thesis, the observer attempted not to be too friendly with the participants, nor to put them at ease. On the other hand, manipulations to increase the arousal inducing effect of the observer were avoided. This was because social manipulations, such as behaving in an intimidating manner, could have only been applied in the proximate observation condition. To maintain comparability of the remote observation and proximate observation conditions, the difference between these was restricted to only the presence or absence of the observer and any consequences, such as the participants knowing that they were observed. However, the observer's familiarity with observation increased over the sequence of experiments. By Experiment 5 she was used to observing, and this was the experiment without even a slight observation effect, even though the proximately observed participants reported awareness of being observed. Research into the calming or arousing aspects of observers is therefore recommended for understanding both remote and proximate observation effects.

One final difference between the remote observation influence used in Experiments 3, 4, and 5 and that used in most previous social facilitation and remote observation

research, is that our participants were not informed that they would be remotely observed. It is possible that not informing the participants may have reduced the magnitude of the remote observation influence. Telling people that they are being observed can mimic the effect of observation on behaviour (J. L. Cohen & Davis, 1973). J. L. Cohen and Davis presented participants with a one-way mirror through which they were told that people were watching them. There was, in reality, no-one watching. Participants practiced solving lexical problems which involved following a pattern. For example, finding the word *goat* in GZOQART follows the pattern of reading alternate letters. This trained pattern was deemed the dominant response, and finding the word *art* in GZOQART was deemed the non-dominant response. This deception of observation condition significantly affected participants' performance on the lexical problem task: Participants responded with more dominant responses when they thought that they were being observed. Likewise, Ganzer (1968) reported a social facilitation effect using a deception of observation condition. Participants were either misinformed that they were being observed through a one-way mirror, or not told that they were being observed. Ganzer used a deception of observation condition instead of a genuine observation condition because, in pilot tests, misinforming participants that they were being watched had exerted the same social facilitation effect as actually being watched through a one-way mirror.

Telling participants that they are being monitored can also enhance the social facilitation effect. For example, Aiello and Svec (1993) compared participants' performance on a complex anagram solving task. All the participants had been shown the computer monitoring system with which their performance was observed by someone watching their progress onscreen from a remote location. The participants who were told that they would be monitored made the most errors, participants who had been shown the monitoring system, but neither told that they would or would not be monitored made fewer errors. The fewest errors were made by participants who were told that they would not be monitored. So informing people

that they are being observed affects their behaviour in a similar way to genuinely observing them.

In most previous remote observation research participants were informed that they would be watched remotely, but not when they would be watched (e.g. Williams, 1983, W. G. Braud et al. 1993a,b). One study directly compared observee's physiological responses to remote observation with and without telling participants that they would be watched (Lobach & Bierman, 2004). Participants' electrodermal activity was measured while they were secretly remotely observed at random intervals by a hidden observer. Participants were then informed that they would subsequently be observed, and their electrodermal activity was measured throughout further remote observation and non-observation trials. There was no significant difference in their electrodermal activity between the observation and non-observation trials, irrespective of whether the participants knew that they might be observed. The findings from this experiment were therefore inconclusive. It is therefore uncertain whether informing people they might be remotely observed affects them any differently from just remotely observing them. Future research could compare psychophysiological and behavioural responses to remote observation simultaneously, with and without informing participants that they are being watched.

To increase the ecological validity of research into behavioural responses to remote observation, future research could investigate behaviours that have been reported in spontaneous remote observation detection experiences. One such behaviour is turning to look at the observer (Sheldrake, 2003). Sheldrake and a group of other observers, hidden behind a one-way mirror, observed an audience queuing for a show at random intervals interspersed with non-observation intervals. A video recording of the audience, rated by a judge who was blind to the observation manipulations, revealed that more people had turned to look at the hidden observers when they were observing than when they were not. This study was not conducted under controlled conditions, and has some limitations, for example, details pertaining

to the randomisation methods, and exactly how many people turned round, were not reported. However, it is the only previous example of observation that people were not informed of affecting their behaviour. Sheldrake suggested, based on many reported spontaneous experiences, that people regularly experience looking in exactly the direction of a hidden observer. One suggestion for future research involves using modern head-mounted eye-tracking devices. These record the direction that people look in. Participants could be involved in another task, justifying the use of the eye-tracker, and be subject to remote observation at random intervals. Their eye movements could then be compared with the direction that the observer was watching them from.

