CHAPTER 7
ATTITUDINAL AND DISPOSITIONAL EFFECTS ON GOAL ORIENTATION DURING A GAMBLING TASK

Don't ever forget, winning is about attitude.
Bruno Casciato (2001, p. 1)

I cannot believe that God would choose to play dice with the universe.
Albert Einstein (letter to Max Born, 1926, cited in Einstein, Born, & Born, 1969, p. 130)

7.1 Attitudes and Dispositions

In the present chapter, the tempering effects of three attitudes/dispositions[60] on pro attitudes in a forced-choice gambling task are investigated. These are (i) attitude towards various types of gambling, (ii) belief in good luck, and (iii) intuition as a personality variable. It is hypothesized that pro attitudes are modified by these attitudes/dispositions, such that the positive aspects of the above-mentioned attitudes/dispositions are conducive to paranormal effects.

In this chapter, evidence is also sought for the presence of *compliant* and *noncompliant* pro attitudes, and an attempt is made to reach a conclusion as to whether evidence for compliance (and noncompliance) can actually be used to inform us about the nature of the pro attitude. Thus, the present study is another (but alternative) test of the concept of the pro attitude, being an investigation into the effects that individual differences may have on the outcomes of paranormal tasks. Before reviewing the literature on these above-mentioned attitudes/dispositions, the nature of compliance in the experimental situation needs consideration.

[60] There is some conceptual overlap in the meanings of the nouns 'attitude' and 'disposition'. Attitude is taken to mean a particular 'position' or 'standpoint' (hence, *attitude* towards gambling), while 'disposition' refers to 'tendency' or 'inclination' (hence, a *disposition* to believe in good luck). Both can be long-standing characteristics of the individual (i.e., they may be *pre*-dispositional), and many a state or trait could be described unequivocally as both an attitude and a disposition.

7.1.1 The Pro Attitude and the Problem of Compliance

In the experimental situation, if the participant adopts a pro attitude, then it is usually understood that they have a goal state or target in mind at the time of instruction by the experimenter. There exists, therefore, on the part of the experimenter, an underlying factor or condition that is concomitant with the participant's putatively adopted pro attitude, and it is the experimenter's assumption that participants willingly comply with the requirements of said experimenter's instructions in accordance with the requirements of the experimental task.

In Chapter 5 (see *5.1.2*), the following statements were presented: "*Given sufficient other conditions, if there is significant hitting on first hexagrams, then a sufficient number of pro attitudes were present; therefore, if there is no significant hitting on second hexagrams, then a sufficient number of pro attitudes were not present*" (p. 77). For first hexagram hitting, it is tacit that a sufficient number of compliant pro attitudes must have been present, given the significant hit-rate. The second statement, though, should not lead us to think that noncompliant pro attitudes were present. A sufficient number of participants might simply have been ignorant of the possibility of targeting second hexagrams, which of course was the whole point of the exercise in Chapter 5—to show that pro attitudes must be present for an effect to take place. But consider the following cases:

1. *First-hexagram-hitters who were also second-hexagram-hitters* (see *5.7.5*, Hypothesis 2). Eleven percent of first-hexagram-hitters (3 of 27 participants) had used the *I Ching* prior to the experiment so would be well aware of the advantage of an extra reading that comes with generating a second hexagram. Any or all three of these participants may have used psi, not only to generate *at least* one changing line in order to get a second hexagram, but might actually have (quite needlessly) adopted noncompliant pro attitudes for a match on second hexagrams simply because they were focused on the idea of getting a second hexagram. Of course this is all conjecture since participants were not asked to target second hexagrams, and this 'lack' of instruction may explain the rather low nonsignificant hit-rate of 22%.

2. *First-hexagram-missers who were second-hexagram-hitters* (see *5.7.5*, Hypothesis 3). Of the 52 first-hexagram-missers, 7 (13.5%) had used the *I Ching* prior to the experiment, and may have wanted second hexagrams. However, given that 73% of this subgroup were believers (i.e., they answered 'yes' to one or both belief questions), it is even more unlikely that a sufficent number of skeptics, if any,

could have used psi to avoid the first hexagram and target only second hexagrams, but the hit-rate for second hexagrams was fairly high (29%), albeit nonsignificant. Some degree of noncompliance cannot be ruled out.

Such cases where second-hexagram-hitting was evident, bearing in mind the chance outcomes, *might* still be clear violations of the task requirement, bringing into consideration the problem of noncompliance. This problem has repercussions for the concept of the pro attitude, and is dealt with next.

Whenever support for the concept of the pro attitude is found (in the form of a significant result), the assumption can always be made that a sufficient number of participants have complied with the experimenter's instructions, and that sufficient numbers of participants must therefore have adopted the appropriate pro attitude. However, the experimenter must be alert to the possibility of nonsignificant results *in cases where psi hitting is expected* (not the case for second hexagram hitting), which may prove to be ambiguous in terms of meanings to be drawn in respect of the so-called compliant pro attitude because three conclusions are possible:

(i) Nonsignificant results decidedly cannot say anything about the concept of the pro attitude for the express reason that pro attitudes and compliance do not exist. (This conclusion is counter-intuitive and likely to be invalid because it does not square with the common-sense evidence that individuals in experiments do demonstrate goal-directed behaviours which can be achieved, so that some level of compliance would most likely have been effected.)

(ii) Nonsignificant results may say nothing other than that the compliant pro attitude was not sufficiently present. Notwithstanding the possible lack of certain sufficient conditions, noncompliant pro attitudes might be considered—any number of pro attitudes may have been present during the experiment, some of which may in fact be counterproductive to the experimenter's intentions. (These conclusions can be reached because conclusion (i) cannot always be true.)

(iii) Nonsignificant results are ambiguous and meaningless because the pro attitude hypothesis is unfalsifiable—there can never be evidence that pro attitudes of any kind were *not* present, given the presence of sufficient conditions. Thus, a nonsignificant result is not evidence that the assumption

of sufficient compliance was *not* met because that assumption is also unfalsifiable.

It is possible to eliminate conclusion (i) by philosophical induction. Conclusion (iii) can be *tentatively* ignored since falsifiability is not an experimental problem (only a philosophical one) if evidence for the compliant pro attitude is found. (Note that the existence of such evidence is implied by induction—see conclusion (i).) Thus, conclusion (ii) can be provisionally maintained without threat to the concept of the pro attitude or the compliance assumption. However, since it is nonsignificant results that are under discussion here, the falsification problem raised in conclusion (iii) cannot be completely ignored. I will return to this problem shortly, but conclusion (ii) must first be reconsidered before falsification can be dealt with.

To expand on conclusion (ii), it is noted that the methodology of most psi experiments may only make it possible to consider, but not imply, the presence and form(s) of the noncompliant pro attitude(s), given nonsignificant results. For example, even in the case of psi-missing (a significant effect), which would be evidence of noncompliance, and therefore noncompliant pro attitudes, the form of the noncompliant pro attitude is indeterminate if the effect is also indeterminate in terms of its form—that is, where 'displacement effects' (both in kind and/or 'steps' or 'removes'), 'position effects', or 'serial-position effects', is/are not evident (see Thalbourne, 1982, pp. 21, 54, 71, respectively, for definitions of these effects.) In fact, this problem of 'form' has always existed in paranormal experiments where no specific protocol existed, or statistical test was available, that would help provide evidence of alternative targeting.

Crandell and Hite (1983, p. 209) refer to psi-missing as "improperly focused psi." This term clearly implies the likelihood that noncompliance (or the inability to comply), and therefore noncompliant pro attitudes, can be present during an experiment. In such a situation, noncompliant pro attitudes (i.e., 'improper focus') may default to conditions of avoidance, and/or certain forms of 'retargeting' (as just listed), or even "mix-up" effects (see Timm, 1969). These effects in combination may even be the direct result of (say) 'strategies' (albeit unconscious) of multiple pro attitudes.[61]

[61] The term 'strategy' does not imply any conscious conspiracy or collusion as such, but its presence and influence might result in undetectably weak effects caused by sufficient numbers of individuals who vary in personality and attitude, and therefore may retarget in different ways, which, for example, would be typical of skeptics.

Thus, it appears that a 'complex' of multiple noncompliant pro attitudes would be difficult to identify (especially if produced by subgroups of small numbers of participants), and under these conditions it seems that the experimenter may never be able to show that complexes of noncompliant pro attitudes exist. This is the worst-case scenario, and it is presented to illustrate the difficulties the experimenter may face when investigating the pro attitude and the forms it can take. However, theoretically (and thus returning to the problem of falsifiability), the concept of the pro attitude can still be refutable. Thalbourne (2000a) has indicated the possibility that pro attitudes may be mutually opposed to one another. This 'oppositionalism'[62] can be derived from his statement in which he subsumes not only "motives, desires and goals on a full-blown conscious level, but also their less conscious counterparts" under the umbrella-term 'pro attitude' (Thalbourne, 2000a, p. 65).

To falsify the pro attitude hypothesis, the experimenter would need to find evidence that a pro attitude (in this case, the compliant one) was *not* present by finding evidence that a noncompliant pro attitude *was* present, which would imply the absence of the compliant pro attitude. That is, the experimenter finds evidence where the pro attitude is clearly not related to the task at hand, as explained by the experimenter, because paranormal effects have been elicited in a counter-task where clear instruction was given to avoid a target. This protocol was not implemented in the *I Ching* experiment where there was the mere *suggestion* only that sufficient numbers of *noncompliant* pro attitudes might *not* have been present. That is, some number of participants who knew about second hexagrams may have had pro attitudes to generate second hexagrams, but there was no statistical evidence to support that assumption. In fact, participants were not given clear instructions to approach *or* avoid second hexagrams so that the conclusion must be that second hexagrams may have been *ignored* by a sufficient number of participants, in the sense that participants were ignorant of the possibility of targeting second hexagrams (should they so desire). The Gambling experiment is an attempt to rectify these faults.

To summarize this subsection:

(i) The assumption of sufficient compliance is argued as being a crucial component of the experiment since the alternative for the experimenter is to assume that effects can take place that are not expected, or not planned.[63]

[62] The oppositional nature of conscious and unconscious pro attitudes will not be discussed here in full—for a detailed discussion of the pro attitude, see *12.2.2*.

[63] There are cases where the experimenter does have a 'plan'. For example, the experimenter aims to demonstrate a sheep-goat effect, although the experiment actually requires the participant to hit, not miss.

(ii) The conclusion that there are compliant (or noncompliant) pro attitudes of a sufficient number of participants in a given experiment is argued as being concomitant with, and dependent on, statistical evidence for compliance (or noncompliance).

(iii) Given all the necessary conditions, and insofar as there is no statistical evidence of noncompliance, it cannot be assumed that an insufficient number of noncompliant pro attitudes were present unless stipulation was made that a specific counter-task was to be avoided by participants.

(iv) Given all the necessary conditions, and insofar as there is statistical evidence of both sufficient compliance and noncompliance, it can be expected that the compliant and noncompliant pro attitudes are incompatible (i.e., mutually opposed).

Like the pro attitude, compliance and noncompliance are dispositions or attitudes, which characterize the nature of a given pro attitude. In the next section, the gambling literature is reviewed, since it is hypothesized that paranormal effects are involved in the gambling process, and that positive attitudes towards gambling are necessary conditions that bring about exo-psychopraxia.

7.2 Gambling

Recent research on gambling has shown that it is significantly related to individual differences in risk taking, liberalism, and previous gambling experience (Kassinove, 1998; Peltzer & Thole, 2000). Kassinove (1998) pointed out that gambling is a "universal phenomenon that occurs in a myriad of forms" (p. 763). For example, in the United States, gambling was a cornerstone of the American West and slipped into the folklore of that nation (Burnham, 1994; Munting, 1996), and it had a similar impact (both positive and negative) in other countries as diverse as Switzerland (Bondolfi, Osiek, & Ferraro, 2000) and Australia (Delfabbro, 1999; Ellicott, 1999). Thus has gambling not only become a profession for many people around the world, but it has also become a social problem of great menace, and is associated with loneliness and isolation (Trevorrow & Moore, 1998),

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Compliance, therefore, is a separate issue from the experimenter effect since it refers to the requirements of the task, not the agenda of the experimenter.

alcohol abuse (Bondolfi, Osiek & Ferrero, 2000; Perrier & Toner, 1984), and psychiatric comorbidity (Blaszczynski & Farrell, 1998; Ibanez et al., 2001).

Scales that measure attitudes towards gambling have been used to indicate a readiness to participate in gambling tasks and to take risks, and even to identify pathological gamblers (for examples, see Kassinove, 1998, and Lesieur & Blume, 1987). Therefore, some research into gambling using gambling scales has been carried out, and the sociological and psychological issues associated with gambling are well recognized (as suggested by the studies mentioned above), but no use whatsoever has been made of gambling scales as predictors of paranormal ability. Radin and Rebman (1998), however, did recognize the potential of 'psi in the casino'. They looked for evidence of anomalous influences of lunar cycles, gravitational tidal forces, and planetary geomagnetic field flux (GMF flux) on paranormal performances on various casino games, including blackjack, roulette, keno, and slot machines ('slots'). Data from a casino in Las Vegas, covering a four-year period, and accumulated environmental data of lunar, solar and Earth's periodic effects for the same period, were analyzed.

Radin and Rebman (1998) found that daily payout percentages significantly correlated with gravitational forces, and nearly significantly correlated with lunar cycle and GMF flux (as influenced by lunar activity). They also found a relationship between jackpot payouts (slots, roulette and keno) and the days of a full moon, but in real terms the increase in payouts was only "about 2%" (p. 216). Radin and Rebman (somewhat negatively?) took this above chance rate to mean that "most players will lose a little slower than usual" (p. 217).

Etzold (2001) claimed that the lunar-periodic anomaly effect had been replicated in data he gathered over a later four-year period. The payout rates on casino games (again for slots, roulette and keno) were higher during the three days that centered around the time of the full moon. No evidence for an influence on psi was found for a solar-periodic effect, although Etzold (2001) used solar activity as a measure of Earth's GMF flux rather than lunar activity, as used by Radin and Rebman (1998). He concluded that "selective perception," "confirmation bias," and "experimenter effect" could all be ruled out as possible causes of these effects.

Only three studies were found that featured psi tasks using gambling techniques. Brier and Tyminski (1970) used a roulette wheel. Participants wrote lists of winning numbers, which were to be matched against the numbers generated from future spins of the roulette wheel. In two out of four series, there was significant matching of numbers. Brier and Tyminski ran a 'confirmation series', using participants' pre-recorded lists of numbers

against throws of dice. Significant results were obtained. In a second confirmation study, playing cards were used, but results were only 'marginally' significant.

Kugel (1990-1991) also used a roulette wheel. In an experiment that tested participants' ostensibly random number decisions (actually unconscious patterned choices) against the random selections of a computer program, bets were made on certain trials according to certain statistical criteria. Participants' hit-rates were compared with those of the program. On two occasions, the program out-performed participants on predicting hits—in the toy roulette wheel condition (14 out of 20 sessions), and in a Berlin casino with a significant hit-rate (169 out of 313 trials).

In a forced-choice guessing task, which was part of a larger study on gambling behaviour, Don, McDonough, and Warren (1998) attempted to elicit 'event-related potentials' in participants that would be greater in "negative-going amplitude" when participants viewed future targets (graphic images), but not when they viewed future non-targets. Differential brain responses indicated that participants, although they did not consciously recognize the future target when first presented, were responding as if they unconsciously 'knew' that it would be presented later as a target. The results were "interpreted as evidence of unconscious or preconscious psi" (p. 127).

None of the above studies used gambling scales. Insofar as a limited number of researchers have used participants in gambling situations to test paranormal ability, it remains to be seen whether 'gamblers' *per se* (i.e., gamblers characterized as such on the basis of scores on gambling scales) are predisposed towards gambling success (or failure), since, as stated, no study yet has sought a relationship between attitude towards gambling and paranormal ability.

There are those gamblers who are professional (they are generally successful and may, for example, be strongly influenced by intuition when they gamble—see below, **7.4**), and there are those who lose habitually—the so-called 'problem gamblers'. Others have merely a social interest in gambling as a form of harmless entertainment. However, anyone may qualify as a gambler, since the only stipulation is that they participate in gambling tasks. Individuals who are supportive of gambling (specifically referring to successful gamblers here) may be so oriented because they have certain dispositions, which encourage a pro attitude towards gambling success when they participate in gambling tasks (for an example, see Rhine, 1967, pp. 166-168). In fact, based on Thalbourne's (2000a, pp. 65-66) statements, it could be argued that a pro attitude towards the goal of success at gambling contributes to actual success at gambling. This pro attitude may be tempered by the attitude of the participant towards gambling. Thus, it is hypothesized that gambling

success is at least partially attributable to the paranormal influence of the gambler. In the present study, positive attitudes towards gambling were investigated as possible necessary conditions for eliciting paranormal effects.

The next section looks at belief in good luck, which may well be concomitant with a positive attitude towards gambling (see *7.5.2*, Hypothesis 12).

7.3 Belief in Good Luck

The concept of luck, in fact the definition of luck, like psi, is rather elusive, and this elusiveness is exacerbated by cross-cultural differences. Darke and Freedman (1997a, pp. 502-503), for example, found that cultural differences were quite apparent in the way that 'Asian-American', 'African-American', 'Latino', and 'White' participants responded to their 12-item Belief in Good Luck Scale (BIGL). In some cultures the word does not exist, but an alternative terminology is used. For example, in Japan the expression 'your destiny is good!' replaces our expression 'you are lucky!' In terms of having a 'good destiny' it is perhaps not surprising that Smith, Harris and Joiner (1996) found that the term 'luck' was associated with events in one's life that worked out well, but were essentially attributable to chance.

Cross-culturally, although there may be some agreement about what it means to be lucky in terms of outcomes, beliefs about luck may differ, even within a culture. Thus, it is again not surprising that Smith, Harris and Joiner (1996) found that people tended to hold one and only one belief about the nature of luck: (i) luck was "an attribute that was either present or absent at birth," or (ii) the "level of luck" could be controlled by "superstitious behaviour," or (iii) "luck was given to them (and taken away from them) by a 'powerful other' " (p. 37). The so-called 'unlucky' person tended to believe (i) or (iii) above, and thought that their bad luck was outside their control, whereas the 'lucky' person tended to believe (ii) above, and thought that they were the 'cause' of their good luck.

Smith, Harris and Joiner (1996) also found indications that the cause of luck could come from (a) *cognitive biases* (i.e., selective memory, where optimists tend to remember the good, i.e., 'lucky', events, and pessimists tend to remember the bad, i.e., 'unlucky', events), (b) *motivational biases* (illusions of control could be set in place such as soft throws of dice to get low numbers, or personally selecting one's own lottery ticket), (c) *implicit learning* (with practice, strategies are learned unconsciously, and these, and not luck, account for improved performance), and (d) *psi* ("individuals might be using psi to create favourable situations" (p. 38)). The focus of the present study is on (d), where it is

hypothesized that outcomes attributed to 'luck' are actually caused by paranormal means (see the hypotheses in *7.5.1*).

Greene (1960) was one of the first to investigate the concept of luck in paranormal research. She used the Greene Luck Questionnaire, which measures perceived luckiness, to determine a relationship between participants' perceptions about their own luck and the success at a PK task involving the throwing of a ten-sided die. No relationship between luckiness and PK success was found. Ratte and Greene's (1960) variation on Greene's (1960) task used throws of a die to determine outcomes in an imaginary basketball game, and this time, scores on the luckiness scale correlated significantly with PK scores. Ratte (1960) replicated a PK-luckiness effect in three of four conditions of a die-throwing task.

Generally, studies on luck are few and far between. Only in the 1990s has there been a renewed interest. Wiseman, Harris and Middleton (1994) administered a 2-trial 4-choice free-response clairvoyance test to participants who rated themselves on luckiness, but the correlation between perceived luckiness and actual ESP performance was not significant. They did find a significant positive correlation between perceived luckiness and actual paranormal performance for those participants who believed the paranormal task to be dependent on non-chance factors. Non-chance factors may include the perception that luck was involved.

Smith, Wiseman, Machin, Harris, and Joiner (1997) rated participants as 'lucky', 'unlucky', or 'uncertain' according to their responses on a Luckiness Questionnaire. Participants were asked to rate in advance their performance on a pseudo-RNG-based coin-flipping task, and then to perform the task. There were only chance differences between 'lucky' and 'unlucky' participants on psi scores, and ratings of predicted psi performance. However, a significant positive correlation was found between predicted psi performance and actual psi performance.

Darke and Freedman (1997a, 1997b) were also among the first to revive the interest in the phenomenon of luck. In terms of belief (and disbelief) in good luck, Darke and Freedman (1997a) define belief in good luck as "the [irrational] view that luck is a somewhat stable characteristic that consistently favors some people but not others and is especially likely to favor oneself," whereas disbelief in luck is defined as "the tendency to agree with the rational view of luck as random and unreliable" (p. 490). Darke and Freedman (1997b) found that scores on the BIGL scale predicted positive expectations for the outcome of everyday situations that are typically associated with luck. They also found that those who believed themselves to be lucky were more confident and bet more money

on a betting task, while those who believed themselves to be unlucky, were less confident and bet less money.

Watt and Nagtegaal (2000) administered the BIGL to participants who then purchased tickets in the UK National Lottery. They found that 'lucky' participants (based on total BIGL scores) did not do significantly better than 'unlucky' participants at the lottery task. However, those who specifically believed their luck could affect their lottery success (based on answers to Question 8 on the Lottery Questionnaire scale about luck and its effect on lottery success; see Watt & Nagtegaal, 2000, p. 51) had significantly greater lottery success than those who did not believe their luck could affect their lottery success. In a dice-throwing task, again using the BIGL, there was no significant relationship between BIGL scores and success at the task.