In summary, Experiments 3, 4, and 5 did not find a psi-mediated social facilitation effect from remote observation. These experiments also did not find the normal (non-psi) social facilitation effect. We have interpreted the null findings to mean that the remote observation influence was not fully tested by these experiments. However, Experiment 3, in which post-hoc tests indicated that the speed of typing the code was facilitated by proximate observation, but not remote observation, indicated that remote observation has a minimal effect on task performance. Therefore, it might be reasonable for covert observation to be used in psychology testing as an equivalent to an alone condition, but further testing with paradigms that find the normal social facilitation effect are required to uphold this claim. The hidden observers used in Experiments 3, 4, and 5 were different from the remote observers that participants were informed about in previous remote observation research. Future research closely examining the effect of informing participants about remote observers and testing behavioural, psychophysiological, and conscious guess measures in tandem would ascertain whether social facilitation effects are a viable way to investigate the effects of remote observation.

10. Final Summary, Discussion, and Conclusions

The aim of this thesis was to investigate ways that human behaviour nonintentionally responds to hidden psi information or influences. These psi influences were hidden targets or hidden observers. Not only were these hidden psi influences beyond the participants' sensory reach, the participants did not know that they existed. We investigated whether participants' behaviour would respond as if they knew about the hidden information or influences. This was based upon models of psi function that propose that psi is an unconscious process, but that psi information can come to consciousness (Tyrell, 1947). This suggested that the consciously known psi information was the 'tip of the iceberg'. Therefore, measures that respond to both conscious and unconscious information, such as behaviour, would be very suitable for testing psi. We also based this research on theories, including the PMIR model, that suggest that psi operates nonintentionally, and that the function of psi is to alter behaviour in line with needs or dispositions, or to aid survival (Broughton, 1988, 2006a,b; Eisenbud, 1966-67; Stanford, 1974, 1977, 1982, 1990). These theories proposed that psi information is able to influence everyone all of the time, nonintentionally, and often without awareness. Thus, the nonintentional psi paradigm used in these experiments attempted to simulate how psi might function naturally and spontaneously in real life. We achieved this experimentally by setting up situations in which certain predicted effects from hidden psi information or influences on participants' behavioural responses in psychological tasks could be measured.

We used two main paradigms to investigate nonintentional behavioural responses to psi. In Experiments 1 and 2, we investigated the effects of hidden targets on participants' performance on psychological tasks. These experiments also explored the effects of a variety of personality characteristics and other individual differences on psi performance (extraversion, neuroticism, openness to experience, state anxiety and belief in psi). In Experiments 3 and 5, we investigated the effect of hidden observers on participants' task performance. In this, we compared the effect of

remote observation from hidden observers, proximate observation from physically present observers, and participants being alone and unobserved. In Chapters 5 and 9 both of these different paradigms were subjected to thorough discussion, and in this chapter we now focus on the common elements of both paradigms. Firstly, we relate our findings to the original research question of whether behaviour nonintentionally responds to psi. Secondly, we compare our outcomes to the wider literature considering nonintentional behavioural responses to psi. We conclude with the key findings from the studies in this thesis and their contributions to parapsychology.

Overall, the empirical findings in this thesis did not provide support for the psi hypothesis. However, in Experiment 1 there was an indication of a psi effect in a replication of a response bias effect (the tendency for participants' less frequently chosen responses in a task to manifest a higher proportion of psi hits). In Experiment 1 there was also a significant correlation between participants' extraversion scores and hit rates. Both of these findings will be considered in more detail below. In the general discussions in Chapters 5 and 9, we considered whether the predominantly null results might have been due to our use of unselected participants with, perhaps, too little motivation or experience in psi testing. In Experiments 3 – 5, the null effect of remote observation on behaviour was interpreted in light of the null effect of proximate observation on behaviour. We argued, therefore, that these studies might not have provided a chance for remote observation to manifest a behavioural effect. The results of Experiment 3, however, indicated that if there is a social facilitation effect from remote observation, it is likely to be very weak. This is a relevant finding, given the high prevalence of covert surveillance in general use in modern times (Sheldrake, 2001), but it is a tentative finding that requires replication and confirmation. On the whole, we did not find support for the hypothesis that human behaviour nonintentionally responds to psi.

This thesis therefore did not support the hypothesis that behaviour nonintentionally responds to psi. As reviewed in Chapter 2, there is previous experimental support for

nonintentional behavioural responses to psi, and for many of the hypotheses in the PMIR model. However, some of the previous studies were unable to reject the null hypothesis that there is no psi effect (e.g., Stanford & Castello, 1977). Thus, there is evidence for and against the psi hypothesis. The studies in this thesis add to this body of research findings that do not support the psi hypothesis overall.

However, some previous research into quite different nonintentional psi paradigms has yielded positive results, and we consider those here in relation to our findings and note any differences that might explain our null overall effects. For example, a series of experiments investigated the effects of one person's intention on remote human or animal behaviour (W. G. Braud & Schlitz, 1991). In all of these experiments the *influencee* (the organism who was influenced) was physically distant and sensorially shielded from the sender. The sender attempted to alter aspects of the influencee's behaviour through intention, attention, or imagery. Some of their experiments used an automatic behavioural response, involuntary movements measured by the swing of a pendulum held in the hand, as the dependent variable. In other experiments they investigated the effect of remote influence on muscle tremor; the influencee aimed to keep his or her hand as steady as possible, while holding a device that recorded any tremors. There were also experiments on animal behaviour; senders aimed to alter the orientation and location of fish swimming freely in a tank, or the speed of a gerbil running in a wheel. The position of the fish and the speed of the gerbil's wheel were automatically recorded without the need for human observation or presence. Three of the behavioural measures demonstrated a significant psi effect: the involuntary movement, the swimming fish, and the running gerbil. Muscle tremor did not show a significant effect. W. G. Braud and Schlitz noted that muscle tremor was the behaviour that was most amenable to conscious control.

It is possible that more automatic, unconscious behaviours are better at capturing psi effects. If so, the tasks used in the experiments in this thesis might have been too

much under the participants' conscious or voluntary control. This suggestion is corroborated by the relative success of nonintentional psi tasks that used affective judgement tasks (e.g., Luke, Roe et al. 2008; Luke, Delanoy et al., 2008), which respond to unconscious information (Kunst-Wilson & Zajonc, 1980; Merikle & Daneman, 1998). However, null results were also found in similar affective judgement tasks (Watt & Nagtegaal, 2000; Watt & Ravenscroft, 2000), so it is by no means certain that automatic behaviours would capture psi effects better; nevertheless, it might be a fruitful avenue for future exploration. To assess whether behaviour is automatic, four aspects should be considered. These are whether one acts: intentionally, with awareness, with voluntary control, and efficiently, in terms of mental resources (Bargh, 1994). According to those criteria none of the tasks used in Experiments 1 – 5 were fully automatic; although, according to Kihlstrom's (1987) definition of unconscious, the simple task of typing one's name would be. It is possible that future experimentation would benefit from considering the automaticity of tasks for psi testing. Tasks that have become automatic are also likely to have correct dominant responses, and therefore count as simple tasks in social facilitation terminology (Zajonc, 1980). Therefore, automatic tasks show promise for future nonintentional psi tasks in general, and those that use social facilitation as the behavioural response.

In the theories of nonintentional psi explained above, one key aspect was that psi led to behaviour that served one's needs or was in line with one's dispositions (e.g., Stanford, 1990). We considered in Chapter 4 whether adequate motivation was present in Experiments 1 and 2 to induce a need for participants to score in line with their hidden targets. In Experiments 3 - 5, no motivational incentive was created, but instead participants were expected to respond to the psi influence (remote observation) as they would to the non-psi influence (proximate observation). This expectation was in line with Stanford's hypotheses: firstly that people would respond to psi-mediated information about situations if they would respond to the same situations given sensory knowledge of them, and secondly, that, through psi, people

would experience changes in arousal in response to extrasensory situations. Thus, we argued that if there was a social facilitation effect from proximate observation, there would also be a social facilitation effect from remote observation. As noted above, these hypotheses were not supported.