While the results of the above studies are encouraging, with the relationship between luck and psi success significant approximately 50% of the time, more research on belief in luck is needed. In the present study, the focus is on belief in good luck. The tacit understanding will be maintained that belief in good luck has the rational and irrational dimensions already described (viz., Darke & Freedman's, 1997a, definitions given above). The paranormal experiment in the present study necessarily and legitimately eliminates the first three biases (a), (b), and (c), given above as the cause of luck (Smith, Harris & Joiner, 1996). Therefore, luck *per se* may simply be another name for psi, and if that is the case, a relationship between psi success and the BIGL scores might be expected. Thus, belief in good luck, as measured on the BIGL, will be investigated to determine whether it is a condition conducive to paranormal success.

7.4 Intuition

In Jung's (1987) theory of Psychological Types it was proposed that intuition (the dominant personality function in consciousness of the so-called 'intuitive type') serves the purpose of determining the potential (i.e., the efficacy, the possibility, or the future state) of the object under observation, or the outcome of an event. Intuition is one of four functions of Jung's typology, along with Thinking, Feeling, and Sensation. Jung (1957/1977) states, "you get your orientation, you get your bearings in the chaotic abundance of impressions, by the four functions" (pp. 341-342), and described the four functions, thus:

> As soon as the unconscious [content] enters the sphere of consciousness . . . it can become an object of experience only by virtue of the four basic functions of consciousness. It is *perceived* as something that exists (sensation); it is *recognized* as this and *distinguished* from that (thinking); it is *evaluated* as pleasant or unpleasant, etc., (feeling); and finally, intuition tells us where it came from and where it is going. (Jung, 1964, para. 774)

Jung (1957/1977) noted that intuition functioned in an unconscious manner, whereby conclusions were arrived at through mental processes for the most part unexplained. Intuition supposedly accounts for hunches and guesses which 'feel right' to the subject, and may in part (notwithstanding the usual professional expertise and the chance factor) account for successful diagnoses made by doctors, or successful investments made by bankers and stockbrokers, etc.

Jung (1957/1977, p. 309) also saw intuition, especially extraverted intuition, as being the function that assisted gamblers the most in their decision-making. Introverts are too concerned with inner (personal) processes to make judgements about external events. Jung may also have been responding to earlier research in parapsychology that found introverts tended not to score as high on psi tasks as extroverts. Thalbourne (2000a) too has hypothesized that intuition may be a condition necessary for bringing about exosomatic psychopraxia. He refers to a special form of "infallible intuition" that keeps one in "*a condition or state of consciousness*" where "no information is inaccessible" (p. 117). In the present study, both extraverted intuition and introverted intuition will be tested as predictors of success at a gambling task (see **7.5**).

Daniels (1996) defined intuition as "the non-paranormal ability to grasp the elements of a situation or to draw conclusions about complex events in ways that go beyond a purely rational or intellectual analysis." However, we do not know yet whether successful (i.e., efficacious) intuition is an exclusively normal function, and it would be presumptuous to regard it as such. In fact, it is possible that valid intuitions can be the result of paranormal processes. Many decades ago, Hart (1948) noted the possibility of "practical applications of intuition for more rapid and successful development of research in the field of parapsychology" (p. 12). Since then, some efforts have been made towards incorporating the idea of intuition into parapsychological theory and experimental design (for examples, see Edge, 1977; Targ, 1993; Tobacyk & Nagot, 1994).

Weiner (1982) argued that psi may be a process assisted by intuition. Following on from Lousia Rhine, Weiner viewed ESP as a "two-stage process that involves reception of a message followed by the mediation of that message to awareness through the normal psychological vehicles that transmit unconscious material, such as dreams and intuition"

(p. 9). It follows that intuitive types may have an advantage in paranormal tasks, that they may have paranormal abilities, and that they may more often be gamblers than the other three types. The professional gambler may even be more successful at gambling tasks than nonprofessionals due largely to his or her reliance on their intuitive function.

The function of intuition as a Jungian concept has been poorly researched in regard to the paranormal. Parker, Grams and Pettersson (1998) thought that performances on a ganzfeld task by Feeling types, as measured on the Myers-Briggs Type Indicator (MBTI), would be superior to those of the other three types, including the Intuitive type. They found that "hitters scored significantly higher than missers on the Feeling side of the Feeling/Thinking polarization" (p. 331). However, their sample (N = 39) was comprised of (a) 'hitters' (1st rank only), and 'missers' (3rd and 4th rank only, but not 2nd), and (b) mainly females (n = 31; 79% of the sample).

In the case of (a), the absence in the data of participants with 2nd rank positions may have created an artifactual mean Feeling score for missers. Alternatively, if a binary-hits measure was used (which would have meant that *all* the data, rather than *most* of the data, would have been used), 2nd rank 'missers' (i.e., 'near-hitters') would have been included as hitters, producing a different mean Feeling score for hitters than that calculated by Parker, Grams, and Pettersson (1998). However, they did not use their data in either of these ways, nor did they perform the appropriate tests, so it can only be said that their finding for the Feeling function may be misleading, or is ambiguous at best.

In the case of (b), according to Weiner (1982), in her review, "individuals who volunteer to participate in ESP experiments . . . are more often female than male and indicate interests of aesthetic, social, and religious natures" (p. 9). Given Weiner's finding, Parker, Grams and Pettersson (1998) may have tested a biased sample because not only was a majority of females used, but this majority may also have been mostly Feeling types, as would usually be the case for females (see Jung, 1987, para. 597, 640).[64] Feeling types also tend to have "aesthetic [and] *social*" interests (Weiner, 1982, p. 9; emphasis added; and again see Jung, 1987, para. 596, where the importance of a social life amongst feeling types is clarified). (Note also, Chapter 6 for an analysis of sex bias as an influence on ESP-personality outcomes—see *6.2.3* and *6.2.4*.)

It is also noteworthy that Parker, Grams and Pettersson used the MBTI, a test based on the 'bi-polar assumption', which Jung made in his theory about types, but Loomis and Singer (1980) have found evidence that undermines this assumption. The Singer-Loomis

[64] Loomis (1982, p. 67) disputes Jung's claim that the feeling function is pre-eminent amongst females, but there is, as yet, no evidence to support her position.

Type Deployment Inventory (SL-TDI) allows for ostensibly more realistic responses to the functions by not forcing participants to treat them as polarized constructs (i.e., as dimensional pairs: viz., thinking-feeling and sensing-intuiting). Instead, the four functions are measured on separate scales. Therefore, in the present study, the Extroverted and Introverted Intuiting subscales of the SL-TDI, as independent variables, were used as possible predictors of psi performance. Parker, Grams and Pettersson (1998) did not find a significant level of ganzfeld success amongst Intuitive types but, once again, their analysis of subsets only of the data (as indicated above) does not permit the making of a conclusive statement about the intuitive function as a possible psi conducive faculty.

In an exploratory study, Steinkamp (1998) did not find a significant relationship between scores on intuition-type questionnaires and performance on a psi task conducted on the World Wide Web (WWW). However, she criticized the methodology of WWW psi experiments because they tend to lack rigour.

Alexander's (2000) ganzfeld experiment tested selected participants (i.e., 'gifted subjects') on the MBTI to determine possible relationships between Intuition-Feeling (NF) and ganzfeld success. Results were in the opposite direction, so that NF participants produced a scoring rate that was slightly lower than non-NF participants, but the difference was not significant.

As can be seen from the above literature review, limited research in the area of intuition gives little support to the hypothesis that intuition may be a condition necessary for bringing about paranormal effects. Nevertheless, it has become a surrogate term for paranormal process, and has gained sufficient appeal for some researchers, leading them to establish research organizations devoted almost exclusively to research into intuition as a faculty of the human personality.[65] Research by these organizations may lead to further knowledge about intuition and its ostensible twofold (normal and paranormal) function. Intuition was investigated in the experiment described next to determine the possibility of it being a necessary condition for paranormal achievement.

7.5 The Pseudo-Gambling Experiment

Participants were first required to complete four scales (see *7.6.2*). They were then told that they would be participating in a forced-choice 'pseudo-gambling' card-identifying

[65] For example: (1) Physics Intuition Applications, Inc., (online at: <http://www.p-i-a.com/>), (2) Richard Broughton's Intuition Laboratories, Inc., in Durham, NC, USA, and (3) the Perrott-Warrick Research Unit, Psychology Department, University of Hertfordshire, England (intuition research).

experiment, so called because they would not be required to make bets using their own money, although the decisions they made in the task would be made as if they were gambling. They were instructed to use hunches, guesswork, their 'sixth sense', and any other 'faculty' or mode of behaviour or apprehension they considered helpful in making a correct card selection. They were also informed that they would win or lose according to those decisions. In five trials, participants had to identify the correct location of five Aces of Spades while avoiding five Aces of Clubs. 'Instant Scratchies' tickets were paid-out for correctly identified Aces of Spades only (for details, see *7.6.4*).

There were three aims in the present experiment:

1. To gain insight into the nature of compliant and noncompliant pro attitudes.
2. To find necessary conditions for exo-psychopraxia, in the form of scale scores on the GAS, the SL-TDI, and the BIGL, as predictors of paranormal performance.
3. To determine relationships between the three scales used (viz., the GAS, the SL-TDI, and the BIGL).

7.5.1 Parapsychological Hypotheses

The following parapsychological hypotheses were proposed. (The tests used are given in parentheses with each hypothesis.):

1. The number of correctly identified aces of spades (spade-hitting) is above chance ($P_{MCE} = 1.00$) (single-sample *t* test, one-tailed).

2. The number of correctly identified aces of clubs (club-hitting) is below chance ($P_{MCE} = 1.00$) (single-sample *t* test, one-tailed).

3. There is a negative relationship between spade-hitting and club-hitting (Pearson *r* test).

4. There is a positive relationship between attitude towards general gambling (as measured on the GAS) and spade-hitting, and a negative relationship between attitude towards general gambling and club-hitting (two Pearson *r* tests).

5. There is a positive relationship between attitude towards horse racing (as measured on the GAS horse racing subscale) and spade-hitting, and a negative relationship between attitude towards horse racing and club-hitting (two Pearson *r* tests).

6. There is a positive relationship between attitude towards casino gambling (as measured on the GAS casino gambling subscale) and spade-hitting, and a negative relationship between attitude towards casino gambling and club-hitting (two Pearson *r* tests).

7. There is a positive relationship between attitude towards lotteries (as measured on the GAS lotteries subscale) and spade-hitting, and a negative relationship between attitude towards lotteries and club-hitting (two Pearson *r* tests).

8. There is a positive relationship between scores on belief in good luck (as measured on the BIGL) and spade-hitting, and a negative relationship between BIGL scores and club-hitting (two Pearson *r* tests).

9. There is a positive relationship between scores on Extraverted Intuition (EN; as measured on the SL-TDI) and spade-hitting, and a negative relationship between EN and club-hitting (two Pearson *r* tests).

10. There is a positive relationship between scores on Introverted Intuition (IN; as measured on the SL-TDI) and spade-hitting, and a negative relationship between IN and club-hitting (two Pearson *r* tests).

These ten hypotheses are concerned with the issue of compliance. Hypothesis 1 is concerned exclusively with compliance, whereas Hypotheses 2 to 10, which refer to spade-hitting and club-hitting, are concerned with compliance and noncompliance, respectively, since club-hitting would be regarded as the possible result of a noncompliant pro attitude.

7.5.2 Psychological Hypotheses

As discussed above, gambling, intuition, and belief in good luck may be interrelated. Therefore, the following specific hypotheses were proposed (Pearson *r* tests are used to test all three hypotheses):

11. There are positive relationships between EN scores and scores on the four subscales of the GAS (general gambling, horse racing, casino gambling, and lotteries), and negative relationships between IN scores and scores on the four subscales of the GAS (eight Pearson *r* tests).

12. There are positive relationships between BIGL scores and scores on the four GAS subscales (four Pearson *r* tests).

13. There are positive relationships between (a) EN scores and BIGL scores, and (b) IN scores and BIGL scores (two Pearson *r* tests).

7.6 Method

7.6.1 Participants

A total of 100 participants volunteered for the experiment. The majority of the sample was comprised of Adelaide University students at all levels, but mainly undergraduates, all of whom were invited to participate by lodging tear-off acceptance slips in a ballot box located in the Barr Smith Library on the Adelaide University city campus. There was a minority of non-students found by word of mouth, including a subset of 12 members from the paranormal investigation group, PRISM International.[66] The total sample consisted of 45 males (45%) and 55 females (55%). The mean age was 26 years (*SD* = 10.75).

7.6.2 Measures

Four measure were used in the experiment:

(i) The Gambling Attitude Scales (GAS), which comprises four subscales measuring attitude towards gambling in 'general', 'casino', 'horse-racing', and 'lotteries'.

(ii) Singer and Loomis's (1996) Type Deployment Inventory (SL-TDI). The SL-TDI has eight subscales as follows: Introverted Sensing (IS), Introverted Intuiting (IN), Introverted Thinking (IT), Introverted Feeling (IF), Extraverted Sensing (ES), Extraverted Intuiting (EN), Extraverted Thinking (ET) and Extraverted Feeling (EF).

[66] PRISM International (Paranormal Research Investigation Services and Monitoring) is an amateur group of individuals interested in claims of the paranormal, and it conducts investigations into such claims. Members of PRISM International tend to believe in paranormal phenomena and to report paranormal experiences. There is anecdotal evidence that some members have psychic ability. L.S. thanks the president of PRISM International, Mr. Laurie Pearce, for his assistance in acquiring volunteers for the gambling experiment.

(iii) Darke and Freedman's (1997b) belief in Good Luck Scale (BIGL), which is a 12-item scale designed to measure the level of belief in the concept of good luck.

(iv) Thalbourne and Storm's 'states of mind' scales (SOMS), which are eight visual analogue scales designed to measure various state of mind (see Appendix A).

7.6.3 Apparatus

Ten sets of material were used in the experiment: (1) four corkboard panels with wooden frames measuring approximately 450 x 600 mm (18 x 24 inches; each panel includes a 5 x 5 array of clips suitable for holding playing cards in place); (2) 20 packets of 'Queen's Slipper' style playing cards (52 cards/deck); (3) five cards/hand comprised of 1 x Ace of Spades, 1 x Ace of Clubs, 3 x Kings, laid face-down in a row, each row constituting a trial (there are 5 rows, thus 5 trials); (4) 'Cling Wrap' clear plastic sheeting; (5) small, circular 'quik-stik' self-adhesive labels (colour: yellow); (6) Pagano's (1986, pp. 479-480) random number tables; (7) card selection record sheet (see Appendix O); (8) information and instruction sheet (see Appendix P); (9) picture chart showing the Ace of Spades and the Ace of Clubs (see Appendix Q); and (10) debriefing letter to participants (see Appendix O).

7.6.4 Procedure

7.6.4.1 Stage 1 (questionnaires): Participants were required to complete the four measures (see *7.6.2*).

7.6.4.2 Stage 2 (the gambling task): Participants were then required to complete the gambling task (note that the psi task was actually a 'pseudo-gambling' task because the participant was not required to make bets using his/her own money or representations of his/her own money). The participant was presented with a 5 x 5 array of playing cards face-down (pattern on top; total number of cards = 25). In a total of five trials (each row represents a trial), participants were required to locate in each trial the Ace of Spades in a

'hand' of five cards, all cards of which were face down so that their faces could not be seen with the naked eye (for each trial, $P = 0.20$).

Five 'Instant Scratchies' tickets of small cash value (but with high potential cash reward) were issued to the participant before 'gambling' commenced. Each time the participant won, s/he retained a ticket, but a failure to find the Ace of Spades meant the participant had to 'pay' out a ticket from the five previously issued tickets. The participant was free to stop the task at any time and take the winnings accrued as of the time of cessation of the task. Structured this way, the 'pseudo-gambling' task took on the dynamism of a real gambling task because the participant felt and/or thought that s/he was 'winning' or 'losing' without actually making a personal investment in the task.

An Ace of Clubs (a decoy) was also concealed in each of the five hands (for each trial, $P = 0.20$). The decoy was necessary to test the hypothesis that significant overall success at the paranormal task of identifying aces of spades is evidence of a compliant pro attitude toward winning. Participants were thus requested to maintain a pro attitude towards the *detection of the Ace of Spades only*. However, should there be a significant number of hits on the decoy (i.e., the Ace of Clubs), then evidence would exist that undermines the assumption of sufficient compliance, and show that noncompliant pro attitudes can exist as well (see *7.1.1*).

Playing cards were covered entirely with Cling-Wrap, and card selection was made by sticking an adhesive label onto the Cling-Wrap over the card of choice. Thus, the possibility of cheating was eliminated because the participant could not touch the cards, but could only *indicate* his/her choices. Also, the 5 x 5 card arrays were not prepared by the experimenter (L.S.), but by an assistant (either M.A.T. or A.L.B.)[67]. The experimenter was not present during the randomized card-positioning process, and the locations of the cards were not made known to the experimenter prior to the trial. Positioning of the Aces (Spades and Clubs) was by a random process using Pagano's (1986, pp. 479-480) random number tables.

Feedback by e-mail was given to all participants at a later date after questionnaires were scored.

[67] The experimenter, L.S. thanks M.A.T. (Michael A. Thalbourne) and A.L.B. (Alison L. Bruer) for their assistance. Both are researchers in the Anomalistic Psychology Research Unit, Department of Psychology, Adelaide University.

7.7 Results

7.7.1 Preliminary Analyses

All 100 participants in the gambling experiment completed the GAS and BIGL scales, and five trials each in the forced-choice component of the experiment. All but one participant completed the SL-TDI. Randomicity tests were conducted to test the reliability of the data. As just stated (see *7.6.4.2*) placement of the aces (spades and clubs) by the experimenter's assistants was a random process involving the use of Pagano's (1986, pp. 479-480) random number tables. Two tests on the randomicity of spade and club locations were conducted using Friedman's ANOVA test for related-samples (a type of goodness-of-fit test). The 5 columns of spades were compared with each other, as were the 5 columns of clubs. There was no evidence of nonrandom placement of ace-cards—Spades: $\chi^2(4, N = 100) = 2.44$, $p = .656$; Clubs: $\chi^2(4, N = 100) = 8.62$, $p = .071$.

It was therefore advisable to test whether all card selections by participants were random. Therefore, two tests on the randomicity of spade and club selections were conducted using Friedman's ANOVA test for related-samples. The 5 columns of spades were compared with each other, as were the 5 columns of clubs. There was no evidence of nonrandom selection of ace-cards—Spades: $\chi^2(4, N = 100) = 3.67$, $p = .452$; Clubs: $\chi^2(4, N = 100) = 6.70$, $p = .153$. (Descriptive statistics for spade-hitting and club-hitting are listed in Table 7.5—see *7.7.4.1*.)

7.7.1.1 The Gambling Attitude Scales: The theoretical mean for each of the four GAS subscales is 31.50, and the theoretical range for each subscale is 45 (9 to 54). Kassinove (1998, p. 766) reported that the four scales have good reliability, with alpha coefficients ranging between 0.86 and 0.90. In the present study, the alpha coefficients range between 0.80 and 0.88, again showing adequate internal consistency. The test-retest reliability coefficients range between 0.62 and 0.85, which are indicative of good stability over time. The mean scores and standard deviations for the four GAS subscales are given in Table 7.1. The actual ranges, actual minimum scores, and actual maximum scores are also given in the table.

Although 'Casino' and 'Lotteries' were not significantly above the mean, 'General' and 'Horse Racing' mean scores were significantly below the theoretical mean, $t(99) = -7.61$, $p < .001$ (two tailed); $t(99) = -10.01$, $p < .001$ (two tailed), respectively. The sensitivity of our 'politically correct' society to the allegedly nefarious treatment of

animals in sport (especially horse racing) may explain this latter score. The significantly low mean score of the more overarching 'General' attitude towards gambling may be an artifact of the specific attitude towards horse racing which might contaminate the general attitude.

Table 7.1
Means and SDs for the Four Gambling Attitude Scales

Attitude	Mean	*SD*	Range (min. and max.)
General	25.70	7.62	35 (11 to 46)
Casino	30.63	9.02	38 (11 to 49)
Horse Racing	23.77	7.66	33 (9 to 42)
Lotteries	33.02	7.39	32 (16 to 48)

Skews for the four distributions of scores for the four GAS subscales were between –0.096 and 0.315, none of which deviated significantly from normal distributions using the formula given in Chapter 5 (see p. 104, n53). Using independent samples *t* tests, it was found that there were no significant sex differences on any of the four subscales.

7.7.1.2 Belief in Good Luck Scales: Darke and Freedman (1997a, p. 496) reported good levels of reliability for the BIGL, with alpha coefficients ranging between 0.78 and 0.85 in three independent studies. In the present study the scale again proved to be quite reliable with an alpha coefficient of 0.80. They also reported a test-retest reliability coefficient of 0.63, suggesting that the scale was quite reliable "a number of weeks later" (Darke & Freedman, 1997a, p. 496).

The theoretical mean score for the BIGL scale is 42. The actual mean score was 39.56 (*SD* = 9.37), which was significantly below the theoretical mean, $t(99) = -2.60$, $p = .011$ (two tailed). This low score may be attributable to bias in the sample, such that a sufficient proportion of the large number of university students in the sample, possibly having adopted the 'received position' on superstition based on educational prejudice, may have been particularly suspicious of the concept of 'good luck'.

The theoretical range for the BIGL scale is 60 (12 to 72). The actual range was 46 (17 to 63). The distribution of scores had an extremely slight negative skew of –0.020 (*SE*

= 0.241), which was not significant. The mean score for males (M = 41.20, SD = 9.55) was slightly higher than females (M = 38.22, SD = 9.09), but the difference was not significant.