It is therefore worth considering whether psi possibly serves “stronger” needs. Broughton’s evolutionary theory of psi stated that the function of psi is to bias behaviour to aid survival. Whilst it would be unethical to put human participants in life-threatening situations for psychology experiments, some research with animals has investigated just this. Morris (1977) reviewed evidence considering whether fish behave differently immediately prior to being caught in a net and taken out of the water than fish that were not about to be caught. There was evidence that the fish randomly selected to be removed were more active just prior to being caught. Increased activity might convey an advantage if survival depended upon escape. However, Morris noted that much of this evidence was from pilot studies, which were not optimally controlled. The question of whether psi serves needs, and, if so, which needs, is still open to debate.

We now consider the key findings from this thesis, which were both from Experiment 1. In Experiment 1 there was a replication of a response bias effect. This is the tendency for higher psi hit rates in participants’ less frequently chosen answers. The response bias effect has been found in both intentional psi tasks (Stanford, 1967, 1973) and nonintentional psi tasks (Kreitler & Kreitler, 1972; Stanford, 1970; Wilson, 2002a, 2004). In most of the previous research, the less frequently and more frequently chosen answers were ascertained at group level, and may therefore not have applied equally to all of the participants. In Experiment 1, we extended the response bias effect research by considering preferences at the individual level. Our replication of the response bias effect adds to the claim that there is a psi effect relating to how often a target is chosen. This sheds light on the kind of situations in which psi might have an influence. Psi might be most effective in situations where

habit or non-psi information points the other way. In other words, a psi effect might manifest most in participants' non-dominant responses. This might conflict with the expected social facilitation effect, that dominant responses are increased by observation or presence (Zajonc, 1965). If, for example, a sender could elicit a social facilitation effect, that would increase dominant responses. If psi manifests in the less dominant responses, it is debatable whether a sender would increase or decrease a psi effect overall. Also, Stanford and Stio (1976) found support for their hypothesis that psi targets would be more effective in facilitating than inhibiting participants' dominant responses. Thus, these social and psi influences might combine in many ways, and further investigation of this is a potential avenue for future research.

In Experiment 1 there was also a significant positive correlation between psi hit rate and extraversion. This is a key finding for two reasons. Firstly, it is, to our knowledge, the first investigation of the role of extraversion in a nonintentional psi task. Secondly, the relationship between psi and extraversion in forced-choice tests has been debated. It is still a controversial area owing to a potential artefact Honorton et al. (1998) mooted, of measuring extraversion after participants know their psi scores (Honorton et al., 1998; Krishna & Rao, 1991; Palmer & Carpenter, 1998). Experiment 1 provided support for this relationship without any risk of this artefact.

To conclude, this thesis investigated nonintentional psi with novel experimental tasks. If psi is predominantly a nonintentional process, then nonintentional psi experiments are more relevant than the traditional intentional psi tasks. Nonintentional psi is also comparatively under-researched compared to conscious guess and psychophysiological paradigms in parapsychology. The experiments reported in this thesis contributed an investigation of a novel nonintentional psi task: line-length judgements, and a conceptual replication of a nonintentional effect on timing. The effect of personality variables on psi performance is an important area to investigate to understand how psi might work, and it is also under-researched in nonintentional psi-testing paradigms. These experiments also contributed an

investigation of nonintentional psi and several individual differences and personality variables.

This thesis also asked the original question: Is there a social facilitation effect from remote observation? The investigation of this extended both the fields of social facilitation and remote observation detection. There are very few studies that have investigated a behavioural response to remote observation, and this is the first to consider the effect of normal (proximate) observation in comparison to the effect of remote (psi-mediated) observation. One previous remote observation experiment used a performance task as a dependent variable (Lee et al., 2002), but did not consider the social facilitation effect. This thesis, therefore, represents a valuable conceptual development in the field of remote observation research. There have been many studies investigating social facilitation that used remote observation in the alone condition (e.g., Markus, 1978). This was on the grounds that remote observation would have no effect on the participants' task performance, an assumption unchallenged within the field of social facilitation. Our experiments are the first thorough investigation, with tried and tested performance tasks, of whether remote observation is measurably different from being alone. The prediction was that the social facilitation effect would be replicated, and that remote observation would exert a similar effect to proximate observation. The results did not support these predictions, but represent the first step in investigating this new paradigm. These experiments presented in the thesis provide a baseline for future research to build on. Therefore, this thesis contributed theoretical and experimental developments to the parapsychological and psychological literature. The question of whether psi can nonintentionally affect human behaviour is still open, and worthy of future investigation.