7.7.1.3 The Singer-Loomis Type Deployment Inventory: There are eight subscales in the SL-TDI (see *7.6.2*). The SL-TDI used in the present study is Version 4, which has alpha coefficients for each of the eight scales ranging between 0.67 and 0.76 (Kirkhart & Kirkhart, 1996). In the present study, the alpha coefficients ranged between 0.60 and 0.77. As the SL-TDI is a relatively new inventory, test-retest reliability analyses have not yet been done on the subscales.

Results for the SL-TDI are given in Table 7.2. The theoretical range for each of the eight subscales is 80 (20 to 100). Actual ranges are also given in Table 7.2. Skews for the eight distributions of scores for the eight SL-TDI subscales were between –0.563 and 0.244. Only two distributions of scores deviated significantly from normal distributions: IF (skew = –0.563, SE = .243) and IS (skew = –0.522, SE = .243). These deviations are explained by the presence of an outlier case whose scores on both subscales were extremely low. The exclusion of this case resulted in nonsignificant deviations of the IF and IS distributions. Independent samples t test results showed that there were no significant sex differences on any of the eight subscales.

Table 7.2
Means and SDs for the Eight Singer-Loomis Type Modes

Type Mode	Mean	*SD*	Range (min. and max.)
Introverted Sensing (IS)	65.69	8.83	53 (33 to 86)
Extraverted Sensing (ES)	59.68	8.82	50 (32 to 82)
Introverted Intuiting (IN)	59.24	10.36	64 (23 to 87)
Extraverted Intuiting (EN)	57.56	9.70	49 (33 to 82)
Introverted Thinking (IT)	61.79	9.29	55 (29 to 84)
Extraverted Thinking (ET)	59.11	8.92	55 (30 to 85)
Introverted Feeling (IF)	64.14	7.87	54 (32 to 86)
Extraverted Feeling (EF)	62.61	9.30	42 (40 to 72)

Using independent samples t tests, it was found that there were no significant differences on scoring between the PRISM group (n = 12) and the rest of the sample (n =

88) on the following measures: ace of spades hitting, ace of clubs hitting, the four GAS subscales, and the BIGL scale. There were significant differences on introverted intuiting, $t(97) = -2.32$, $p = .022$, two-tailed, and extroverted intuiting, $t(97) = -2.19$, $p = .031$, two-tailed. Hays (1963) formula was used to calculate estimated effect sizes (omega-squared; ω^2) from the *t* test results (this formula was used in a previous chapter—see *6.2.1*). It was found in both cases that approximately 4% only of the variance in the dependent variables was explained by group affiliation. This value falls short of the critical value of 9%, which is here deemed the minimum level of variance explained that would be functionally important. Therefore, the two groups were not tested separately on the relevant hypotheses (see Hypotheses 9 and 10 in *7.7.2*; and Hypotheses 11 and 13 in *7.7.3*).

7.7.2 Parapsychological Hypotheses

Hypothesis 1: The number of correctly identified aces of spades (spade-hitting) is above chance ($M_{MCE} = 1.00$). The number of aces of spades *was* above chance ($M_{spades} = 1.02$, $SD = 0.82$), but it was not significant, $t(99) = .245$, $p = .807$. The hypothesis was not supported.

Hypothesis 2: The number of correctly identified aces of clubs (club-hitting) is below chance ($M_{MCE} = 1.00$). The number of aces of clubs was *above* chance ($M_{clubs} = 1.06$, $SD = 0.93$). The directional hypothesis was not supported (see *8.7.5*).

Hypothesis 3: There is a negative relationship between spade-hitting and club-hitting. Due to the two variables being semi-independent, the relationship between spade-hitting and club-hitting is likely to be negative, since the outcome on one variable limits the outcome of the other variable. That is, within-trial hitting is not independent, but between-trial hitting is independent. A negative relationship, however, is not guaranteed. For example, given that participants are expected to comply, it could be hypothesized that club-hitting is constant. Alternatively, although unlikely, club-hitting and spade-hitting could pair off for a majority of trials and/or cases due to approach-avoidance conflicts—the upper limit for spades and clubs would be 2.5 of each outcome per participant. Thus, even given the likelihood of 'ceiling' and 'floor' effects, it is possible that the relationship could be near constant, or even positive, so that, theoretically, the direction of the hypothesis could be rejected.

There was a negative and significant correlation between the two types of hitting, $r(98) = -0.23$, $p = .011$, one-tailed. Thus, the relationship between the two types of hitting,

although weak, was not constant in one variable for any given value in the other variable. But in order to be more confident that this correlation is not to be expected by chance, a Monte Carlo method should be employed (though beyond the scope of this thesis). The finding as is, *appears* to support the earlier conjecture (see *7.1.1*) that if compliance is present, noncompliance tends not to be present. That is, if a compliant pro attitude dominates, the noncompliant pro attitude tends to subside, and *vice versa*. However, a related-samples *t*-test was performed with the participant as the unit of analysis and it was found that, taking into account the correlation between spade and club hitting, that neither variable significantly exceeded the other, $t(99) = -0.29$, $p = .771$ (two-tailed). This latter result may also be ambiguous. It is unclear about whether this is the correct approach, but if the choice was available, it would fall on the Monte Carlo method.

Hypothesis 4: There is a positive relationship between attitude towards 'general' gambling and spade-hitting, and a negative relationship between attitude towards general gambling and club-hitting. The relationship between general gambling and spade-hitting was not positive, but a negative relationship between general gambling and club-hitting *was* found. However, the correlation was not significant, $r(99) = -0.11$, $p = .148$, one-tailed. The two-part hypothesis was not generally supported, although the direction was as hypothesized for the general gambling/club-hitting correlation.

Hypothesis 5: There is a positive relationship between attitude towards 'horse racing' and spade-hitting, and a negative relationship between attitude towards horse racing and club-hitting. The relationship between horse racing and spade-hitting was positive, but it was extremely weak and not significant, $r(99) = .04$, $p = 342$, one-tailed. The relationship between 'horse racing' and club-hitting was negative and significant, $r(99) = -.26$, $p = .005$, one-tailed. The hypothesis was partially supported, with directions being correctly hypothesized for both correlations, and significant in one. The more positive the attitude towards horse racing became, the less participants scored in a noncompliant manner by way of club-hitting.

Hypothesis 6: There is a positive relationship between attitude towards 'casino gambling' and spade-hitting, and a negative relationship between attitude towards casino gambling and club-hitting. The relationship between casino gambling and spade-hitting was not positive, but a negative relationship between casino gambling and club-hitting was found. However, this correlation was very weak and not significant either, $r(99) = -0.07$, $p =$

.250, one-tailed. The two-part hypothesis was not supported, although the direction was as hypothesized for the casino gambling/club-hitting correlation.

Hypothesis 7: There is a positive relationship between attitude towards 'lotteries' and spade-hitting, and a negative relationship between attitude towards lotteries and club-hitting. The relationship between lotteries and spade-hitting was not positive, but a negative relationship between lotteries and club-hitting was found. However, this correlation was very weak and not significant either, $r(99) = -0.10$, $p = .161$, one-tailed. The two-part hypothesis was not supported, although the direction was as hypothesized for the lotteries/club-hitting correlation.

Hypothesis 8: There is a positive relationship between BIGL scores and spade-hitting, and a negative relationship between BIGL scores and club-hitting. The relationship between BIGL scores and spade-hitting was not positive, but the relationship between BIGL scores and club-hitting was negative, but not significant, $r(99) = -0.08$, $p = .230$, one-tailed. The two-part hypothesis was not supported, although the direction was as hypothesized for the BIGL scores/club-hitting correlation.

Hypothesis 9: There is a positive relationship between scores on Extraverted Intuiting (EN) and spade-hitting, and a negative relationship between EN and club-hitting. The relationship between EN and spade-hitting was positive, but not significant, $r(98) = .09$, $p = .192$, one-tailed. The relationship between EN and club-hitting was not negative. The hypothesis was not supported, although the direction was as hypothesized for the EN/spade-hitting correlation.

Hypothesis 10: There is a positive relationship between scores on Introverted Intuiting (IN) and spade-hitting, and a negative relationship between IN and club-hitting. The relationship between IN and spade-hitting was positive, but not significant, $r(98) = .13$, $p = .104$, one-tailed. The relationship between IN and club-hitting was not negative. The hypothesis was not supported, although the direction was as hypothesized for the IN/spade-hitting correlation.

7.7.3 Psychological Hypotheses

Hypothesis 11: There are positive relationships between EN scores and scores on the four subscales of the GAS, and negative relationships between IN scores and scores on the four subscales of the GAS. There were no significant correlations between the subscales of the

GAS and EN, although they were in the direction hypothesized. There were no significant correlations between the subscales of the GAS and IN, although they were in the direction hypothesized.

Hypothesis 12: There are positive relationships between BIGL scores and scores on the four GAS subscales. There were four correlations to be tested in this hypothesis, and all four were positive and significant. Table 7.3 lists these correlations.

While the correlations listed in Table 7.3 do not suggest a causal relationship between attitudes towards gambling and belief in good luck, a common-sense relationship is evident in the fact that pursuing an indulgence in various forms of gambling (at least as measured on the GAS subscales) would carry with it a desire in the typical gambler to win, and since chance plays such a big part in winning, a corollary of wishing to win would be a concomitant belief in good luck.

Table 7.3
Correlations between the Four GAS Subscales and BIGL Scores

Variable	*r*	*p*
General Attitude	0.25	.006
Casino Gambling	0.21	.020
Horse Racing	0.17	.048
Lotteries	0.24	.008

Notes: $df = 98$; p values are one-tailed

Note that the four subscales correlate highly with each other, so it is to be expected that the four significant correlations in Table 7.3 may be artifacts due to the intercorrelatedness of the subscales (see Table 7.4). This result is also a possible explanation for the general failures of Hypotheses 4 to 7 to be confirmed.

Table 7.4
Correlation Matrix for the Four GAS Subscales

Variable	General Attitude	Casino Gambling	Horse Races	Lotteries
Casino Gambling	.88	*	*	*
Horse Races	.67	.58	*	*
Lotteries	.51	.42	.37	*

Notes: $df = 98$; all correlations are significant at the 0.01 level (1-tailed).

Hypothesis 13: There are positive relationships between (a) EN scores and BIGL scores, and (b) IN scores and BIGL scores. The relationship between EN and BIGL scores was positive, but not significant, $r(98) = 0.01$, $p = .486$, one-tailed. The relationship between EN and BIGL scores was also positive, but not significant, $r(98) = 0.09$, $p = .183$, one-tailed.

7.7.4 Post Hoc Analyses

7.7.4.1 Attitude towards horse-racing as a necessary condition: The negative and significant horse-racing/club-hitting correlation warranted a median-split analysis of the sample to determine if low scores on attitude towards horse-racing was a condition necessary for bringing about significant club-hitting. The median score for horse-racing in the moderate group was 24. High scorers (> 24) produced a mean hit-rate of 0.78 (SD = 0.704; ES = –0.10), which was significant, $t(44) = –2.12$, $p = .040$, two-tailed. Low scorers (< 24) produced a mean hit-rate of 1.27 (SD = 0.804; ES = 0.13), which was also significant, $t(48) = 2.10$, $p = .041$, two-tailed. It appears that a negative attitude towards horse-racing (scores below 24) is necessary to elicit psi-missing, which might be explained as an impressive form of protest or statement against this form of gambling (reasons for the negative relationship were given above, see *7.7.1.1*). It also seems that a positive attitude (scores above 24) elicits a paranormal attempt to avoid psi-missing, which might be regarded as a move towards psi-hitting. This effect might be a form of approach-avoidance conflict.

7.7.4.2 Effect sizes—spade-hitting and club-hitting: The gambling experiment is a typical forced-choice experiment, the domain of which has a very low, but significant effect size (ES = 0.012; see Chapter 3, Table 3.2). Therefore, Hypotheses 1 and 2 were not likely to be supported, given the relatively small sample size (N = 100), and the fact that only a participant-based calculation of the numbers of hits was tested (see *7.7.2*). On this basis, hit-rates, trial-based z scores, and effect sizes for the two types of hitting were calculated (see Table 7.5).

Rosenthal (1986) recommended that the "conceptual confusion" (p. 316) over replication can be eliminated by focusing on "effect size as the more important summary statistic" (p. 319) because degree of success or failure is more useful than a misleading "dichotomous decision" (p. 319) set up by a dependence on the value of p. Thus, if an

effect size in one study is not significantly different from that of a previous study, replication has been achieved.

Table 7.5
Trials, Hit-Rates, Trial-Based Z Scores and ES Scores for Spades Hitting and Clubs Hitting (N = 100)

Hitting Variable	Total Trials	Total Hits	Proportion of Hits	*Z* score	*ES* [a]
Aces of Spades	500	102	0.204	0.170	0.008
Aces of Clubs	500	106	0.212	0.615	0.028

Notes: [a] The estimate of effect size $z/n^{½}$ is used here, where *z* scores are 'exact'

From Table 7.5 it can be seen that the *ES* was higher on club-hitting than spade-hitting. The *ES* norm for the forced-choice domain falls between these two rates of hitting, with spade-hitting below the mean *ES* norm, and club-hitting above the norm, but the two forms of hitting were not significantly different from each other, $\chi^2(1, N = 1000) = 0.019$, $p = .891$ (see Appendix N for the formulae used in this calculation). Without further testing, it is logical to conclude that these two effect sizes are not only comparable with the significant psi effects reported for the forced-choice domain, but are replications of those effects as well (Honorton & Ferrari, 1989—see Chapter 3, Table 3.2). Therefore, paranormal hitting on aces did occur in the Gambling experiment. These results suggest that the noncompliant pro attitude had more influence than the compliant pro attitude, although the difference appears to be a chance fluctuation only (see *7.8.1*).

7.7.4.3 A reconsideration of the GAS: The present study sought to find relationships between scores on the subscales of the GAS and psi performance. These relationships were thought to be linear. However, from the mostly nonsignificant results relevant to the GAS subscales presented above (Hypotheses 4 to 7—see *7.7.2*), and the nonsignificant results relevant to the BIGL and SL-TDI scales presented above (Hypotheses 8 to 10—see *7.7.2*), there seemed to be good reason to reconsider the GAS, with particular emphasis on the underlying influence of gambling attitudes on the correlations tested in these seven hypotheses (i.e., Hypotheses 4 to 10).

It was conjectured that extreme attitudes on gambling may inhibit other functions, including the psi function—low scorers would have a noncompliant pro attitude, which

may interfere with psi, and/or intuition, and/or luck, whereas high scorers, even with a compliant pro attitude, may suffer the negative consequences of 'heightened anxiety' as a result of trying too hard (cf. Broughton & Alexander, 1997, p. 223). However, 'moderate' scores on the GAS scales might reflect a type of individual who is even-keeled on the subject of gambling. Such a participant, free from the constraints of bias for or against gambling, generally and in its various forms (i.e., horse-racing, casino, and lotteries), may be able to use his or her intuition, and/or belief in good luck, and/or psi function, in an unencumbered manner. Thus, moderate scores may still show a linear relationship with psi scores, as originally hypothesized, and this group of 'gambling moderates' may produce different results from those already found for Hypotheses 4 to 10 (see *7.7.2*).

On the basis of the above rationale, scores for the four scales were combined into an unbiased aggregate gambling score by totaling the four scores of the four GAS subscales (M = 113.12, SD = 26.22; Range: 120; min. = 55, max. = 175). The aggregate score absorbs the diverse effects of the various attitudes on the subscales. An alpha coefficient of 0.93 was calculated for the 36-item aggregated GAS scale, thus showing a high level of internal consistency. The sample was then divided into three groups: 'low' (n = 34), 'moderate' (n = 33), and 'high' scorers (n = 33) based on aggregated scores. Table 7.6 shows the means and standard deviations for the three groups.

When Hypotheses 1 to 3 were re-tested for each of the three groups, there was (i) no significant spade-hitting or club-hitting for any group, and (ii) no significant spade-hitting/club-hitting correlation for the 'low' and 'high' groups, but the correlation for the 'moderate' group was significant, $r(31) = -0.35$, $p = .024$.

Table 7.6
Mean and SDs for Aggregate Gambling Scores by Groups

Group	Mean	*SD*	Range (min. and max.)
'low' (n = 34)	84.97	13.36	54 (55 to 109)
'moderate' (n = 33)	113.15	10.56	38 (97 to 135)
'high' (n = 33)	141.24	14.16	64 (111 to 175)

When the proportion of hits and effect sizes were calculated, however, the low group produced hit-rates above the *ES* norm for the forced-choice domain, as did the moderate group on spade-hitting, and the high group on club-hitting (see Table 7.7). These effect sizes were compared. Note that the use of trial-based z scores and effect size

estimates follows Honorton and Ferrari's (1989) observation that "most parapsychological experiments . . . have used the trial rather than the subject as the sampling-unit" (p. 283). Thus, valid comparisons of *z* scores in the Gambling experiment, as they appear in Table 7.7, are made against Honorton and Ferrari's calculation of the *z* score norm for the forced-choice domain (viz., $z = 0.38$).

Using the single-sample *t*-test, two performance comparisons were made between the six *ES* values listed in Table 7.7 and (i) the mean *ES* of 0.097 for the six groups as the test statistic, and (ii) the mean *ES* norm of 0.012 for the forced-choice domain as the test statistic. The *t* values in both cases were not significant. Thus, the six *ES* values comprise a homogeneous data set comparable in performance to that of the forced-choice domain.

When Hypotheses 4 to 10 were re-tested for each of the groups, there were no significant correlations for the 'low' group, and two significant correlations for the 'high' group: horse racing/club-hitting, which was in the direction hypothesized, $r(32) = -0.33$, $p = .029$; and IN/club-hitting, which was not in the direction hypothesized. Since fourteen tests were run, this latter significant correlation could well be a Type I error.

Table 7.7
Trials, Hit-Rates, Trial-Based Z Scores and ES Scores for Spades Hitting and Clubs Hitting ('low', 'moderate', and 'high' groups on the Aggregated GAS)

Hitting Variable	Group	Total Trials	Total Hits	Proportion of Hits	Z score	*ES* [a]
Aces of Spades	Low	170	39	0.229	0.862	0.148
	Moderate	165	30	0.181	0.487	0.085
	High	165	33	0.200	0.000	0.000
Aces of Clubs	Low	170	42	0.247	1.440	0.247
	Moderate	165	34	0.206	0.098	0.017
	High	165	30	0.181	0.487	0.085

Notes: [a] The estimate of effect size $z/n^{½}$ is used here, where *z* scores are 'exact'

The moderate group, however, produced four significant correlations out of fourteen tests, all of which were in the directions hypothesized: (i) spade-hitting correlated

with EN, $r(31) = 0.31$, $p = .042$, (ii) spade-hitting correlated with IN, $r(31) = 0.32$, $p = .037$, (iii) club-hitting correlated with EN, $r(32) = –0.28$, $p = .05$, and (iv) club-hitting correlated with IN, $r(31) = –0.34$, $p = .027$ (all tests were one-tailed). Note that the IN and EN correlate significantly so that there is some degree of nonindependence between these two variables, $r(31) = 0.65$, $p < .001$, two-tailed. This fact may account for the significant correlations given in (i) and (ii) above. When partial correlations were conducted controlling for and EN and IN, none of the four correlations in (i) and (ii) above were significant. However, the aggregated score of Intuiting (EN and IN combined), correlated postively and significantly with spade-hitting, $r(31) = 0.34$, $p = .025$, and negatively and significantly with club-hitting, $r(31) = –0.34$, $p = .025$ (both tests were one-tailed). These results suggest that Intuiting (provided that the measure is the aggregated score on Intuiting) is a predictor of psi effects, but only when the score on aggregated attitude towards gambling is a moderate one. A further two correlations were in the directions hypothesized, but only approached significance: horse racing/spade-hitting, $r(32) = 0.25$, $p = .077$, and horse racing/club-hitting, $r(32) = –0.26$, $p = .072$.

7.7.4.4 The Interim Report (N = 80): An interim report on the progress of the gambling experiment was prepared and presented as a poster at the Adelaide University Open Day in October 2001. At that time, testing was not complete ($N = 80$). The interim results of tests for each of the above hypotheses were quite different from those of the larger sample presented above ($N = 100$). There were 3 significant correlations, all in the direction hypothesized: (i) club-hitting correlated negatively and significantly with attitude towards horse racing, $r(79) = –0.24$, $p = .015$, (ii) spade-hitting correlated positively with EN, $r(79) = 0.19$, $p = .048$, and (iii) spade-hitting correlated positively with IN, $r(79) = 0.20$, $p = .041$ (all tests were one-tailed). However, the number of aces of spades correctly identified was not above chance ($M = 0.94$, $SD = 0.80$), and the number of aces of clubs correctly identified was not below chance ($M = 1.15$, $SD = 0.94$), where $M_{MCE} = 1.00$. In both cases, results were not in the direction hypothesized and were not significant.

At the time of the Open Day, two conclusions were arrived at: (a) the horse racing variable, as measured on the GAS subscale, was a predictor of psi hitting, and (b) the EN variable and the IN variable, as measured on the SL-TDI subscales, were predictors of psi hitting. Thus, in the case of (b), there was support, at least at that time, for Thalbourne's (2000a) hypothesis that "infallible Intuition" (p. 117) is a form of paranormal information acquisition, insofar as high scores on measures of intuition may be necessary for bringing

about paranormal effects. These three significant results were lost when another 20 participants were tested to complete the study.

Broughton and Alexander (1996) found a similar effect: At the 39th Annual Convention of the Parapsychological Association, they presented an interim report, which featured a significant hit-rate on targets in a series of ganzfeld studies, but when the experiment was completed, their significant result was lost (Broughton & Alexander, 1997, p. 218). In paranormal research, such gains and losses may simply be the result of chance fluctuation. On the other hand, the cause may not be chance, although finding the cause may be a difficult process (cf. *6.2.1*. See also Broughton & Alexander, 1997, p. 223). In the case of the present study, it is possible that further testing might yield a return to the significant results found in the interim report.

7.7.5 Success Rates

In general, out of a total of 13 planned hypotheses, 28 tests were conducted. Only 6 tests were significant (21%). However, 21 out of the 28 tests (75%) produced results that were in the hypothesized directions.

In a total of 10 planned parapsychological hypotheses, 17 tests were conducted. Only 2 tests were significant (12%). However, 10 out of the 17 tests (i.e., 59%) produced results that were in the hypothesized directions.

In a total of 3 planned psychological hypotheses, 14 tests were conducted, 4 of which were significant (29%). All tests produced results that were in the directions hypothesized. All these results may be inflated, or deflated, as a result of non-independent variables. The GAS subscales, for example, correlate with each other, which probably accounts for the generally nonsignificant results of Hypotheses 4, 6, and 7 (see Table 7.4).

7.8 Discussion

There were three aims in the present experiment:

1. To gain insight into the nature of compliance and the pro attitude.

2. To find necessary conditions that bring about psi.

3. To determine relationships between the three scales used (viz., the GAS, the SL-TDI, and the BIGL).

These are now discussed.

7.8.1 Compliance and Noncompliance

The problem of compliance rests hand-in-hand with the concept of the pro attitude because the experimenter plays a pivotal role in the construction of the participant's pro attitude, and he or she does this under the expectation of compliance. Compliance was argued to be an important concomitant of the pro attitude (see *7.1.1*). Take away compliance, and the possibility exists that the goal on which the pro attitude is focused may not manifest. It may, however, only be possible to imply the presence of the pro attitude since measurable effects may not manifest—hence the importance of sufficient compliance in the sense that its presence is crucial in the experiment for eliciting significant results. This situation suggests that falsifying the pro attitude hypothesis may be impossible. However, given that pro attitudes can be oppositional to each other, a solution was proposed that might identify the presence *or absence* of compliant and noncompliant pro attitudes, and therefore falsify the pro attitude hypothesis. Two questions must therefore be asked: "Was there evidence of compliant and noncompliant pro attitudes during the experiment?" and "Was there evidence that no pro attitudes were present, yet psi effects were still elicited?" The second question refers to the problem of falsifiability of the pro attitude. To attempt answers to these questions, evidence would need to be found as to whether compliant and noncompliant pro attitudes existed in a sufficient number of participants.

It must first be stated that some degree of effort was made in the present experiment to refute the concept of the pro attitude. Participants were 'set up', as it were, to choose between two alternative forms of targeting. This protocol encouraged certain types (most likely believers in the paranormal) to adopt a compliant pro attitude (it is tacitly understood that believers would endeavour to adopt a compliant pro attitude), whereas certain other types (most likely skeptics naïve to statistical testing of psi) had the opportunity to adopt alternative but noncompliant pro attitudes of their own devising (extreme skeptics, for example, might target aces of clubs, while moderate skeptics might default to king cards). Structured this way, the protocol attempted to eliminate a number of possible strategies on the part of skeptics by 'forcing' them to adopt one of a limited number of noncompliant pro attitudes. (Note that it is assumed that both skeptics and believers were present in the experiment, as is the case in randomized psi experiments.)

In the present study, a successful hit-rate in the 'Ace of Spades' task could be taken to mean that the assumption of sufficient compliance had been met, and therefore, that a

compliant pro attitude had been held by a sufficient number of participants. Likewise, a successful hit-rate in the 'Ace of Clubs' task could be taken to mean that noncompliant pro attitudes had been held by a sufficient number of participants. In either case, the sample did not produce a significant number of hits on spades or clubs as measured on participant-based counts, although the effect sizes for both forms of hitting, as measured on trial-based counts, were not significantly different from the mean effect size norm for the forced-choice domain (see *7.7.4.1*). From the relative strengths of the effect sizes there appeared to be evidence of replicated psi effects, so that both forms of hitting suggest that compliant and noncompliant pro attitudes were present during the experiment.

It was also shown that the presence of a compliant pro attitude tended to indicate the absence of noncompliant pro attitudes, as was found in Hypothesis 3 (see *7.7.2*). Thus, it was shown that, rather than there being a relationship based on the possibility that one of the pro attitudes might be near constant (or even positive), there was an inverse relationship between the compliant and noncompliant pro attitudes, and this relationship is an important one. From the perspective of the theory of psychopraxia, Thalbourne (2000a, p. 65) suggested the possibility of mutually opposed pro attitudes. The negative relationship also indicates that evidence for compliance and noncompliance, albeit conjectured on the basis of statistical outcomes, can be found, and if it was ever possible for all participants not to hold a pro attitude of any kind, chance would have decided the outcome, but the chance factor was ruled out, based on the above results (see *7.7.2* and *7.7.4.1*).

In terms of falsification, the result of Hypothesis 3, taken to a logical extreme, suggests that if evidence could be found that a compliant pro attitude exclusively pervaded a given experiment, that same evidence would suffice to indicate that sufficient levels of noncompliant pro attitudes were not present. Proof that noncompliant pro attitudes of any kind are not present would show that the concept of the pro attitude is falsifiable, since a noncompliant pro attitude in one situation can be a compliant pro attitude in another. Thus, the first aim of attempting to understand the relationship between the pro attitude on one hand, and compliance and noncompliance on the other hand, was achieved, but only to the degree that it was possible to falsify the concept of the pro attitude, which was not possible in any absolute way given the weak effect sizes.

7.8.2 Attitudes and Dispositions as Necessary Conditions of Psi

This study attempted to find necessary and sufficient conditions for paranormal performance. No relationships were found between (a) psi effects and belief in good luck, (b) psi effects and extraverted intuiting, or (c) psi effects and introverted intuiting. Only

one of the four GAS subscales (horse racing) correlated negatively (as hypothesized) and significantly with club-hitting, suggesting that a positive attitude towards horse racing tends to result in less noncompliant psi targeting (i.e., in traditional terms, psi missing). It is possible that this trend did not extend into a significant relationship between horse racing and compliant psi targeting (i.e., psi hitting) because participants may have been put off by cognitive dissonance in the form of a movement against the use of animals in sport, and benefiting financially from that arrangement. Some slight support for the other parapsychological hypotheses came in the form of effects in the hypothesized directions in more than half the cases (59%), which could suggest that evidence may be found in later studies that these conditions are necessary for, or at least conducive, to psi.

In a post hoc analysis (see *7.7.4.2*) it was argued that a moderate attitude towards gambling would be psi-conducive because the 'moderate' participant was free from the inhibitory effects of extreme viewpoints about gambling, which might 'scramble' the psi function, and/or the intuitive function, and/or belief in good luck. Insofar as 'low' and 'high' scorers on the aggregated scales of the GAS were excluded from the analysis, this assumption ran counter to previous hypotheses, which assumed linear relationships between the relevant variables (see *7.7.2*, Hypotheses 4 to 10).

Initially it was found that both types of Intuiting (extraverted and introverted) were predictors of both spade-hitting and club-hitting (4 significant correlations out of 14 analyses), but further analysis revealed that EN and IN were nonindependent variables. Partial analyses and further bivariate analyses revealed that aggregated Intuiting scores correlated positively and significantly with with psi-hitting and negatively and significantly with psi-missing. Thus, it appeared that the relationship between intuition and psi seems to be tempered by the effect of a moderate attitude towards gambling. In terms of psychopraxia, if the participant holds a moderate attitude towards gambling, an exosomatic effect seems more likely if the participant scores high on aggregated Intuiting, and this effect can result from either a compliant or a noncompliant pro attitude.

The median-split analysis revealed that negative attitudes towards horse-racing were a necessary condition that elicited psi-missing (i.e., significant club-hitting), while positive attitudes resulted in paranormal attempts to avoid keep club-hitting as low as possible. Low and high scores were necessary conditions that gave participants the opportunity to accomplish these very different psi tasks.

7.8.3 The Relationship between Gambling, Belief in Good Luck, and Intuition

Psychological relationships between seven scales (i.e., four GAS subscales, IN and EN on the SL-TDI, and the BIGL) were hypothesized (see Hypotheses 11, 12 and 13—*7.7.3*). Relationships were found consistently between gambling and belief in good luck—all four GAS subscales correlated significantly with the BIGL and in the directions hypothesized. Gambling and belief in good luck seem to be related in a way that common sense would see as appropriate (see Hypothesis 12—*7.7.3*). However, there was no evidence that extraverted intuition and introverted intuition are related to the BIGL. Nor was there evidence that EN and IN are related to the GAS. Some tentative support for these relationships came in the form of effects in the directions hypothesized.

In the next chapter, the issue of the pro attitude will be taken up again to test Thalbourne's theory that psychopractic goal-states can be altered as initiated by a change in pro attitude. Skeptics were tested in a forced-choice symbol-identifying task to see if the pro attitude is mutable.

CHAPTER 8
THE EFFECTS OF CHANGE OF PRO ATTITUDES ON PARANORMAL PERFORMANCE BY NAÏVE AND SOPHISTICATED SKEPTICS (A PILOT STUDY)

One goal which psi is used to bring about is the reduction of cognitive dissonance.
Michael A. Thalbourne (2000a, p. 57)

Some belief in psi arises from misjudgements of probability.
Susan Blackmore and Tom Trościanko (1985, p. 467)

8.1 The Pro Attitude in the Theory of Psychopraxia

In Chapter 7, the pro attitude was hypothesized as playing an initiating role in bringing about psychopractic effects (notwithstanding the presence of certain other necessary and sufficient conditions). In the present chapter, the pro attitude is the central issue of concern since it is not only necessary to establish the possible existence of the pro attitude (for which evidence was found—again see *5.7.5*, and also *5.8.1.3*), but also to determine whether it is mutable. Therefore, in the present chapter, the possibility of influencing, or better, altering the pro attitudes of so-called sophisticated skeptics as a direct result of a pedagogical explication of the laws of chance are considered and investigated.

This chapter also investigates paranormal belief and experience (as measured on the Australian Sheep-Goat Scale) as a possible predictor of paranormal success, and as a possible 'necessary' condition. Before proceeding to the experiment, the nature and influence of attitudes in ESP research must first be considered. It will be seen that, more generally, attitude appears often to play a crucial role in outcomes of psi tasks.

8.1.1 Attitude-ESP Research

Palmer (1977) noted that attitude towards ESP in the test situation has been "an extensively studied predictor of ESP test performance" (p. 193). Much of the early work on attitude-ESP research actually preceded personality-ESP research. As early as the 1930s, Rhine began taking notice of the types of individuals who seemed to give the best

performances on ESP and PK tasks. He found repeated evidence that the more enthusiastic, curious and motivated participants would more reliably score above chance than did other types of participants (Rhine, 1937/1950, pp. 65, 84-85; 1948/1954, pp. 54, 119).

Some researchers relied purely on their own creativity in scientifically classifying participants. Humphrey (1949), for example, using Elkisch's (1945) expansive-compressive dichotomy, rated participants according to their style of drawing in an ESP picture-guessing task: The 'expansive' type used the available space on the page to maximum effect, whereas the 'compressive' type confined themselves to small sections of the available space. Expansive types scored significantly higher than compressive types. However, in a GESP task (i.e., a general ESP task, which could be taken as a measure of telepathy or clairvoyance), 'compressives' scored higher than 'expansives'. (Note, though, that Palmer, 1977, p. 188, drew attention to the fact that the "expansiveness-compressiveness" is unstable over time and therefore should not be interpreted as a trait measure suitable for ESP research.)

Although these differences between Humphrey's two types on scoring patterns, which varied according to the psi task, created an anomaly in itself, Humphrey's early work established a paradigm as to how to classify participants. Specifically, dichotomous measures to this day are considered suitable means of separating out two types of subject.

Schmeidler (1945, 1960) took a different approach from Humphrey when she introduced the term 'sheep' to describe a person who believes in the possibility of ESP (sheep) under the given experimental conditions, and 'goat' as one who rejects this possibility. That is, Schmeidler considered the possibility of a direct relationship between paranormal belief and psi performance. Her measure also draws out a specific attitude to psi rather than a personality type *per se*. The sheep/goat nomenclature usually applies in the experimental situation, where participants are classified according to their answers to a question about their belief in ESP or PK, or answers to a series of psi-relevant questions that together constitute a scale score.

When Schmeidler (1945, 1960) administered an ESP task to sheep and goats, she found significant differences in scoring between the two groups—sheep tended to score significantly above *MCE* and goats tended to score significantly below *MCE*. Also, when sheep were sub-divided into two subgroups ('well-adjusted' and 'poorly adjusted'), the well-adjusted sheep scored higher than the poorly adjusted sheep.

Palmer (1977) reported the general finding that the sheep-goat dichotomy is probably the most reliable predictor of ESP performance to date: Thirteen out of 17 sheep-goat experiments (76%) from 1947 to 1970 "were in the predicted direction" (p. 193). He

added that experiments from 1970 to 1977 consistently confirmed the sheep-goat hypothesis. For example, Schmeidler and McConnell (1973) conducted a series of experiments and again found that sheep scored significantly above *MCE* and goats scored significantly below *MCE* (with a highly significant difference between mean scores for sheep and goats). (See Palmer, 1977, pp. 193-195, for a review of other sheep-goat experiments. See also Lawrence's, 1993, meta-analysis of the sheep-goat effect—*3.1.6*.)

Storm and Thalbourne (1998-1999, pp. 113-114) constructed a 'super-sheep' question (see Beloff & Bate, 1970), where a super-sheep is defined as "a subject who is sure that their score on a test of [ESP] will be high, by virtue of their own psychic ability" (Thalbourne, 1982, p. 72). Perhaps not surprisingly, Storm and Thalbourne found a significant correlation between the super-sheep question and the 29-item Transliminality Scale (Thalbourne, 1998), since 14% of items refer to paranormal phenomena. The belief question itself was also a direct predictor of transliminality (see the path analysis shown in Figure 8.1).

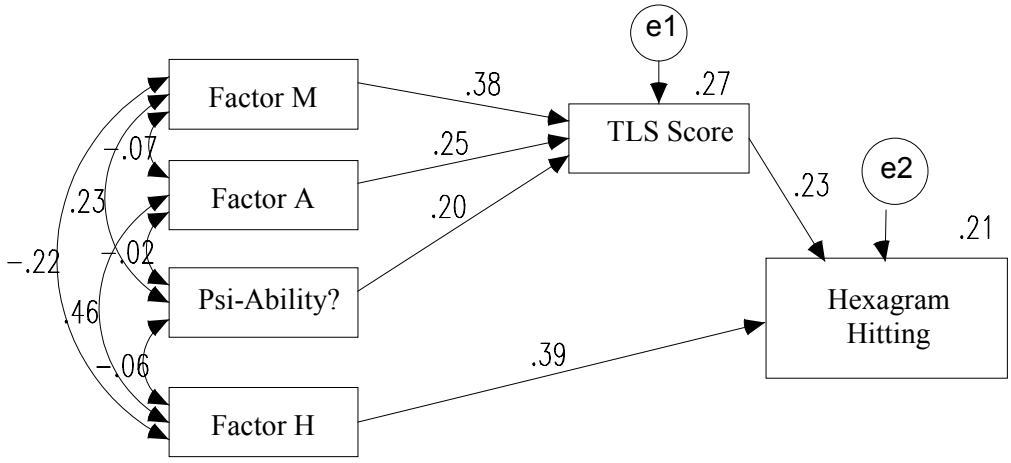

Figure 8.1. Path analysis: Belief in one's paranormal ability (Psi-Ability?) is a direct predictor of transliminality (TLS Score), and an indirect predictor of Hexagram-Hitting.[68]

[68] The path coefficients are the β values from two MRAs (for details, see Storm & Thalbourne, 1998-1999, p. 112). The R^2 values are also shown on the top-right of 'TLS Score' and 'Hexagram Hitting'; e_1 and e_2 are unobserved variables, not accounted for in Storm and Thalbourne's initial study, which contribute unexplained error variance to the model. Curved paths are covariances between the four exogenous variables.

The belief of a 'super-sheep', which can be regarded as an attitude toward psi, was also an indirect predictor of hexagram hitting, and therefore, a possible indicator of paranormal performance.

In the study reported in Chapter 6, the correlation between transliminality and hexagram hitting was significant, and the correlation between answers to a belief question and a paranormal task (generation of changing lines) was also significant (N = 200; see *6.3.4* and *6.3.8*). Specifically, these correlations suggest that belief in paranormal phenomena, and belief in the possibility of producing such phenomena, are involved in the *I Ching* process, thus leading to the generation of psi effects.

Having found reasonable support for the hypothesis that attitude is conducive to ESP, especially as has been found from the meta-analysis of belief in ESP (see again, *3.1.6*), a more detailed consideration of the purpose of this chapter is next presented.

8.1.2 The Nature of the Pro Attitude

The concept of the pro attitude was introduced in Chapters 4 (see *4.4.1*) and discussed in Chapter 5 (see *5.1.2*). Recall Thalbourne's (2000a) definition of the pro attitude in *4.4.1*: "a person may be said to have a pro attitude towards state **S** when they would consciously prefer **S** rather that **–S** [**not S**] if those two alternatives were to be brought to their attention" (Thalbourne, 2000a, p. 65).

It was shown in Chapter 5 that where significant hitting occurred on pre-designated hexagrams, but not on second hexagrams (*5.7.5*, Hypotheses 2 and 3), that there was evidence that the pro attitude of the participant towards a specific outcome existed. It was then stated (see *7.1.1*) that the presence of pro attitudes could be implied by sufficient compliance (or noncompliance). However, the implication of a pro attitude is only partial evidence of its existence. To show that the pro attitude is likely to exist, it is necessary to measure it directly, or, as in the present study, to manipulate the pro attitude, in some way. The manipulation itself would need to take the form of an influence that alters more than the goal of the participants, but preferably, alters the predisposition of participants. Of course, experimentally, it is preferred that the possible effects of this influence be consciously acknowledged by the participant, so that its effects are measurable (specific details of this process are given in the next section—see *8.2.1*).

Thalbourne (2000a, pp. 56-60) points out that significant negative scoring is not regarded as evidence for psi by most people, but is instead regarded as, for example, "fail[ing] miserably" (p. 59) at the psi task. Psi-missing is supportive of the psi hypothesis in general, but, in particular, some skeptics (i.e., naïve goats) may not see it that way.

Rather, they would argue that extreme failure vindicates their belief that "chance" has again played its part in the failure to reject the Null hypothesis. Thus Thalbourne (2000a, p. 61) suggested that changes in scoring outcomes (or changes in the variance of those scores between multiple runs indicating shifts in scoring towards chance-scoring) should occur as a result of the attempted manipulation of skeptics' beliefs about low scoring and its relevance to the psi hypothesis when low scores become psi-missing. The change in scoring would indicate a change in pro attitude.

In (say) a forced-choice experiment, it would be possible to ask unselected participants to target a specific symbol (say 'star') for a given number of trials in the first run, and then, in the second run, ask participants to retarget a different symbol (say 'cross') for the same number of trials. If scoring is significant in both runs, a change of pro attitude would have been demonstrated, but this design would only be testing the sheep-goat effect twice. Thus, there are two issues of concern with the design just described: (a) the change in pro attitude would be a change in the representation of target only (i.e., sheep and goats would perform as expected for both runs, but this effect has been demonstrated many times—see *8.1.1* above), and (b) the nature and extent (i.e., degree of entrenchment, or predisposition) of an overarching, or 'core-value', pro attitude towards psi-missing, which is the *real* pro attitude of interest, is the one that is irreconcilable with a pro attitude towards psi-hitting, but one that might be changed to the latter.

The Skeptics experiment, then, offers the experimenter the opportunity to attempt a treatment of the under-lying predisposition of the participant (e.g., to psi-miss at all times—see Thalbourne, 2000a, pp. 60-63), which is a pro attitude here defined as *primary* in nature, rather than just treat a formal pro attitude here defined as *secondary* in nature. A primary pro attitude would represent, or be underscored by, a core value, and would affect paranormal performance the same way every time, from experiment to experiment, and is therefore *permanent* (or at least *enduring*). A secondary pro attitude is merely based on the situational contingencies of a given experiment, and is therefore *ephemeral* (e.g., a 'star' might be targeted this time, a 'cross' the next).

8.2 The '*Skeptics*' Experiment

8.2.1 Description of the Experiment

Skeptics are those persons who reject the possibility that paranormal phenomena such as ESP or PK can occur. In the theory of psychopraxia, this 'rejection' is the

foundation of a *primary* pro attitude towards failing at a psi task, e.g., by getting a low score. Generally, skeptics are not likely to be easily dissuaded from their *a priori* beliefs, and it is likely that they will adopt the appropriate pro attitude that would vindicate their belief that low scores are evidence of the absence of ESP (see Thalbourne, 2000a, pp. 60-63). This skeptical pro attitude, however, may change as a result of statistical explication.

There are two main types of skeptics in this study—those that are *naïve* (i.e., not educated to the concept of significant scoring in its two forms, psi-hitting and especially psi-missing, and thus still presumably have a pro attitude towards missing), and those that are *sophisticated* (i.e., educated to the concept of significant negative scoring). Moreover, in the sophisticated group, there are two subtypes: *entrenched* and *converted* (note that since both types are 'sophisticated' in the sense that they are educated to the concept of significant negative scoring). Those that are *entrenched*, are those who believe that a statistically significant result does not prove the existence of psi (these are those participants who answered 'No' to the question: "Do you believe that a score which deviates significantly from chance might indicate the presence of ESP?" (see *8.3.2*). Those who are *converted* are those who believe that there can be statistical evidence of psi (these are those participants who answered 'Yes' to the same question).

The purpose of this experiment is to examine performances of naïve and sophisticated skeptics on a computer-run forced-choice symbol-identifying task over the course of two runs (50 trials each). Appropriate hypotheses are made about entrenched and converted skeptics (see *8.2.2*). For entrenched skeptics, it is expected that the Null hypotheses will be retained (i.e., deviations in scores and/or variances between runs are at chance), but it is expected that the Null hypotheses will not be retained for the converted skeptics.