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12. Appendices

12.1. Appendix 1: The Koestler Parapsychology Unit Belief in Psi Questionnaire

Participant code.....

Date.....

Please use the following definitions for the purpose of answering these questions

PSI: Direct interactions between mental processes and the physical world or other mental processes occurring outside currently understood channels. Thus this is a 'blanket' term used to refer to all paranormal processes and causation.
PSI is commonly divided into two categories:

1. EXTRASENSORY PERCEPTION (ESP): Reception of information without the use of known senses or logical inference.

ESP is for convenience further subdivided into three categories:

TELEPATHY: ESP of the thoughts, feelings or behaviour of another person or organism.

CLAIRVOYANCE: ESP of distant physical events or concealed objects.

PRECOGNITION: ESP of the future.

2. PSYCHOKINESIS (PK): Mental influence on the physical world.

1. What best describes your own psi ability? (please tick one box)
 I have psi ability Uncertain I have no psi ability
2. Is the existence of ESP? (please tick one box)
 Certain Uncertain Impossible
3. Have you ever had an experience which is best explained by telepathy? (please tick one box)
 Yes Uncertain No
4. Have you ever heard or read of an experience which is best explained by telepathy? (please tick one box)
 Yes Uncertain No
5. Have you ever had an experience which is best explained by clairvoyance? (please tick one box)
 Yes Uncertain No
6. Have you ever heard or read about an experience which is best explained by clairvoyance? (please tick one box)
 Yes Uncertain No
7. Have you ever had an experience which is best explained by precognition? (please tick one box)
 Yes Uncertain No

8. Have you ever heard or read about an experience which is best explained by precognition?
(please tick one box)

Yes Uncertain No

9. Is the existence of psychokinesis: (please tick one box)

Certain Uncertain Impossible

10. Have you ever had an experience which is best explained by psychokinesis? (please tick one box)

Yes Uncertain No

11. Have you ever heard or read about an event which is best explained by psychokinesis? (please tick one box)

Yes Uncertain No

12. Do you believe that you might be able to demonstrate any psi ability in a controlled laboratory experiment? (please tick one box)

Yes Uncertain No

12.2. Appendix 2: The Maths Task

START THE STOPWATCH

- 1) $12 \times 1 =$ _____
- 2) $4 \times 9 =$ _____
- 3) $7 \times 7 =$ _____
- 4) $1 \times 1 =$ _____
- 5) $6 \times 4 =$ _____
- 6) $3 \times 3 =$ _____
- 7) $3 \times 8 =$ _____
- 8) $0 \times 11 =$ _____
- 9) $6 \times 1 =$ _____
- 10) $5 \times 12 =$ _____
- 11) $10 \times 8 =$ _____
- 12) $9 \times 6 =$ _____
- 13) $12 \times 9 =$ _____
- 14) $8 \times 5 =$ _____
- 15) $3 \times 9 =$ _____
- 16) $8 \times 6 =$ _____
- 17) $11 \times 12 =$ _____
- 18) $6 \times 5 =$ _____
- 19) $12 \times 2 =$ _____
- 20) $7 \times 6 =$ _____
- 21) $5 \times 5 =$ _____
- 22) $1 \times 8 =$ _____
- 23) $8 \times 4 =$ _____
- 24) $11 \times 6 =$ _____
- 25) $7 \times 8 =$ _____