Participants were instructed to guess which of five different Zener-card symbols presented in random sequence on a computer screen was the target symbol pseudo-randomly pre-selected by the computer before each trial (see Figure 8.2). There were 50 trials in the first run, and possibly 50 trials in a second run if the computer typed them as a skeptic according to their scores on the 18-item Australian Sheep-Goat Scale (ASGS; Thalbourne, 1995b), which would need to be less than 17, which was empirically derived.

At the conclusion of each run, participants would receive feedback from the computer (given as a score out of 50 for each run). If participants were skeptics, and so were to proceed to the second run, the computer would then do one of two things, the course of action being chosen at random: either (a) tell them to continue directly to the

second run, or (b) give them additional information about the statistical interpretation of their scores, and then they would be told to continue to the second run.

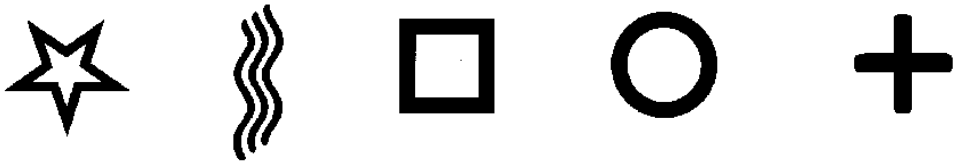

Figure 8.2. The Zener-card symbols: Star, Waves, Square, Circle, and Cross.

8.2.2 Hypotheses

The following parapsychological hypotheses were proposed. (The tests used are given in parentheses with each hypothesis.):

1. The mean score for symbol hitting for converted skeptics is (a) below chance in the first run (*MCE* = 10 correct symbols; single-sample *t* test, one-tailed), (b) above chance in the second run (single-sample *t* test, one-tailed), and (c) lower in the first run compared to the second run (Wilcoxon test, one-tailed).

2. The mean theoretical variance[69] of symbol hitting for converted skeptics is smaller in the second run than in the first run, where theoretical variance = $(10 - \text{hits})^2$ (Wilcoxon test, one-tailed).

3. The mean score for symbol hitting for entrenched skeptics is (a) below chance in the first run (*MCE* = 10 correct symbols; single-sample *t* test, one-tailed), and (b) below chance in the second run (single-sample *t* test, one-tailed).

4. The mean theoretical variance of symbol hitting for entrenched skeptics is different between the two runs (Wilcoxon test, two-tailed).

[69] Theoretical variance is here calculated as the squared deviation from *MCE*, where *MCE* = 10. Note that other investigators have also used theoretical variance as an indicator of paranormal performance (for examples, see Rogers, 1966; Rogers & Carpenter, 1966; and Stanford, 1966).

5. There is a positive relationship between ASGS scores and symbol hitting for believers (1st RUN data) and converted skeptics (2nd RUN data; Two Pearson *r* tests, one-tailed).

8.3 Method

8.3.1 Participants

A total of 131 participants volunteered for this experiment. Thirty-two participants (24%) were Adelaide University students, while the majority (76%) were volunteers from the general public, most of whom attended the Adelaide University's two Open Days (October 13, 2000, and August 19, 2001). Fifty-two participants (40%) were male and 79 were female (60%). The mean age was 26 years (*SD* = 12.85).

8.3.2 Measures

Four measures were used in the experiment:

(i) The 'states of mind' scales (SOMS—see Appendix A).

(ii) The forced-choice version of the Australian Sheep-Goat Scale (ASGS; Thalbourne, 1995—see Appendix R).

(iii) The single-item question: "Before you started the SECOND run of 50 trials, did you understand that if your FIRST-RUN score was *way below* or *way above* chance, it might reach statistical significance?" The concept of significance was explained to sophisticated skeptics as follows: "If your score was *way below* chance such that it might reach statistical significance, it would indicate the presence of a form of ESP in which the correct target was *avoided* more often than chance would allow. However, if your score was *way above* chance such that it might reach statistical significance, it would indicate the presence of a form of ESP in which the correct target was *sought* more often than chance would allow. If only chance was operating and there was no ESP, your score would be expected to be much closer to chance." The question requires a 'yes' or 'no' answer (see Appendix S).

(iv) The single-item question: "Do you believe that a score which deviates significantly from chance might indicate the presence of ESP?" The question requires a 'yes' or 'no' answer (see Appendix S).

8.3.3 Apparatus

Four items were used in the experiment: (1) information sheet (see Appendix T); (2) instruction sheet (see Appendix U); (3) desktop computer; and (4) computer program, including the 1 (or 2) runs of 50 Zener symbol-guessing trials, and specifically worded instructions addressed to the participant throughout the symbol-guessing component of the experiment (for exact details, see Appendix S).

8.3.4 Procedure

Once ethics approval was granted from the relevant departmental ethics committee, the experiment was advertised in the foyer of the Department of Psychology. Apart from being opened to the general public at the two Adelaide University Open Days (see *8.3.1*), the experiment was also opened to First-Year psychology students towards credit for their overall grade in the psychology course, but non-psychology students also participated. Participants who required information about the experiment were issued leaflets explaining the running of the experiment (see Appendices T and U).

Most participants logged-on to a computer in the Computer Suite (Department of Psychology), but some were able to access the experiment on computers outside the suite. Starting from the Department of Psychology's *Home Page* via *Netscape Navigator* they followed the meta-links to 'ESP Experiment' (i.e., the Skeptics experiment).

The experiment opened with a consent form. To start the actual experiment, participants clicked on the bar at the bottom of the consent form screen, which implied that they gave their consent to participate (this screen could not be bypassed). There were 2 stages to the experiment:

Stage 1—Two Questionnaires: A survey of belief in the paranormal using the ASGS, followed by a short inquiry into participants' states of mind using the SOMS.

Stage 2—The Paranormal Task: One (or two) runs of the symbol-guessing tasks (50 trials/run). Participants were required to guess the computer's pre-selected Zener symbol (1 of 5; see Figure 7.2). Selections were made by participants' clicking the 'radio' button under the symbol they thought to be the computer's pseudo-randomly pre-selected symbol.

Order of the symbols was presented at random to help avoid symbol preference. Total scores correct followed each run.

8.4 Results

8.4.1 Preliminary Analyses

Randomicity tests were conducted to test the reliability of the data. Although the computer program was designed to placed the target symbols randomly, four Friedman's ANOVA tests for related-samples were conducted to test the reliability of the randomization procedure of the program, for the computer's target choice and the column location of that target symbol. There was no evidence of failure to randomise the symbol choice, and no evidence of nonrandom placement of columns; Computer target selection (1st RUN): $\chi^2(49, N = 131) = 43.71$, $p = .687$; Computer target selection (2nd RUN): $\chi^2(49, N = 37) = 44.21$, $p = .667$; column location (1st RUN): $\chi^2(49, N = 131) = 48.24$, $p = .504$; column location (2nd RUN): $\chi^2(49, N = 37) = 54.48$, $p = .274$.

It was also considered advisable to test whether all card selections by participants were random. Therefore, two tests on the randomicity of symbol selections were conducted, again using Friedman's ANOVA test for related-samples. There was no evidence that participants choice of symbol was nonrandom. Symbol choice (1st RUN): $\chi^2(49, N = 131) = 61.39$, $p = .110$; Symbol choice (2nd RUN): $\chi^2(49, N = 37) = 50.28$, $p = .423$.

There were a total of 131 participants in the sample. As determined by the computer program, participants whose total score on the ASGS was below the median (determined from previous data[70] to be less than 17 out of a possible 36) were classified as skeptics, while those equal to, or above 17, were classified as believers. There were 87 believers (66%; $M_{ASGS} = 24.84$, $SD = 4.94$) and 44 skeptics (34%; $M_{ASGS} = 9.18$, $SD = 4.98$). A chi-square test showed this difference to be significant, $\chi^2(1, N = 131) = 14.12$, $p < .001$, two-sided.

[70] The median score of 17 was calculated from data used in an unpublished study by Dr. M. A. Thalbourne called "Transliminality and the Oxford-Liverpool Inventory of Feelings and Experiences," which used the ASGS ($N = 55$).

Of the 44 skeptics, 24 were naïve, and 20 were sophisticated.[71] Sophisticated skeptics were asked two questions (see *8.3.2*, [iii] and [iv]). Table 8.1 shows the frequency of responses to these questions. As Table 8.1 also shows, the majority of sophisticated skeptics had no problem understanding the principle of 'significance', but the total pool of sophisticated skeptics split into two basic types when questioned on belief in ESP: (i) converted skeptics (n = 10),[72] and (ii) entrenched skeptics (n = 5), as has been described in *8.2.1*.

Table 8.1
Frequency of Responses by Sophisticated Skeptics to the Two Questions

Question	Yes	No	Total
Before you started the SECOND run of 50 trials, did you understand that if your FIRST-RUN score was *way below* or *way above* chance, it might reach statistical significance?	19	1	20
Do you believe that a score, which deviates significantly from chance, might indicate the presence of ESP?	10	5	15[a]

Notes: [a] 5 participants did not respond

8.4.2 The Australian Sheep-Goat Scale

In this sample, the ASGS had a high reliability coefficient: Cronbach's alpha = 0.91. Minimum and maximum scores on the ASGS are 0 and 36, respectively, and the observed range was almost the same (viz., 0 and 35; N = 131). The mean ASGS was 19.58 (SD = 8.91). The distribution of ASGS scores is shown in Figure 8.3.

The distribution shows a number of deviations above the norm in scores ranging from 20 to 33, and some deviations above the norm in scores ranging from zero to 8. There is also a deviation below the norm in scores ranging from 9 to 18. These deviations may be attributable to the nature of the experiment, and the ethical demands on the experimenter. On the one hand, the experiment was designed specifically for skeptics, and therefore attracted a larger number of low ASGS scorers than might be expected. On the other hand, and rather ironically, the experimenter could not turn away from the experiment believers

[71] Seven sophisticated skeptics never actually received a second run of 50 trials. It is, therefore, appropriate to include the data from these 7 with the data for the 24 naïve participants, bringing the total of naïve skeptics to 31. For the purposes of the analyses in *8.4.4.1*, the total of sophisticated skeptics is taken as 13 (not 20).

[72] Due to computer error, 2 converted skeptics did not get the chance to do the 2nd RUN, so for the purposes of analyses in *8.4.3*, there are only 8 converted skeptics in total.

interested in the paranormal. Hence the experiment may have attracted a larger than usual number of high scorers on the ASGS. These two facts may help explain the deficit of 'in-between' scorers whom Lawrence (1993) referred to as "conflicts" or "undecideds" (p. 76).

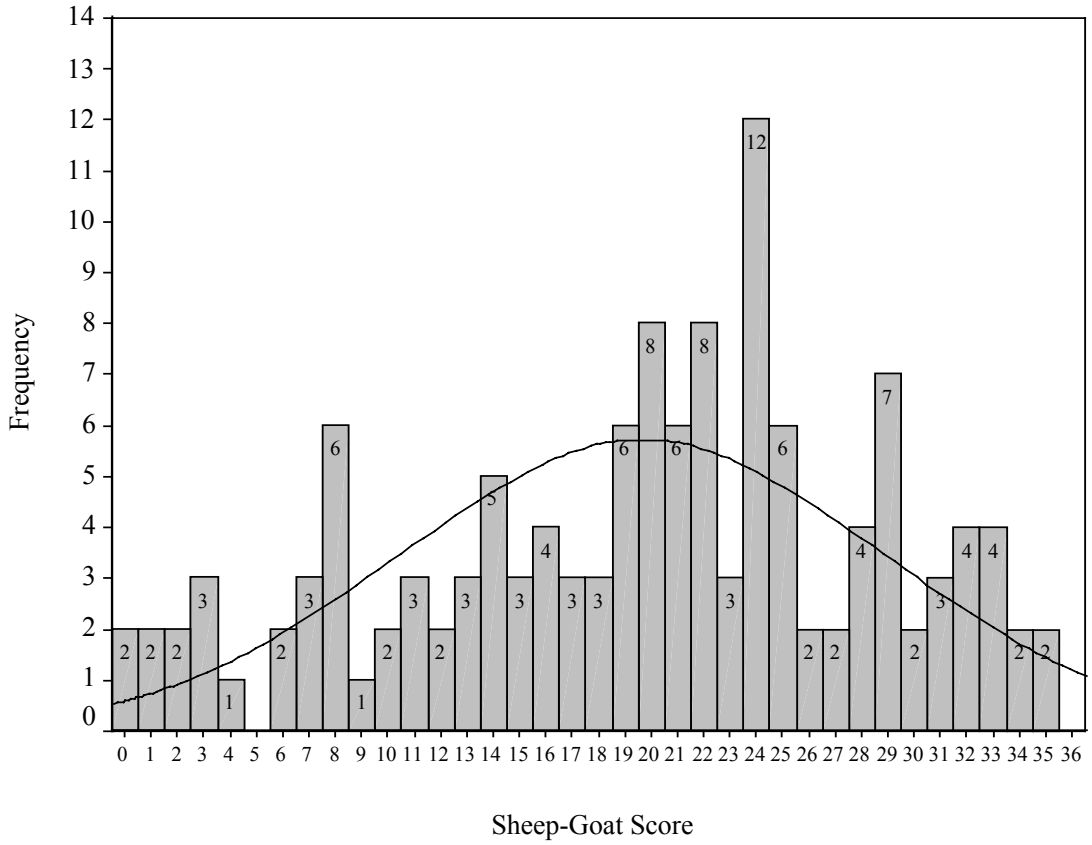

Figure 8.3. Distribution of sheep-goat scores (N = 131).

The skewness of the distribution was –0.380 (SE = 0.21). A test of the skewness[73] showed that it was not significant.

8.4.3 Hypotheses

Hypothesis 1: The mean score for symbol hitting for converted skeptics is (a) below chance in the first run (MCE = 10 correct symbols), (b) above chance in the second run, and (c) lower in the first run compared to the second run. In the first run, the mean score for converted skeptics (n = 8) was not below chance, M = 10.13 (SD = 2.42). However, in the

[73] See Chapter 5, p. 105, n53, for the formula for this calculation.

second run the mean score *was* above chance, $M = 11.13$ ($SD = 3.52$), but not significantly, $t(4) = 0.90$, $p = .198$, one-tailed. Although the mean score *was* lower in the first run compared to the second run, the difference between the two mean scores was not significant, $z = -0.07$, $p = .472$, one-tailed. The three-part hypothesis was not supported, but the hit-rate in (b) was in the right direction.

Hypothesis 2: The mean theoretical variance of symbol hitting for converted skeptics is smaller in the second run than in the first run. It is expected that 2nd RUN scores will be closer to chance, resulting in a smaller variance compared to the variance based on 1st RUN data. In the first run, the mean theoretical variance (MTV) was 5.13 ($SD = 8.58$), but in the second run the MTV was unexpectedly higher at 12.13 ($SD = 17.03$). (The difference was not significant, $z = -1.06$, $p = .291$, two-tailed.) Participants were not scoring closer to chance in the 2nd RUN. The hypothesis was not supported.

Hypothesis 3: The mean score for symbol hitting for entrenched skeptics is (a) below chance in the first run (MCE = 10 correct symbols), and (b) below chance in the second run. In the first run, the mean score for entrenched skeptics ($n = 5$) *was* below chance, $M = 9.80$ ($SD = 1.79$), but it was not significant, $t(4) = -0.25$, $p = .408$, one-tailed. In the second run, the mean score was above chance, $M = 10.60$ ($SD = 2.30$). As expected, the two-part hypothesis was not supported. Since ESP scores can be expected to be low in paranormal experimentation, and given that the sub-sample of entrenched skeptics is very low, it was expected that the difference between the two mean scores would not be significant, $z = -0.73$, $p = .465$, two-tailed. The results are consistent with the expectation that the Null hypothesis would not be rejected (see *8.2.1*).

Hypothesis 4: The mean theoretical variance of symbol hitting for entrenched skeptics is different between the two runs. In the first run, the MTV was 2.60 ($SD = 3.98$), but in the second run the MTV was higher at 4.60 ($SD = 4.28$). The difference was not significant, $z = -1.60$, $p = .109$, two-tailed. As expected, the Null hypothesis was not rejected (see *8.2.1*). The participants were not scoring at chance in the 2nd RUN.

Hypothesis 5: There is a positive relationship between ASGS scores and symbol hitting for believers and converted skeptics (2nd RUN only). There was a negative and extremely weak correlation between ASGS scores and symbol hitting for believers, but the correlation between ASGS scores and symbol hitting for converted skeptics was positive, moderate in strength, and significant, $r(6) = 0.63$, $p = .049$. The hypothesis was supported for converted

skeptics. Conversion, as the word suggests, appears to have made believers out of some skeptics, which accounts for a correlation usually expected of believers. The negative correlation for believers may be a chance result.

8.4.4 Post Hoc Analyses

8.4.4.1 Effect sizes—believers and skeptics: The Skeptics experiment is a typical forced-choice experiment, of which the effect sizes are generally low compared to other domains (i.e., for the forced-choice domain, *ES* = 0.012—see Chapter 3, Table 3.2). Given the small sample size, and the fact that the sample of believers and skeptics did not distribute normally (see Figure 8.3), it was not expected that the proportion of symbols correctly identified by the whole sample (N = 131) would vary significantly from chance (*MCE* = 0.20). Therefore, hit-rates for various groups and subgroups were calculated.

Table 8.2 gives the hit-rates, trial-based z scores, and effect sizes for four groups (viz., believers, naïve skeptics, converted skeptics, and entrenched skeptics), and subgroups thereof. As can also be seen from Table 7.2, the *ES* improved for converted skeptics from the 1st to the 2nd run, which was an expected trend given that they had been converted, and might therefore try to improve their scoring in the hope of avoiding psi-missing. This trend is suggestive only of a change in pro attitude since the improvement in scoring was not significant.

Table 8.2
Trials, Hit-Rates, Trial-Based Z Scores and ES Scores for Various Groups & Sub-Groups

Groups/Subgroups	Total Trials	Total Hits	Mean Hit-Rate	*z score*	*ES* [a]
1. Believers (n = 87; 50 trials)	4350	889	10.22	0.70	.011
2. Naïve Skeptics (n = 24; 1st & 2nd RUNS: 100 trials)	2400	488	20.33	0.40	.008
3. Converted Skeptics (n = 8; 1st RUN: 50 trials)	400	81	10.13	0.06	.003
4. Converted Skeptics (n = 8; 2nd RUN: 50 trials)	400	89	11.13	1.06	.053
5. Converted Skeptics (n = 8; 1st & 2nd RUNS: 100 trials)	800	170	21.25	0.83	.029
6. Entrenched Skeptics (n = 5; 1st RUN: 50 trials)	250	49	9.80	–0.08	–.005
7. Entrenched Skeptics (n = 5; 2nd RUN: 50 trials)	250	53	10.60	0.39	.025
8. Entrenched Skeptics (n = 5; 1st & 2nd RUNS: 100 trials)	500	102	20.40	0.17	.008

Notes: [a] The estimate of effect size $z/n^{½}$ is used here, where z scores are 'exact'

The *ES* also improved for entrenched skeptics (from a negative to a positive effect size), which was *not* expected given that they should not have attempted to improve if their pro attitudes remained fixed in the second run. However, the change is not significant (see *8.4.*3, Hypothesis 3), but it is possible that some number of entrenched skeptics may have entertained doubts about their fixed beliefs!

8.4.4.2 Replication of effect sizes: While Table 8.2 shows that effect sizes are extremely low, they may not vary significantly from the significant mean *ES* norm of 0.012 for the forced-choice domain. Using the single-sample *t*-test, two performance comparisons were made between the eight *ES* values listed in Table 8.2 and (i) the mean *ES* of 0.015 for the nine groups/subgroups as the test statistic, and (ii) the mean *ES* norm of 0.012 for the forced-choice domain as the test statistic (Honorton & Ferrari, 1989—see Chapter 3, Table 3.2). The *t* values in both cases were not significant, (i) $t(7) = 0.23$, $p = .825$, two-tailed; (ii) $t(7) = 0.69$, $p = .512$, two-tailed. Thus, the eight *ES* values comprise a homogeneous data set comparable in performance to that of the forced-choice domain.

8.4.4.3 The Sheep-Goat effect: As was indicated in the literature review above (see *8.1.1*), a differential effect can usually be expected in paranormal experiments, where sheep tend to psi-hit and goats tend to psi-miss. The Pearson test showed no significant relationship between ASGS scores and hit-rates, $r(129) = 0.05$, $p = .281$, one-tailed. However, when believers ($n_1 = 87$) and skeptics ($n_2 = 44$) were looked at separately using the one-sample Kolmogorov-Smirnov test on the normality of the distributions of hits for these two groups, there was a marginally significant deviation from a normal distribution of hits for believers, $z = 1.17$, $p = .065$, one-tailed, but for skeptics there was no significant deviation from a normal distribution, $z = 0.91$, $p = .194$, one-tailed. Figure 8.4 illustrates these effects quite dramatically.

In Figure 8.4a, there is a marginally significant trend for believers to deviate in their scoring behaviour *away* from a normal distribution *in the direction of* psi-hitting *and*, uncharacteristically, psi-missing, but mainly towards hitting. Two participants were psi-hitters with significant scores of 16 and 18, respectively, but note also (i) the above average number of scores of 15 by six participants, which contributed to the significant trend, and (ii) the deviation away from scoring exactly at *MCE*. Slightly more than half (52%) of believers scored above *MCE*.

In Figure 8.4b, there is no significant trend in scoring for skeptics—only a preponderance of scoring exactly at *MCE* (a kind of reversal of effect when compared to Figure 8.4a). Slightly more than half (53%) of skeptics scored below *MCE*. If the sheep-goat effect has manifested in these data, it only comes as faintly suggestive trends in the expected directions.