- 26) $12 \times 12 =$ _____
- 27) $5 \times 9 =$ _____
- 28) $0 \times 10 =$ _____
- 29) $8 \times 11 =$ _____
- 30) $9 \times 9 =$ _____
- 31) $10 \times 2 =$ _____
- 32) $4 \times 10 =$ _____
- 33) $6 \times 6 =$ _____
- 34) $8 \times 11 =$ _____
- 35) $9 \times 7 =$ _____
- 36) $11 \times 5 =$ _____
- 37) $1 \times 11 =$ _____
- 38) $3 \times 7 =$ _____
- 39) $8 \times 8 =$ _____
- 40) $1 \times 9 =$ _____
- 41) $11 \times 11 =$ _____
- 42) $9 \times 2 =$ _____
- 43) $10 \times 7 =$ _____
- 44) $4 \times 2 =$ _____
- 45) $5 \times 7 =$ _____
- 46) $12 \times 4 =$ _____
- 47) $5 \times 2 =$ _____
- 48) $7 \times 12 =$ _____
- 49) $3 \times 2 =$ _____
- 50) $11 \times 3 =$ _____

STOP THE STOPWATCH

TIME (minutes, seconds and fractions of seconds): _____

YOUR PARTICIPANT CODE NUMBER: _____

12.3. Appendix 3: Personality Questionnaires from Experiments 2 and 5

LimeSurvey

<http://capisci.org.uk/limesUNI/admin/admin.php?action=showprinta...>

x3_personalityQs

section1

This questionnaire is about aspects of your personality. You will rate statements describing behaviours and feelings for how well they describe you. There are no right and wrong answers. Please respond to each statement as truthfully as you can for how you usually think, feel and behave, except for the final section which is about how you are feeling now (there will be a reminder at the time). Please read each statement carefully before choosing one answer. There are five-point answer scales for all but the final section, for which there is a four-point scale. The information is held anonymously and the results will never identify you or your answers.

* 101: Please enter your participant code.

Please write your answer here:

* subsection1: Please rate how much you agree or disagree with the following statements about how you USUALLY are.

Please choose the appropriate response for each item:

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I feel comfortable around people.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am very pleased with myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I believe in the importance of art.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I adapt easily to new situations.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am easily intimidated.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I seldom joke around.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I love large parties.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I make friends easily.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not easily bothered by things.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I have a vivid imagination.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I don't worry about things that have already happened.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am afraid that I will do the wrong thing.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not easily amused.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I talk to a lot of different people at parties.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** subsection2: Please rate how much you agree or disagree with the following statements about how you USUALLY are.**

Please choose the appropriate response for each item:

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I am skilled in handling social situations.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I feel comfortable with myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I tend to vote for liberal political candidates.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not easily disturbed by events.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I find it difficult to approach others.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I amuse my friends.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I enjoy being part of a group.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am the life of the party.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I seldom feel blue.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I carry the conversation to a higher level.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am relaxed most of the time.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am afraid to draw attention to myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I laugh aloud.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I involve others in what I am doing.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** subsection3: Please rate how much you agree or disagree with the following statements about how you USUALLY are.**

Please choose the appropriate response for each item:

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I know how to captivate people.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I rarely get irritated.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I enjoy hearing new ideas.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I get caught up in my problems.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I only feel comfortable with friends.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

I look on the bright side of life.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I love surprise parties.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I have little to say.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I panic easily.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not interested in abstract ideas.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I get stressed out easily.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I stumble over my words.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I love life.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I prefer to be alone.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I keep in the background.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** subsection4: Please rate how much you agree or disagree with the following statements about how you USUALLY are.**

Please choose the appropriate response for each item:

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I have frequent mood swings.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I do not like art.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am afraid of many things.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not embarrassed easily.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I laugh my way through life.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I want to be left alone.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I would describe my experiences as somewhat dull.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am often down in the dumps.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I avoid philosophical discussions.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I fear for the worst.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am comfortable in unfamiliar situations.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I express childlike joy.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I don't like crowded events.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I don't like to draw attention to myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** subsection5: Please rate how much you agree or disagree with the following statements about how you USUALLY are.**

Please choose the appropriate response for each item:

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I dislike myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I do not enjoy going to art museums.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I worry about things.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am not bothered by difficult social situations.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I have a lot of fun.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I avoid crowds.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I don't talk a lot.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I often feel blue.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I tend to vote for conservative political candidates.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am able to stand up for myself.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I radiate joy.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I seek quiet.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** subsection6: Please note that there is a different answer scale and time that these are referring to. Please rate these statements for how you are feeling RIGHT NOW.**

Please choose the appropriate response for each item:

	Not at all	Somewhat	Moderately	Very much
I feel calm.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am tense.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I feel upset.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am relaxed.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I feel content.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I am worried.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Submit Your Survey.
Thank you for completing this survey..