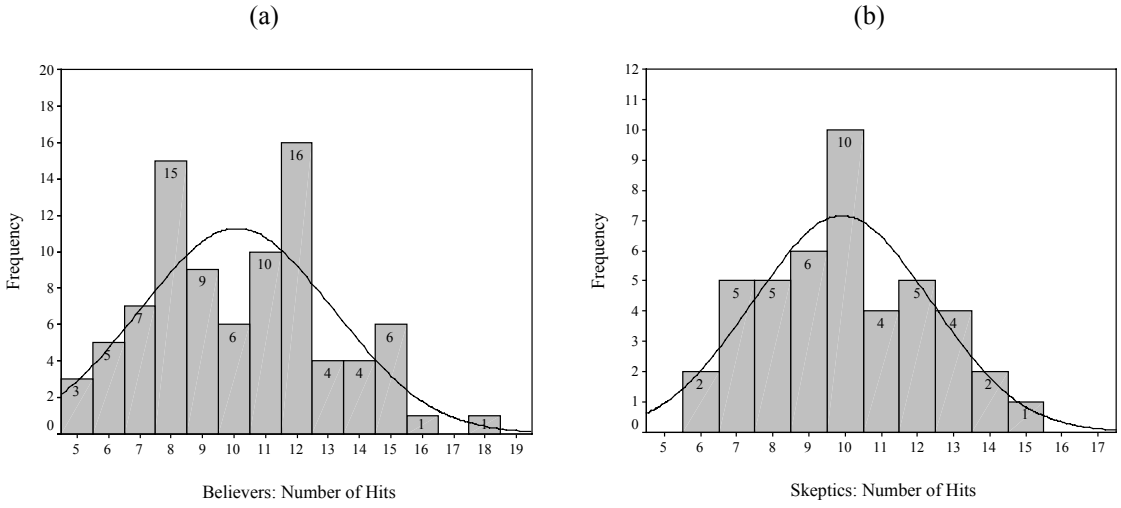

Figure 8.4. (a) For believers, scoring deviated significantly from a normal distribution, which included a prominent deficit in scores at *MCE* = 10. (b) For skeptics, scoring followed a normal distribution, although scoring at *MCE* was very high.

8.4.4.4 High ASGS scores as a necessary condition: For converted skeptics, there was a positive relationship between ASGS scores and symbol hitting (see *8.4.3*, Hypothesis 5). A median-split analysis was performed on ASGS scores to determine whether relatively high ASGS scores might be a necessary condition for successful symbol-guessing. The median score on the ASGS for converts is 10.50. Note that 'high' scores in the case of converts (> 10.50) are still below the median score for the whole sample, viz., 17, because converts are (were?) skeptics, after all. High-scoring converts, therefore, would more accurately be described as 'undecideds' or 'conflicts', to use Lawrence's (1993, p. 76) terminology. Nevertheless, the theoretical relationship that low ASGS-scorers tend to psi-miss, while high ASGS-scorers tend to psi-hit, should still hold true. And, of course, we do not know what converts' post-2nd RUN mean ASGS score would be because they were not re-tested.

There are only 8 converted skeptics—four were 'high'-scorers (> 10.50), and four were 'low'-scorers (< 10.5). High scorers produced a significant hit-rate (2nd RUN) of $P_{obs.}$ = 0.30 (p = .047; where P_{MCE} = 0.20), whereas low-scorers hit-rate was nonsignificant,

$P_{obs.}$ = 0.20 (p = .465). Results show that 'high' scoring on the ASGS was necessary in order to elicit psi-hitting in the Skeptics experiment. It is of interest to note that before converts were converted (1st RUN), their mean hit-rate was at chance, $P_{obs.}$ = 0.20 (p = .465). It appears that new belief can have a measurable effect. This finding, albeit post hoc, supports the major postulate of the present study: that primary pro attitudes can be changed.

8.5 Discussion

The Skeptics experiment was conducted in order to test the concept of the *primary* pro attitude. Having found, in the *I Ching* experiment, evidence that *implied* the existence of a pro attitude towards a specific outcome on the part of sufficient numbers of participants (see *5.8.1.3*), the next phase of the investigation into the concept of the pro attitude required that evidence be found that would *confirm* the existence of the pro attitude, by confirming its mutability.

That phase was launched with the running of the experiment described in this chapter, which was primarily aimed at skeptics, since common sense would dictate that their pro attitudes could be regarded as *at least* gravitating towards some form of target-avoidance, based on doubt, or even denial, of the psi hypothesis. Given such an orientation, it was deemed possible that skeptics' *primary* pro attitudes could be changed if sufficient cause was given, and these changes would be indicated by changes in scores across runs and/or changes in the mean theoretical variances (MTV) of scores across runs. Thus, the Skeptics experiment differed from alternative forms of testing changes in targeting by focusing on primary pro attitudes; the disposition of participants (i.e., the reason they perform as they do in psi tasks), rather than focusing on a change in *secondary* pro attitudes which merely tests the ability to shift focus from one nominal target to another.

The results of the experiment are inconclusive, but it must be stated that this experiment was only a pilot study. Computer program failure, and the inability to acquire sufficient numbers of skeptics, put the experimenter at a disadvantage, although the results are not altogether uninterpretable. Thus, two reasonably fair conclusions can be made regarding the Skeptics experiment:

1. For converts—even though scoring was higher in the 2nd than in the 1st run, it cannot be said whether sufficient numbers of converted skeptics' pro attitudes did

change in the 2nd run *to a degree of conviction and psi conducive strength* that would have elicited a statistically significant movement towards psi-hitting had the number of converts been greater. In fact, the substantial, but nonsignificant, increase in MTV from the 1st to 2nd run (possibly indicating that scoring by converts became less consistent) suggests that a trend towards psi-hitting would have been unlikely, even with an increase in the number of converts, given that the MTV trend might be a consistent effect for converts due to the cognitive upheaval inherent in adopting a new belief structure.

2. For entrenched skeptics—even though scoring improved from the 1st to the 2nd run, the nonsignificant result suggests that sufficient numbers of entrenched skeptics' pro attitudes may have stayed the same.

In the former case (converts), only further testing will validate this conclusion. In the latter case (entrenched skeptics), replication is needed before strong conclusions can be made about the fixity of the pro attitude across time.

Overall, given the small sub-sample of skeptics, there have been some encouraging replications and results in this experiment, which, however, must be regarded only as a template only for a future study.

Regarding the ASGS scale as a possible predictor of paranormal performance, there was only one significant correlation out of two tests (see *8.4.3*, Hypothesis 5). This significant correlation was not for believers, as might be expected, but was for converted skeptics. It is possible that believers did so poorly because their anticipation and self-expectation of a good performance disrupted their focus. Unfortunately they were not given a second run to redeem themselves. Converted skeptics, on the other hand, may have been spurred on by their newfound belief, and had a second chance to prove themselves. Thus, they showed the greatest improvement in scores, reaching the highest *ES* of .053 in their 2nd run (see Table 8.2). In fact, for converted skeptics, there was evidence that high scoring on the ASGS was a necessary condition for eliciting paranormal effects. Low-scoring converts elicited a mean score exactly at chance, as did converts prior to conversion (see *8.4.4.4*). This finding supports the hypothesis that primary pro attitudes can change.

An important final note regarding the methodology of the experiment concerns Thalbourne's (2000a, p. 60) original suggestion of deceiving all skeptics after the 1st run into thinking that they had attained significantly low scores (i.e., psi-missing) in order to create an even greater level of cognitive dissonance amongst skeptics than that elicited in

skeptics in the present experiment. The present author's original ethics proposal followed Thalbourne's design idea, since it gave promise of greater endeavours on the part of sophisticated skeptics to perform at chance, with reduced MTV in the 2nd run compared to the 1st, but the Psychology Department ethics subcommittee of Adelaide University insisted that deliberate attempts to lie to participants must be regarded as unethical—hence the compromised, less potent design described and used in this study (see *8.2.1*).

Aside from a number of relevant investigations of the theory of psychopraxia, the previous four chapters (Chapters 5, 6, 7, and 8) reported mainly in depth investigations of the concept of the pro attitude. Thus, having spent considerable time on that investigation, the study in the next chapter investigated the issue of compensation in terms of the diasomatic hypothesis. According to the theory of psychopraxia, deficits in the endosomatic domain not only result in compensation within the endosomatic domain, but may also result in compensation in the exosomatic domain (i.e., paranormal effects). Specifically, vision-impaired participants were compared with sighted participants in a free-response picture-identification task in order to determine if paranormal compensation can occur.

CHAPTER 9
PARANORMAL EFFECTS USING SIGHTED AND VISION-IMPAIRED PARTICIPANTS IN A QUASI-GANZFELD TASK[74]

The sensory world of the blind in fact appears to be very rich in meaning.
Allan Paivio (1971, p. 520)

The blind do not have the same knowledge of how vision works as do their sighted peers.
Ann E. Bigelow (1992, p. 184)

9.1 The Nature of Exosomatic Psychopraxia

In Chapter 4 (see **4.5**), the diasomatic hypothesis was described, and exo-psychopraxia was seen as acting either in a compensatory way (substitution mode) or as a 'spill-over' effect. In this chapter, exo-psychopraxia acting as *compensation* for some temporary or permanent ostensibly 'adverse condition' in the test participant is investigated. The particular form of adverse condition tested in this study is vision-impairment. This experiment is the first of its kind in which a free-response task was administered to vision-impaired participants rather than a forced-choice (card-guessing) task.

In addition, the conditions of relaxation and belief in paranormal processes, hypothesized as conditions necessary for bringing about exo-psychopraxia, were also investigated. Thus, the aims of the present study were:

1. To determine whether there was a significant difference in regard to paranormal performance between vision-impaired participants and sighted participants.

2. To determine whether relaxation was conducive to, and/or necessary for, paranormal performance.

[74] This chapter was published as a refereed article (see Appendix AG).

3. To determine whether belief in paranormal processes was conducive to, and/or necessary for, paranormal performance.

It might be argued that in certain paranormal tasks, where visual acuity could be regarded as an advantage, a deficit in vision may work against vision-impaired participants. If so, should the vision-impaired perform poorly (compared to sighted participants) on a given paranormal task, an artefact of vision-impairment rather than lack of paranormal ability may be said to have occurred, but this conclusion may be wrong. The next section is an attempt to 'level the playing field', as it were, for the sighted and the vision-impaired alike, by addressing this issue in order to determine how valid such an argument may be.

9.1.1 Experimentation with the Vision-Impaired

It is a commonly held belief that totally or partially blind[75] individuals must in some way be limited in their capacity to form visual images in their minds. Early research on imagery deficits associated with congenitally blind individuals was conducted by Schlaegel (1953), who tested sighted and blind participants on their capacity to form images in response to key words (nouns), either visual (e.g., *painting*, *sky*, *sunset*), or auditory (e.g., *laughter*, *prayer*, *echo*) in reference. Schlaegel found that the imagery of vision-impaired participants was significantly affected by two factors: (a) "present visual acuity—those subjects with the poorest vision had the least number of visual, and the greatest number of auditory, responses," and (b) "age of onset of incapacitating loss of vision—if the onset was before the age of six, visual imagery tended to disappear, the tendency being most pronounced in those subjects with the poorest vision" (p. 265).

Schlaegel (1953) also found that as visual acuity in the vision-impaired increased, there was an average increase in visual imagery responses to an extent even greater than that of normal sighted controls. This finding suggests that vision-impaired individuals can compensate for lack of vision, thus taking them above and beyond the capacities of their fully sighted peers by combining what little vision they have with other (putatively superior) mental functions. This finding has pertinent ramifications on how well vision-

[75] The term 'partially blind' is now superseded by the more appropriate term 'vision-impaired' (P. Greco, personal communication, May 17, 1999). The latter term is used in the present thesis to describe generally any participants in the vision-impaired experiment whose levels of blindness may be partial or total (degrees of vision-impairment are assigned on the basis of legal, clinical, or even personal considerations).

impaired participants (particularly the partially blind) might be expected to perform in tasks involving paranormal picture identification.

Like Schlaegel, Paivio (1971) claimed that "purely visual words would be effectively abstract [to the blind], whereas words with referents that can be heard or felt or otherwise experienced would be the concrete words of the blind" (p. 518). To date, Paivio's research, and that of others, has furnished rather ambiguous results (Gardner & Paivio, unpublished, cited in Paivio, 1971; Paivio & Okavita, 1971; Zimler & Keenan, 1983).

Gardner and Paivio (cited in Paivio, 1971, p. 518) conducted an imagery noun-recall experiment using "blind" participants only. Participants were presented with auditory ("concrete") and visual ("abstract") words, followed by a recognition test where participants were required to select the correct response from provided alternatives after the stimulus. Nonsignificant performance differences were found between scores on the visual and the auditory tasks. Therefore, it was not demonstrated that abstract and concrete words were qualitatively and quantitatively different for the vision-impaired. Paivio and Okavita (1971) repeated the experiment, this time assuming that visual imagery was dominant and superior in sighted compared to vision-impaired participants. They used both types of participants and found a significant difference between the two on the visual task only. In this case, the vision-impaired appeared to be at a disadvantage.

However, Zimler and Keenan (1983) showed that in a similar task, recall by totally blind participants of words high on visual imagery was "remarkably similar to the sighted" (p. 269), suggesting that either (a) "the visual imagery used by the sighted is no more facilitating than the abstract semantic representations [ASR] used by the blind, or (b) the sighted are not using visual imagery" (p. 269). Either way, if visual imagery was used by the sighted, it did not appear to give them an advantage over the blind—if not, the sighted too must have been using ASR to the same degree as the blind (assuming that ASR was the only other faculty involved for both sighted and blind participants in such tasks).

Ring and Cooper (1997) took the idea of ASR even further. They found that the majority (64%) of congenitally blind participants in their study claimed to 'see' during near-death experiences and out-of-body experiences. Ring and Cooper claimed that there were occasional reports from participants of 'visually-based' knowledge that could not have been obtained by normal means. The term 'mindsight' was coined by the authors to describe this ostensible perception of the vision-impaired that functioned like sight, involving a deep awareness and profound ability to know (see also Ring & Cooper, 1999). Christie-Murray (2000) and Parker (2000b) have commented on this alleged ability, and

both researchers hint at the implications it may have for parapsychological research in regard to the nature of paranormal processes.

When comparing psi task performances between sighted and vision-impaired, it must also be acknowledged that the world of the vision-impaired individual is unlike ours in that perception for the vision-impaired depends on sense data that we may ignore for the most part, such as tactual contacts involving air pressure and temperature changes, sound distortion (e.g., echoing or dampening effects), and changes in light intensity, as sensed by the corresponding indicators of changes in irradiated heat from certain objects (for further examples, see Brodey, 1969). Thus, at this stage, it may be too early to arrive at categorical conclusions about who is really disadvantaged in visually oriented tasks—the sighted or the vision-impaired. Nevertheless, the limited evidence so far indicates that it is reasonable to compare both types of participant if the task can be judged as showing no sign of bias for or against either type. In parapsychology, many paranormal tasks are, for the most part, free from bias that might compromise the performance of the vision-impaired. After all, success at paranormal tasks is often shown not to depend on vision at all (for example, consider the ganzfeld percipient who is effectively 'blind' during the experiment—see *2.3.1*).

Only if it can be argued that certain spatial skills and/or manipulation of target pictures allow for concept formation that may lead to a greater capacity to form imagery with 'knock-on' effects (i.e., concomitant repercussions) conducive to paranormal performance could it then be claimed that sighted participants might have an advantage over their non-sighted cohorts. But again, as was shown above, where sight might seem to be an advantage, the vision-impaired and even the congenitally blind do not seem to be challenged to the degree that we may have come to believe.

9.1.2 Paranormal Experimentation with the Vision-Impaired

Alvarado (1988) reports that blindness, imagery and paranormal performance have been topics of interest as far back as 1891. He refers to F. W. H. Myers (1891), who was one of the first psychical researchers to conjecture that paranormal ability might manifest in the blind, suggesting that the blind person "will exercise a sight, *which he* [sic] *does not recognize as sight*, which belongs in fact to that pre-natal undifferentiated *continuum* of perceptive faculty of which telepathic and clairvoyant phenomena show us the vestigial or obsolescent trace" (p. 127). Myers (1891) thought it possible that clairvoyance, in particular, might manifest through "novel channels where specialized [i.e., auditory and tactile] sense is in default" (p. 126).

Decades later, Price and Pegram (1937) were the first to investigate paranormal performance in the vision-impaired (they referred to their participants as "blind," but their sample included partially impaired individuals). Sixty-six participants were given Zener card-calling tests with 25 "calls" per run, using three matching techniques: Open Matching (cards in the pack are face up, and the participant is told the order of the cards in the pack, each of which must be placed against 1 of 5 unidentified key cards), Blind Matching (same as Open Matching, but cards in the pack are face down, and the participant is not told the order of the cards in the pack), and Match Piling (the participant divides the 25 cards into 5 piles, and names the piles, e.g., "this is the circle pile, that is the star pile," etc.). The number of runs per participant ranged from 3 to 86. There was a significant deviation from chance, with forty-four percent of the sample producing individually significant scores. Price and Pegram found that age and extent of impairment did not make significant differences to scoring trends. Even more surprising was the fact that scores actually improved when cards from the pack were placed in opaque sealed envelopes!

Price and Pegram (1937) recognized the problem of testing a specific group without using a control group for comparative purposes (i.e., they did not test sighted people). They concluded that the "high proportion [of hits] cannot be considered significantly unusual until further research has been done with non-blind groups of comparable age and grade under similar social conditions" (p. 153). Consequently, in a follow-up study, Price (1938) tested vision-impaired and sighted participants so that performance comparisons could be made. The two groups "were selected for similarity on age and institutional status" (p. 286). Open Matching and Blind Matching methods were primarily used, as in the Price and Pegram (1937) study. When both groups were combined, overall scoring was significantly above chance. The vision-impaired produced higher average scores than the sighted, but the differences were not significant. When cards were sealed in opaque envelopes, scoring was significantly higher for both groups compared to the 'open-card' method. Price concluded that "something meaningful" (i.e., "extra-sensory perception") took place in the tests, and she dismissed the rival hypotheses of sensory leakage that might explain the effects, as was shown by the fact that "subjects scored better in tests with enclosed cards than with the open cards" (p. 282).

Gonzales-Scarano (1982) looked at paranormal task performance in the sighted and the vision-impaired. She (after Paivio, 1971) assumed that individuals who suffered a sensory deficit in one modality would be compensated by other unimpaired modalities. On this basis, Gonzales-Scarano theorized that unconscious memory images (visual, auditory,

etc.) would be activated into consciousness by noncognitive factors—even paranormal processes might be activated by these factors.

Gonzales-Scarano (1982) used a visual memory test similar to that of Paivio and Okovita (1971). High-visual/low-auditory word-pairs and high-auditory/low-visual word pairs were randomized and presented verbally to the participants. The paranormal component of the test involved the identification of pre-designated specific word targets. The experimenter was 'blind' to the identity of the targets. Two psi hypotheses were proposed: (i) that the sighted would recall more 'high-visual concrete nouns' than the congenitally totally blind, and (ii) that the congenitally totally blind would recall more 'high-auditory concrete nouns' than the sighted. Both hypotheses failed to be confirmed. These results suggest that the totally blind participants were no more advantaged or disadvantaged than the sighted.

Like Price (1938), Barnard and Nelson (1983) also ran a card-matching task using 10 sighted and 10 "nonsighted" participants. They hypothesized that the nonsighted would perform significantly better than the sighted, especially when the nonsighted were allowed to "touch" the cards (i.e., "directly handle the cards" p. 58). There were therefore four groups, but no significant main effect, or interaction effect, was found. However, a significant variance difference was found (as an *F max* ratio of the variances) between the sighted and nonsighted groups. Also, in a post hoc *t*-test comparing overall group scores to chance, significant "psi-hitting" was found for the "nonsighted" (p. 59). The combined samples of 20 participants also produced a significant hit-rate, but only in the 'touch' condition.

Overall, the few available studies do no more than suggest that paranormal performances of the vision-impaired may be superior to those of the sighted. Perhaps, more importantly, Gonzales-Scarano's (1982) negative findings suggest that the vision-impaired and the sighted should not be treated as incommensurable groups when given tasks that test their capacities to form images in their minds. There may be no good reason not to compare the vision-impaired and the sighted.

Price and Pegram's (1937) hypothesis, that "the blind may be better at ESP than are the sighted," has been reiterated by Thalbourne (2000a, p. 50). The experiment described in this chapter tested Thalbourne's compensation hypothesis.

9.1.3 Relaxation as a Moderator of Paranormal Performance

Considerable research has been done on the ostensible effects of relaxation as a modifying variable of paranormal performance. Smith and Gibson (1941) were amongst

the first to apply a systematic investigation of factors that "inhibit or facilitate performance of subjects in ESP tests" (p. 58), and in so doing, identified relaxation as one of many facilitators of ESP. Rhine (1946) extended Smith and Gibson's finding by showing that participants' PK results were significantly improved when they were hypnotized and instructed to act "in a spirit of fun and relaxation" rather than "try very hard" (p. 126).

Ultimately, Braud (1975), having identified relaxation as an epiphenomenon of hypnosis, also noted that it was conducive to "good psi performance" (p. 142). He found, (a) relaxation of the "skeletal musculature," (p. 143) and, more generally, "deep mental and physical relaxation" (p. 144) could be achieved if participants listened to relaxation tapes, and (b) those participants on relaxation protocols tended to be the "good psi performers" compared to the "poor psi performers" who were not on the protocols. Relaxation inevitably became one of Braud's seven major characteristics of the "*psi-conducive syndrome*" (p. 142).