12.4. Appendix 4: Post-experimental Questionnaire from Experiments 2 and 5

PDF export

post exp questions

section1

*** 111: Were you aware of being observed (looked at by a person) during the experiment?**

Please choose *only one* of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '111 ']

112: If you answered yes to the previous question:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How much were you aware of being observed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 121: At the time of typing your name and email did you suspect that it might be an experimental task?**

Please choose *only one* of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '121 ']

122: If you answered yes to the previous question:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How much did you suspect that the name and code might be an experimental task?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 131: Were you aware of being monitored (your actions recorded by a machine of some sort) when you typed in your name and email address?**

Please choose *only one* of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '131 ']

132: If you answered yes to the previous question:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How much were you aware of being monitored while doing the name and code?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 141: Were you aware of being monitored (your actions recorded by a machine of some sort) when you did the Stroop colour task?**

Please choose **only one** of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '141']

142: If you answered yes to the previous question:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How much were you aware of being monitored during the Stroop colour task?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 151: Were you aware of being monitored (your actions recorded by a machine of some sort) when you did the maths task?**

Please choose **only one** of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '151']

152: If you answered yes to the previous question:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How much were you aware of being monitored when you did the maths task?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 161: Did you win chocolate for the maths task?**

Please choose **only one** of the following:

- Yes
 No

[Only answer this question if you answered 'Yes' to question '161 ']

162: If you won chocolate:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How happy were you to win chocolate?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

[Only answer this question if you answered 'No' to question '161 ']

163: If you did not win chocolate:

Please choose the appropriate response for each item:

	Very little	2	3	Midpoint	5	6	Very much
How disappointed were you NOT to win chocolate?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 211: Please rate the difficulty of:**

Please choose the appropriate response for each item:

	Extremely easy	2	3	Midpoint	5	6	Extremely difficult
Typing your name	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Typing your email address	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The personality questionnaires	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour of strings (+++)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour when the word was written in the colour it said	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour when the word was written in a different colour	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The maths task	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 221: Please rate your familiarity with:**

Please choose the appropriate response for each item:

	Never done before	2	3	Midpoint	5	6	Do it all the time
Typing your name	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Typing your email address	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The personality questionnaires	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour of strings (+++)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour when the word was written in the colour it said	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Selecting the colour when the word was written in a different colour	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The maths task	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 311:**

Please use these definitions to answer the following questions:

Psi = Direct interactions between mental processes and the physical world, or other mental processes occurring outside currently understood channels.**This is a 'blanket' term used to refer to all paranormal processes and causation.****Psi is commonly divided into two categories:****1 --Psychokinesis (PK) = mental influence on the physical world.****2 - Extra Sensory Perception (ESP) = the reception of information without the use of known senses, or logical inference.****ESP is further subdivided into three categories:****a) Telepathy = ESP of the thoughts, feelings or behaviour of another person or organism.****b) Clairvoyance = ESP of distant physical events or concealed objects.****c) Precognition = ESP of the future.**

Please choose the appropriate response for each item:

	No	2	3	Uncertain	5	6	Yes
Do you have psi ability?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Does ESP exist/happen?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever had an experience which is best explained by telepathy?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever heard of, or read about, an experience which is best explained by telepathy?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever had an experience which is best explained by clairvoyance?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever heard of, or read about, an experience which is best explained by clairvoyance?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever had an experience which is best explained by precognition?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever heard of, or read about, an experience which is best explained by precognition?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Does psychokinesis exist/happen?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever had an experience which is best explained by psychokinesis?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Have you ever heard of, or read about, an experience which is best explained by psychokinesis?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Do you believe that you might be able to demonstrate any psi in a controlled laboratory experiment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

*** 101: Please enter your participant code.**

Please write your answer here:

Submit your survey.
Thank you for completing this survey..

12.5. Appendix 5: Stroop Practice items

RED

GREEN

BLUE

YELLOW

WHITE

++++

+++++

++++

RED

GREEN

BLUE

YELLOW

WHITE

RED

GREEN

BLUE