Honorton (1977) cursorily reviewed the 'relaxation' literature, and found 13 studies that used relaxation protocols to "enhance psi receptivity" (p. 457). Seven principal investigators were involved in these studies dating back to the late 1950s. There was a 77% success rate (10 out of 13 studies). However, these figures are misleading (although, not necessarily flawed) because only Schmeidler's (1952, 1957) two studies each used non-treatment (i.e., control) groups, whereas the other 11 studies used alternative designs: That is, (a) split-sample design for a two-group comparison with no control group (such as 'low'-relaxation versus 'high'-relaxation), or (b) relaxation combined with some other treatment(s) for all participants in the sample, and no control group (e.g., relaxation + hypnosis), or (c) all participants on relaxation, but no other treatment, and again, no control group.

Results from studies using experimental designs such as these do not give a clear indication of either, (a) valid performance differences that may be expected between experimental (relaxation) groups and control (non-relaxation) groups, and/or (b) the exclusive influence of relaxation above and beyond the additional influence of another treatment, a situation that may be complicated by (say) an interaction effect between relaxation and the other treatment. In order to gain a general idea of how successful the relaxation studies have been, only the studies that used a two-group design, with an experimental group on relaxation (the treatment group) and a control group, were analysed using the 'vote-counting' technique.[76] The results are presented in Table 9.1.

[76] The vote-counting technique draws "inferences about an experimental effect by counting the number of significant versus nonsignificant studies of the effect" (Utts, 1991, p. 369).

Table 9.1
Relaxation Studies Using Control (i.e., Non-Relaxation) Groups for Comparison

Year	Author(s)	Significant Group Difference	
		Yes	No
1952	Schmeidler	✓	
1957	Gerber & Schmeidler	✓	
1975	Honorton	✓	
1979	Rao (Series II) [a]		✓
1982	Debes & Morris	✓	
1984	Quider		✓
1987	Palmer and Kramer	✓	
Totals (5 + 2 = 7)		5	2

Note: [a] Rao's (1979) study yielded a significant relaxation effect in a single-participant 'pilot' study (Series I).

Table 9.1 shows that 5 out of 7 studies yielded significant differences on performance due to the exclusive effect of relaxation (i.e., a 71% success rate), with the relaxation groups yielding the better performances. However, the success rate drops dramatically if all available studies that featured relaxation in their design are counted again, with the three alternative designs mentioned above not being used as criteria for excluding them in the count. These studies may also be regarded as indicating in some way the psi-conducive effects of relaxation, albeit due to the possible influences of other treatments besides relaxation.

Table 9.2 lists all relevant and appropriate studies (see 'Note', Table 9.2, for criteria of inclusion). Twelve out of 25 studies yielded significant differences in paranormal performance between the two groups (a 48% success rate).[77] Note that Tables 9.1 and 9.2 are not comprised of ganzfeld studies—see Chapter 2 for meta-analyses of ganzfeld studies that feature relaxation as a crucial component of the design protocol. Note also, that regardless of which table one decides to take as definitive of the efficacy of

[77] As can be seen in Table 9.2, four studies—Braud and Braud (1973, 1974), Honorton and Barksdale (1972), and Rao (1979)—in this analysis were counted (twice each) as eight 'studies', thus deflating the success rates of relaxation studies. If taken as individual studies, using aggregated counts, only one successful experiment would be 'absorbed', but the overall success rate would rise to 52% (11 out of 21 studies).

Table 9.2

Relaxation Studies in Chronological Order Using Additional Treatments (includes Control and Non-Control Studies)

Year	Author(s)	Sig. Diff.		Additional Treatments (1 = Group One; 2 = Group Two; ws = whole sample)	Control Group
		Yes	No		
1946	Rhine	✓		Hypnosis (1 + 2); relax (2)	✗
1952	Schmeidler	✓		None	✓
1957	Gerber & Schmeidler	✓		None	✓
1968	Krippner		✓	Relax. (1); hypnosis (2)	✗
1970	Stanford & Lovin		✓	None	✗
1972	Honorton & Barksdale (Expt. 1)	✓		Relax. (1); tension (2)	✗
1972	Honorton & Barksdale (Expt. 2)		✓	Relax. (1); tension (2)	✗
1973	Braud & Braud (Expt. I)	✓		None	✗
1973	Braud & Braud (Expt. II)	✓		None	✗
1973	Friedman, Ganzt, & Sinclair		✓	Relax. + visualization (ws)	✗
1974	Braud & Braud (Expt. I)		✓	High relax. (1); low relax. (2)	✗
1974	Braud & Braud (Expt. II)	✓		Relax. (1); tension (2)	✗
1975	Honorton	✓		None	✓
1976	Altom & Braud		✓	None	✗
1976	Braud & Thorsrud		✓	Relax. + incubation period (ws)	✗
1976	Braud, Smith, Andrew, & Willis	✓		Relax. (1 + 2); sound FX tapes (2)	✗
1976	Miller & York		✓	High relax. (1); low relax. (2)	✗
1979	Morris & Bailey		✓	Visualization (1); concentration (2)	✗
1979	Rao (Series I)-single *S* study	✓		None	✗
1979	Rao (Series II)		✓	None	✓
1982	Debes & Morris	✓		None	✓
1984	Palmer & Kramer		✓	Relax. + drumming (ws)	✗
1984	Quider		✓	Relax. + music (1); music (2)	✓
1987	Palmer & Kramer	✓		None	✓
1992	Van der Sijde & Snel		✓	Relax. (1); tension (2)	✗
Totals (12 + 13 = 25)		12	13		7

Note: Studies excluded from the table include those where participants were merely instructed to relax, but no actual relaxation treatments were administered. Also excluded, were studies that did not use comparison groups, or studies where relaxation was regarded as *no treatment at all*. One exception was found: Honorton and Barksdale (1972) reported an uncharacteristic reversal of effect where the tension treatment caused significantly higher scoring than the relaxation treatment (this study is excluded from the table).

relaxation, the number of successful studies in either table is considerably more than might be expected by chance, using critical $\alpha = 5\%$.

In conclusion, Honorton (1977) made an important observation about relaxation that has had a resounding influence on experiments on the paranormal ever since. He described relaxation as necessarily involving the "elimination" of "somatic noise," and spoke of the need for "relative sensory deprivation" so that "psi retrieval" might be better achieved as a result of "internally-mediated mental processes" (p. 457). We can detect in these words the first hint of a new avenue of paranormal research and experimentation, viz., the ganzfeld paradigm (see Chapter 2). In this chapter, replication of the findings for relaxation (as per Table 9.2) will be sought.

9.1.4 Belief in the Paranormal

In Chapter 8, belief in the paranormal as a variable that may predict paranormal performance has already been discussed (see *8.1.1*). Of particular importance was Palmer's (1977, p. 193) statistically based finding that the sheep-goat dichotomy is probably the most reliable measure we have for determining the relationship between belief in psi and paranormal performance. In support of this claim, Lawrence's (1993) meta-analysis of sheep-goat studies spanning 47 years showed that sheep score significantly higher than goats on paranormal tasks (see *3.1.6*).

In this chapter, attempts to replicate the overall findings for the belief hypothesis were undertaken.

9.2 The Free-Response (Quasi-Ganzfeld) Experiment

9.2.1 Description of the Experiment

The experimental component of this study involved testing the ability of vision-impaired and sighted participants to rank a target picture, a copy of which was initially concealed in an envelope, amongst three other pictures (decoys). The task is essentially free-response[78] and quasi-ganzfeld,[79] as opposed to forced-choice (see *2.3.1* and *3.1.2* for definitions of free-response and forced-choice, respectively).

[78] The free-response design has some advantages over the forced-choice design. Thalbourne (1981) points out the "more life-like and more interesting" (p. 151) attributes of free-response compared to forced-choice (see also Utts, 1991, p. 368). Watson (1973, p. 254), in fact, criticized Rhine for his repeated use of the forced-choice paradigm, which was notorious for decline effects, and motivational problems, etc.

Heart rates of half the participants (alternately selected) were also taken in order to determine the effects, if any, of a relaxation tape on heart rate and paranormal performance.[80] Prior to the experiment proper, a target picture (a hand-drawn image corresponding to a randomly selected word from a dictionary) was randomly selected from four similarly derived pictures, photocopied, and then concealed in a target envelope. A gallery of 180 pictures was used in this experiment. These were hand-drawn originals by M.A.T., who first used a smaller set of 120 pictures (12 sets of 10) in his (Thalbourne, 1976) Honours thesis, and then, as the full set of 180 pictures (18 sets of 10), in his (Thalbourne, 1981) Ph.D. thesis. In the vision-impaired experiment, the 180 pictures were systematically divided into 45 sets of 4 (see Appendix AB). The four-picture set,[81] and the target picture, were each wrapped in aluminium-foil, and then concealed in their respective manila envelopes. This preparatory task was not performed by the experimenter (L.S.), nor was the experimenter present during the selection process.[82] The contents of the target envelope and the target-set envelope were never made known to the experimenter prior to the trial.

Participants were first required to describe verbally the line drawing that was concealed in aluminium-foil inside the manila envelope. Sighted participants (controls) were also tested on the same task. Participants were then required to rank the four pictures #1 to #4 using their mentation reports[83] as a guide in the ranking process (the experimenter

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[79] The term 'quasi-ganzfeld' is reasonably applied here, given the fact that relaxation and sensory homogenization treatments akin to the ganzfeld design were administered to half the participants (particularly the vision-impaired; for details, see *9.3.4*). The multiple-choice ranking protocol (by participants, or by an independent judge) typical of the ganzfeld was also incorporated into the design. However, the experimenter did not use 'senders', as is usually the case in ganzfeld experiments. Also, the "normal, waking state" (Milton, 1998, p. 31), characteristic of the consciousness of the free-response participant (as in this author's experiment), is not typical of the ganzfeld participant. Hence, the term: 'quasi-ganzfeld'.

[80] Dr. Chris Cooper, Adelaide University, gave assistance and advice in regard to the physiological aspects of the relaxation measure.

[81] The usual number of alternatives, *k*, in a ganzfeld experiment is four, and we have adhered to this convention.

[82] The selection process was conducted by L.S.'s supervisor, M.A.T.

[83] Due to the increased interest in analysis of qualitative data in the behavioural sciences, mentation reports in parapsychological research are becoming an important source of information to researchers (e.g., see Wooffitt, 1994). Thalbourne (1981) described the use of mentation reports in the near future as, possibly, a "routine requirement" in parapsychology (p. 28). These reports may hold information about psi targets identifiable only with the use of sophisticated computer programs capable of 'drawing out' distinctive patterns and concepts in a text.

gave impartial assistance in this process by describing the pictures when they could not be seen by vision-impaired participants). The ranking was performed using the preferential ranking method (Thalbourne, 1981, pp. 55-56), where the participant assigns rank #1 to the most preferred picture, rank #2 to the second most preferred picture, and so on, until all four pictures are ranked.

After testing, the mentation reports were transcribed by L.S., assessed for their accuracy by a person skilled in discourse analysis and transcription techniques, and then judged by an independent judge in order to assess the mentation reports for their possible paranormal content from a non-participant's point of view. The judge was a qualified and recognized graphic designer, who was selected for the judging task on the basis of his graphic arts skills and conceptual and topographical 'eye' for imagery. His score on the forced-choice version of the Australian Sheep-Goat Scale (ASGS) was 14, which is just below the median of 17 (Range: 36; see Chapter 7, p. 181, n70, for the source of this median score).

The analytical component of this study involved testing for overall rank-scores, testing for group differences on paranormal performance, and determining correlates, if any, between paranormal ability and the relevant variables (i.e., relaxation and belief in ESP), in accordance with the hypotheses given below (see *9.2.2* and *9.2.3*).

9.2.2 Parapsychological Hypotheses

The following parapsychological hypotheses were proposed. (The tests used are given in parentheses with each hypothesis.):

1. For all three groups; (i) the combined samples, (ii) the vision-impaired, and (iii) the sighted, the levels of scoring (expressed as sums-of-ranks scores) differ from chance (*MCE* = 2.50). (The sum of ordinal weighted ranks formula, two-tailed. Solfvin, Kelly, & Burdick, 1978, p. 99).

2. There is a difference in paranormal performance between the vision-impaired and the sighted such that mean rank scores for vision-impaired are lower (better) than the mean rank scores for the sighted. (Wilcoxon signed-ranks matched-pairs test, one-tailed.)

3. There is a difference in paranormal performance between the relaxation group and the non-relaxation group such that mean rank scores for the relaxation group are lower (better) than the mean rank scores for the non-relaxation group. (The whole

sample, the vision-impaired group, and the sighted group are to be tested.) (Mann-Whitney *U* test, one-tailed.)

4. There is a difference in paranormal performance between believers and disbelievers such that mean rank scores for the believers are lower (better) than the mean rank scores for disbelievers, based on answers to the belief in ESP question: "Do you think it is possible for at least some people to exhibit ESP under the conditions of this experiment?" (The whole sample, the vision-impaired group, and the sighted group were to be tested; Mann-Whitney *U* test, one-tailed.)

9.2.3 Psychological Hypotheses

The following psychological hypotheses were proposed. (The tests used are given with each hypothesis.):

5. Relaxation lowers the pulse (i.e., mean heart rate after the test, HR_{After}, is lower in the relaxation group compared to the non-relaxation group). (The whole sample, the vision-impaired group, and the sighted group are to be tested; Univariate analysis of variance, ANOVA, was used because the test removes the unwanted source of variance in heart rate before the test, HR_{Before}, which is the covariate—see *9.4.1*)

6. Relaxation lowers the pulse (i.e., mean HR_{After} is different between response groups, according to answers to the relaxation question; see *9.3.2*). (The whole sample, the vision-impaired group, and the sighted group are to be tested; Univariate ANOVA.)

9.3 Method

9.3.1 Participants

A total of 84 participants[84] volunteered for the experiment. There were 42 participants in the vision-impaired group and 42 participants in the sighted group, which acted as a control group so that performances between the two groups could be compared.

[84] Vision-impaired participants were acquired with the assistance of Townsend House, the Royal Society for the Blind, the Blind Welfare Association, Guide Dogs Association, and Radio Station 5RPH. Elderly sighted participants were acquired through the assistance of the Probus Organization.

Every second participant was assigned to the relaxation group. There were, thus, four groups altogether with 21 participants in each (see Table 9.3).

Table 9.3
Four Group (2 x 2) Experimental Design

Group Type	Relaxation	No Relaxation	Totals
Vision-Impaired	21	21	42
Sighted	21	21	42
Totals	42	42	84

The vision-impaired and sighted groups were matched on age and sex. Vision-impaired participants were tested first, and then sighted participants, according to their age and sex, were selected for either the relaxation group or the non-relaxation group (e.g., if a 40 year old vision-impaired male had been randomly assigned to the relaxation group, then an approximately 40 year old sighted male would be assigned to the other relaxation group, the two males constituting a matched-pair).

The mean age was 48 years (*SD* = 19.97). There were approximately equal numbers of males (52%) and females (48%). For the vision-impaired group, the average age was 49 years (*SD* = 20.38), and for the sighted group, the average age was 48 years (*SD* = 19.80).

9.3.2 Scales

Three measures were used in this experiment:

(i) Degree of vision, rated on a five-point scale, ranging from "totally blind from birth" = 1; "totally blind since [age here] year(s) old" = 2; "partially blind from birth" = 3; "partially blind since [age here] year(s) old" = 4; and "sighted" = 5 (see Appendix V).

(ii) A relaxation question, asked of all participants after the relaxation exercise or friendly conversation (see Appendix V). Participants answered the question: "To what extent do you feel relaxed?" by rating themselves on a

scale from 1 to 5, thus: "Very Unrelaxed" = 1; "Somewhat Unrelaxed" = 2; "Neutral" = 3; "Somewhat Relaxed" = 4; and "Very Relaxed" = 5.

(iii) A sheep-goat (i.e., attitude to ESP) question requiring a 'yes' or 'no' answer, asked after the experiment (but before the target picture was announced) in order to ascertain the participant's category of belief: "Do you think it is possible for at least some people to exhibit ESP under the conditions of this experiment?" (see Appendix V). This measure was used by Schmeidler (1945) in her sheep/goat experiments, and is endorsed by Lawrence (1993).

9.3.3 Apparatus

Materials/equipment used in the experiment included: (a) instructions to the participant (see Appendix W); (b) a relaxation tape (Braud's, 1986, *Balanced Body/Tranquil Mind*, Track #6: "Breathing," and Track #7: "Mental Quietude." Total running time: approximately 14 minutes)—see transcript, Appendix X); (c) stopwatch; (d) cassette tape recorder to record the mentations; (e) for each participant, a manila envelope containing a concealed drawing (target) wrapped in aluminium foil; (f) for each participant, another manila envelope containing four randomly chosen pictures (one target-picture and three decoys); (g) sheet for participant's details and score (see Appendix V); (h) discourse analyst's declaration form (an assessment of the accuracy of the mentation transcriptions; see Appendix Y); (i) instructions to the independent judge on how to rank the four pictures based on the mentation transcriptions (see Appendix Z); and (j) independent judge's declaration form (declaring that he, the judge, read all 84 transcriptions and ranked each of the four pictures himself according to his best judgment (see Appendix AA).

9.3.4 Procedure

1. **Vision-impaired participants:** Ethical approval for this experiment was given by the Psychology Department's Ethics Subcommittee. In advance of the session, a four-picture set was randomly selected from the pool of 45 sets (see *9.2.1*) using random number tables (Pagano, 1986, pp. 479-480, Table J) and, using the same random number tables, a target picture was selected. At the start of the session, a consent form was filled in, whereupon the experimenter took the pulse of the participant (HR_{Before}) using two fingers on the wrist and a stopwatch. Half the group of participants (alternately selected) listened to approximately 14 minutes of the

relaxation tape. (Note that most participants were tested in the relative silence of their own homes.) After relaxation, the 'relaxed' participants had their pulse taken again (HR_{After}). The other half (control participants) did not receive the relaxation treatment: they and the experimenter engaged in casual conversation. All participants were asked the relaxation question (see Appendix V).

2. A target envelope was then presented to, and held by, the participant. The participant attempted to describe the picture concealed in this envelope before they were allowed to know what it was. The participant's verbal responses—the mentation report—was recorded and transcribed later by L.S. for evaluation and judging purposes (see Appendix AC).

3. Four pictures (one target plus three decoys), similarly sealed in aluminium foil, were then removed from another envelope (i.e., the four-picture set described above—see **9.2**), and were then described/shown to the participant who ranked (as '#1') the picture that they believed best corresponded to their previous descriptions. The second most preferred picture was ranked as '#2', and so on, until all four pictures had been ranked. These rankings were recorded on the participant's details-and-score sheet, which was signed by the participant at the end of the test (Appendix V). Only then was the target picture removed from its envelope so that its rank score could be determined. The rank score given to just that picture was also recorded on the score sheet as the target rank score. (Note that no target set was selected more than 4 times. Four sets were never used—#26, #27, #31, and #44—although the selection procedure was entirely random.)

4. All the above procedures (1) to (3) were conducted afterwards for all sighted participants (note that all sighted participants were tested in the relative silence of a laboratory room).

5. The transcriptions of the mentation reports were assessed for accuracy by a discourse analyst (see Appendix Y). They were then ranked by an independent judge (see Appendix Z and AA), and these ranks were used for further analysis (see *9.4.3*).

9.4 Results

9.4.1 Effects of the Relaxation Exercise on Heart Rate

All participants in the experiment (vision-impaired and sighted) read and signed the consent form, and had their personal details recorded. Participants' pulses were then taken, and these were also recorded. Therefore participants were already seated and settled down for the few minutes that it took to prepare the paperwork. Table 9.4 shows means and standard deviations for HR_{Before} and HR_{After} for all 84 participants.

Table 9.4
Treatment, Pulse (HR_{Before} *and* HR_{After}*), and Heart Rate Difference (*$HR_{Diff.}$*) in Beats/Min.*

Treatment	HR_{Before}	*SD*	HR_{After}	*SD*	$HR_{Diff.}$	*SD*
No Relaxation	76.81	10.38	75.00	9.58	–1.81	4.73
Relaxation	74.71	9.91	71.95	9.68	–2.76	3.77
Mean Diff.	2.10	12.79	3.05	12.39	0.95	5.56

As can be seen from the Table 9.4, those participants who were on the relaxation treatment were already 2.10 beats/minute (bpm) slower than those participants not on relaxation. This difference actually translates as a bias *against* the relaxation group because the mean $HR_{Diff.}$ for the relaxation group might have been larger had they started at the same higher HR_{Before} of the non-relaxation group (76.81 bpm). Thus, although the relaxation group's $HR_{Diff.}$ was almost one full beat slower than that of the non-relaxation group, the efficacy of the relaxation technique may have been undermined somewhat by this unavoidable circumstance. To control for the covariate (HR_{Before}), univariate ANCOVA was used to test Hypotheses 5 and 6.

9.4.2 Results based on Participants' Rank Scores

Hypothesis 1A[85]*: For all three groups; (i) the combined samples, (ii) the vision-impaired, and (iii) the sighted, the levels of scoring (expressed as sums-of-ranks scores) differ from chance (MCE = 2.50).* Tables 9.5, 9.6, and 9.7 show the frequencies of rank scores for the whole sample (N = 84), the vision-impaired group (n = 42), and the sighted group (n = 42), respectively.

[85] The initial 'A' refers to hypotheses that were tested using participant data, and later, the initial 'B' refers to hypotheses that were tested using independent judge data.

Table 9.5
Participants' Rank Scores: Whole Sample (N = 84)

Rank Score	Frequency	%
1	22	26.2
2	29	34.5
3	20	23.8
4	13	15.5
Total	84	100.0

Table 9.6
Participants' Rank Scores: Vision-Impaired Group (n = 42)

Rank Score	Frequency	%
1	8	19.1
2	14	33.3
3	11	26.2
4	9	21.4
Total	42	100.0

Table 9.7
Participants' Rank Scores: Sighted Group (n = 42)

Rank Score	Frequency	%
1	14	33.4
2	15	35.7
3	9	21.4
4	4	9.5
Total	42	100.0

The following results obtain for the whole sample, and the two groups:

(i) For the whole sample, the sum-of-ranks statistic was $z = -2.98$, $p = .002$, two-tailed. The mean rank score was 2.29, which was below (better than) chance (Effect Size,[86] $ES = 0.19$, a 'weak' effect size[87]). The hypothesis for the whole sample was supported. Scoring appeared to be anomalous.

(ii) For the vision-impaired group, the sum-of-ranks statistic was $z = 0.00$, $p = 1.00$, two-tailed. The mean rank score was 2.50 ($SD = 1.04$), which was at chance ($ES = 0.00$). The hypothesis for vision-impaired participants was not supported.

(iii) For the sighted group, the sum-of-ranks statistic was $z = -2.41$, $p = .016$, two-tailed. The mean rank score was 2.07 ($SD = 0.97$), which was below (better than) chance ($ES = 0.39$, a 'weak-to-medium' effect size). The hypothesis for the sighted participants was supported. Again, scoring appeared to be anomalous.

Hypothesis 2A: There is a difference in paranormal performance between the vision-impaired and the sighted such that mean rank scores for vision-impaired are lower (better) than the mean rank scores for the sighted. According to the theory of psychopraxia, compensation for vision-impairment would improve paranormal performance, hence the directional hypothesis. However, performance by sighted participants ($M = 2.07$, $SD = 0.97$; $ES = 0.39$) was actually superior to that of the vision-impaired participants ($M = 2.50$, $SD = 1.04$; $ES = 0.00$). Thus, there was a difference in performance between the vision-impaired and sighted participants ($z = -2.02$, $p = .044$, two-tailed), but it was in the wrong direction. The Null hypothesis was retained.

Hypothesis 3A: There is a difference in paranormal performance between the relaxation group and the non-relaxation group such that mean rank scores for the relaxation group

[86] The effect size (*ES*) measure used in this study is the one used "most often in remote viewing" (Utts, 1995, p. 293), and is calculated using the formula: $(\bar{R}_{MCE} - \bar{R}_{obt.})/[(N^2 - 1)/12]^{1/2}$, where $\bar{R}_{MCE}$ = average rank expected by chance, $\bar{R}_{obt}$ = average rank obtained, and N = the number of choices. In a four-choice design, the effect size can range theoretically between –1.34 and +1.34.

[87] An effect size of zero is consistent with chance, and *ES*'s of 0.20, 0.50, and 0.80 are, by convention, considered small, medium, and large, respectively (Utts, 1995, p. 294).

are lower (better) than the mean rank scores for the non-relaxation group. (The whole sample, the vision-impaired group, and the sighted group were each tested.)

(i) For the whole sample, performance by relaxed participants (M = 2.14, SD = 1.05; ES = 0.32) was superior to that of the non-relaxed participants (M = 2.43, SD = 0.99; ES = 0.06). However, the difference in performance between the two groups was not significant, z = –1.36, p = .087, one-tailed. Though not significant, the result was in the hypothesized direction.

(ii) For the vision-impaired participants, performance by relaxed participants (M = 2.24, SD = 1.05; ES = 0.23) was superior to that of the non-relaxed participants (M = 2.76, SD = 0.99; ES = –0.23). The difference between the two groups was significant, z = –1.62, p = .05, one-tailed. The hypothesis was supported. This result suggests that relaxation was a necessary condition for bringing about significant scoring, but only for the relaxed subgroup of vision-impaired participants.

(iii) For the sighted participants, performance by relaxed participants (M = 2.05, SD = 1.07; ES = 0.40) was superior to that of the non-relaxed participants (M = 2.10, SD = 0.89; ES = 0.36). However, the difference in performance between the two groups was not significant, z = –.37, p = .356, one-tailed. Though not significant, the result was in the hypothesized directional.

Hypothesis 4A: There is a difference in paranormal performance between believers and disbelievers such that mean rank scores for the believers are lower (better) than the mean rank scores for disbelievers, based on answers to the belief in ESP question. (The whole sample, the vision-impaired group, and the sighted group were to be tested.)

(i) For the whole sample, performance by believers (M = 2.34, SD = 1.01; ES = 0.14) was *not* superior to that of disbelievers (M = 2.00, SD = 1.08; ES = 0.45). Therefore, the Null hypothesis was not rejected.

(ii) For the vision-impaired group, performance by believers (M = 2.45, SD = 1.03; ES = 0.04) *was* superior to that of disbelievers (M = 3.00, SD = 1.16; ES = –0.45). However, there was no significant difference between the two groups on rank scores, z = –0.93, p = .175, one-tailed. Therefore, the Null hypothesis was not rejected.

(iii) For the sighted group, performance by believers (M = 2.21, SD = 0.99; ES = 0.26) was *not* superior to that of disbelievers (M = 1.56, SD = 0.73; ES = 0.84, a large effect size). Therefore, the Null hypothesis was not rejected.

Hypothesis 5: Relaxation lowers the pulse (i.e., HR_{After} *is lower in the relaxation group compared to the non-relaxation group). (The whole sample, the vision-impaired group, and the sighted group were each tested.)*

(i) For the whole sample, the mean HR_{After} for the relaxation group was a low 71.95 (SD = 9.68), whereas the mean HR_{After} for the non-relaxation group was relatively high at 75.00 (SD = 9.58). However, the mean difference (3.05) was not significant, $F(1, 81) = 1.94$, $p = .168$.

(ii) For the vision-impaired group, the mean HR_{After} for the relaxation group was a low 71.05 (SD = 7.92), whereas the mean HR_{After} for the non-relaxation group was relatively high at 74.38 (SD = 9.95). However, the mean difference (3.33) was not significant, $F(1, 39) = 0.44$, $p = .512$.

(iii) For the sighted group, the mean HR_{After} for the relaxation group was a low 72.86 (SD = 11.31), whereas the mean HR_{After} for the non-relaxation group was relatively high at 75.62 (SD = 9.39). However, the mean difference (2.76) was not significant, $F(1, 39) = 1.94$, $p = .172$.

In all three cases, the descriptive statistics differed in the hypothesized directions, but not significantly so. Thus, there is only suggestive evidence that the relaxation tape had an effect.

Hypothesis 6: Relaxation lowers the pulse (i.e., HR_{After} *is different between response groups, according to answers to the relaxation question). (The whole sample, the vision-impaired group, and the sighted group were each tested.)* Although there were 5 possible answers to the relaxation question, only 3 responses were used by all participants: 3 ('neutral'), 4 ('somewhat relaxed'), and 5 ('very relaxed'). Thus, three response groups were formed and a one-way ANCOVA was performed (HR_{Before} is the covariate).

(i) For the whole sample, the mean HR_{After} for the 'neutral' group was 75.00 (SD = 9.58), the mean HR_{After} for the 'somewhat relaxed' group was 73.50 (SD = 11.68), and the mean HR_{After} for the 'very relaxed' group was 70.55 (SD = 7.44). Although heart-rate did decline across the 3 groups, as

expected, there were no significant differences between the 3 groups, $F(2, 80) = .99$, $p = .377$.

(ii) For the vision-impaired group, the mean HR_{After} for the 'neutral' group was 74.38 (SD = 9.95), the mean HR_{After} for the 'somewhat relaxed' group was 72.67 (SD = 10.68), and the mean HR_{After} for the 'very relaxed' group was 69.83 (SD = 5.22). Although heart-rate did decline across the 3 groups, as expected, there were no significant differences between the 3 groups, $F(2, 38) = .23$, $p = .794$.

(iii) For the sighted group, the mean HR_{After} for the 'neutral' group was 75.62 (SD = 9.39), the mean HR_{After} for the 'somewhat relaxed' group was 74.18 (SD = 12.91), and the mean HR_{After} for the 'very relaxed' group was 71.40 (SD = 9.71). Although heart-rate did decline across the 3 groups, as expected, there were no significant differences between the 3 groups, $F(1, 39) = 1.13$, $p = .334$.

Participants appeared to be accurate in reporting their states of relaxation, which corresponded appropriately with lower HR_{After} scores across the three groups ('neutral', 'somewhat relaxed', 'very relaxed'). The descriptive statistics supported the directional hypothesis in all three cases, but the test results did not allow the conclusion that the outcomes were other than chance effects.

For Hypotheses 5 and 6, the test statistics did not indicate that a lowering of the pulse was due to the direct effects of the relaxation exercise.

9.4.3 Results based on the Independent Judge's Rank Scores

Hypothesis 1B: For all three groups; (i) the combined samples, (ii) the vision-impaired, and (iii) the sighted, the levels of scoring (expressed as sums-of-ranks scores) differ from chance ($MCE = 2.50$). Tables 9.8, 9.9, and 9.10 show the frequencies of the independent judge's rank scores for the whole sample, the vision-impaired group, and the sighted group, respectively.

Table 9.8
Judge's Rank Scores: Whole Sample (N = 84)

Rank Score	Frequency	%
1	17	20.2
2	20	23.8
3	24	28.6
4	23	27.4
Total	84	100.0

Table 9.9
Judge's Rank Scores: Vision-Impaired Group (n = 42)

Rank Score	Frequency	%
1	7	16.7
2	14	33.3
3	6	14.3
4	15	35.7
Total	42	100.0

Table 9.10
Judge's Rank Scores: Sighted Group (n = 42)

Rank Score	Frequency	%
1	10	23.8
2	6	14.3
3	18	42.9
4	8	19.0
Total	42	100.0

The following results obtain for the whole sample, and the two groups:

(i) For the whole sample, the sum-of-ranks statistic was $z = 1.02$, $p = .308$, two-tailed. The mean rank score was 2.63 ($SD = 1.10$; $ES = –0.12$). The hypothesis was not supported for the whole sample.

(ii) For the vision-impaired group, the sum-of-ranks statistic was $z = 1.03$, $p = .304$, two-tailed. The mean rank score was 2.69 ($SD = 1.14$; $ES = –0.17$). The hypothesis was not supported for vision-impaired participants.

(iii) For the sighted group, the sum-of-ranks statistic was $z = 0.35$, $p = .726$, two-tailed. The mean rank score was 2.57 ($SD = 1.06$; $ES = –0.06$). The hypothesis was not supported for the sighted participants.

Using the sum-of-ranks method, there was no evidence that the independent judge was clearly able to discriminate correspondences between the picture targets and the mentation reports. However, see the Post Hoc Analysis section below (*9.4.4.1* and *9.4.4.2*).

Hypothesis 2B: There is a difference in paranormal performance between the vision-impaired and the sighted such that mean rank scores for vision-impaired is lower (better) than the mean rank scores for the sighted. Performance by sighted participants ($M = 2.57$, $SD = 1.06$; $ES = –0.06$) was superior to that of the vision-impaired participants ($M = 2.69$, $SD = 1.14$; $ES = –0.17$). Thus, there was a difference in performance between the vision-impaired and sighted participants, but it was in the wrong direction. The Null hypothesis was not rejected.

Again, sighted participants performed better than the vision-impaired participants. Note that the judge was 'blind' to the identity of the mentation reports (before judging, the reports were shuffled so that vision-impaired and sighted mentation reports were randomly stacked in the pile, and not classified as either). Nevertheless, the judge seemed to find that the mentation reports of the sighted participants corresponded to the targets somewhat better than those of the vision-impaired participants, although this difference may be a chance outcome only.

Hypothesis 3B: There is a difference in paranormal performance between the relaxation group and the non-relaxation group such that mean rank scores for the relaxation group are lower (better) than the mean rank scores for the non-relaxation group. (The whole sample, the vision-impaired group, and the sighted group were each tested.)

(i) For the whole sample, performance by relaxed participants (M = 2.55, SD = 1.11; ES = –0.04) was superior to that of the non-relaxed participants (M = 2.71, SD = 1.09; ES = –0.19). However, the difference in performance between the two groups was not significant, z = –.70, p = .241, one-tailed. The directional hypothesis was not significantly supported.

(ii) For the vision-impaired participants, performance by relaxed participants (M = 2.48, SD = 1.12; ES = 0.02) was superior to that of the non-relaxed participants (M = 2.90, SD = 1.14; ES = –0.36). However, the difference in performance between the two groups was not significant, z = –1.21, p = .113, one-tailed. This result supports the directional tendency only of the hypothesis.

(iii) For the sighted participants, performance by relaxed participants (M = 2.62, SD = 1.12; ES = –0.11) was *not* superior to that of the non-relaxed participants (M = 2.52, SD = 1.03; ES = –0.02). This result does not support the significance or the directionality of the hypothesis.

In two out of three tests, there was suggestive evidence that relaxation was conducive to better performances on the paranormal task.

Hypothesis 4B: There is a difference in paranormal performance between believers and disbelievers such that mean rank scores for the believers are lower (better) than the mean rank scores for disbelievers, based on answers to the belief in ESP question. (The whole sample, the vision-impaired group, and the sighted group were to be tested.)

(i) For the whole sample, performance by believers (n = 71; M = 2.62 SD = 1.13; ES = –0.11) *was* superior to that of disbelievers (n = 13; M = 2.69, SD = 0.95; ES = –0.17). However, there was no significant difference between the two groups on rank scores, z = –0.17, p = .434, one-tailed. The Null hypothesis was not rejected.

(ii) For the vision-impaired group, performance by believers (n = 38; M = 2.63, SD = 1.15; ES = –0.12) *was* superior to that of disbelievers (n = 4; M = 3.25, SD = 0.96; ES = –0.67). However, there was no significant difference between the two groups on rank scores, z = –1.03, p = .151, one-tailed. The Null hypothesis was not rejected.

(iii) For the sighted group, performance by believers (n = 33; M = 2.61, SD = 1.12; ES = –0.10) was *not* superior to that of disbelievers (n = 9; M = 2.44, SD = 0.88; ES = 0.05). Therefore, the Null hypothesis was not rejected.

9.4.4 Post Hoc Analyses

9.4.4.1 Target ranking by participants and by the independent judge—A comparison: Figures 9.1, 9.2, and 9.3 show the expected (flat) distributions (as dotted lines) compared with the observed distributions for the whole sample, the vision-impaired group, and the sighted group, respectively. For each figure, distributions of (a) participants' rankings and (b) independent judge's rankings are illustrated.

The uniformities of distributions of target rank scores made by participants and by the independent judge were tested using one-sample Kolmogorov-Smirnov tests (two-tailed). First, in the case of participants' rank scores for the whole sample, the vision-impaired group, and the sighted group, all tests showed significant preferences for lower (better) rank scores: Whole sample, z = 2.51, p < .001; vision-impaired, z = 1.39, p = .021, and sighted, z = 2.39, p < .001. The participants' rank scores appeared to show that a sufficient number of participants appeared to be guided in their choices by something other than chance (see Figures 9.1a, 9.2a, and 9.3a).

Second, in testing the independent judge's rank scores for the whole sample, the vision-impaired group, and the sighted group, all showed significant deviations away from expected rank scores: whole sample, z = 2.51, p < .001; vision-impaired, z = 2.32, p < .001; and sighted, z = 1.78, p = .004. However, these deviations were not necessarily towards lower ranks (see Figures 9.1b, 9.2b, and 9.3b).

Some interesting and difficult to interpret patterns emerge from the independent judge's rankings. For the whole sample, there was a definite trend towards higher (worse) ranks, and this trend suggests that participants' mentation reports, taken collectively, were interpreted by the judge in a way counter to the patterns showed by the actual participants. However, in regard to the vision-impaired group's mentation reports, the independent judge showed a significant preference for rank #2 and rank #4 (see Figure 9.2b).

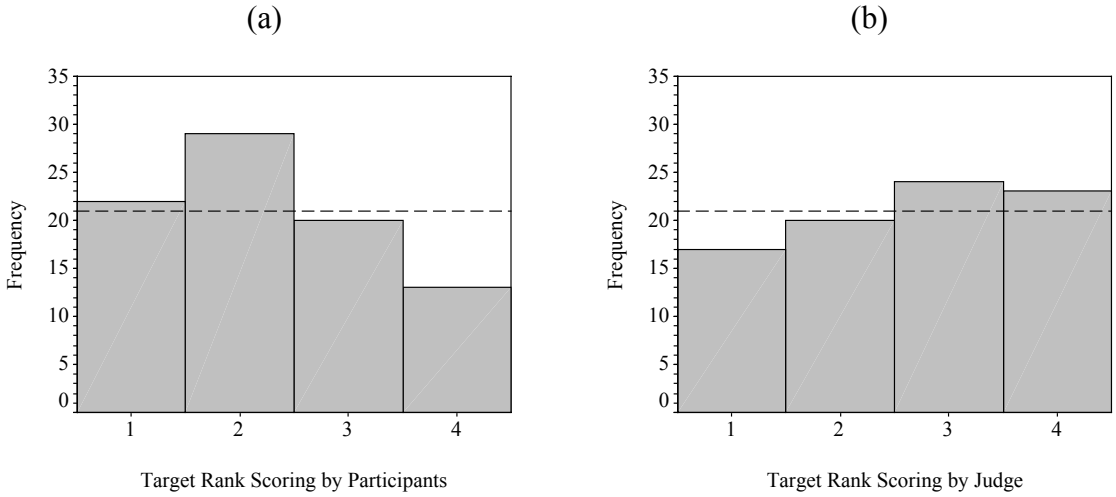

Figure 9.1. Uniformity distributions for the whole sample: (a) participants' ranks, (b) judge's ranks.

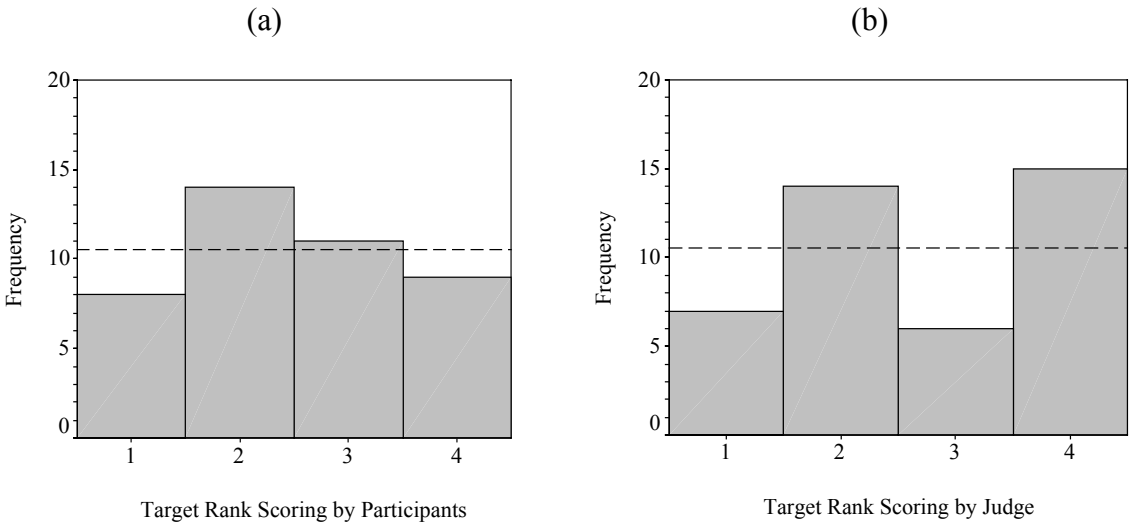

Figure 9.2. Uniformity distributions for the vision-impaired: (a) participants' ranks, (b) judge's ranks.

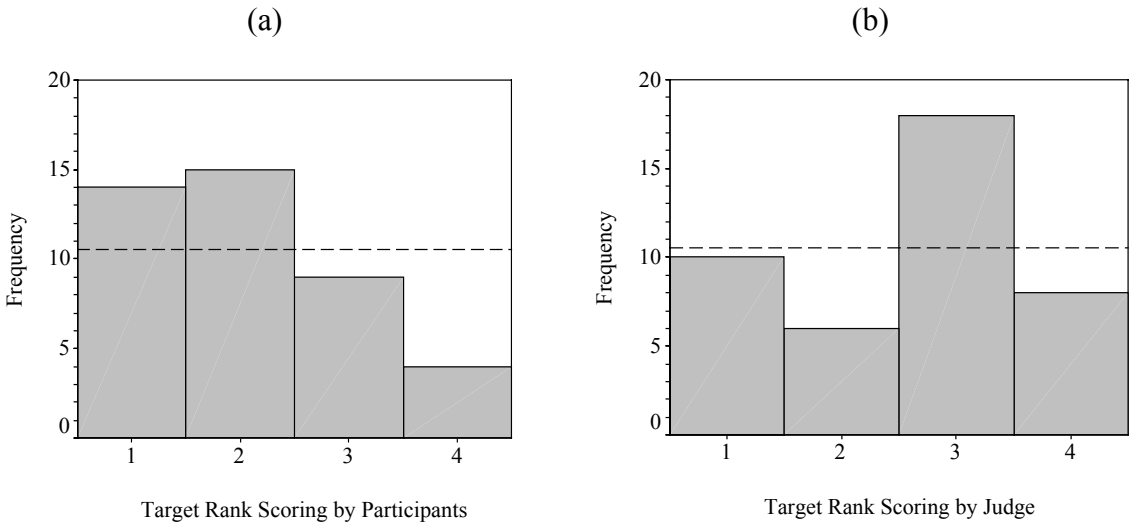

Figure 9.3. Uniformity distributions for the sighted: (a) participants' ranks, (b) judge's ranks.

Mentation reports seemed to contain either sufficient and accurate details of the targets (resulting in rank #2), or completely irrelevant descriptions of the targets (resulting in rank #4). Yet, for the sighted group, the judge reversed this trend, showing an avoidance of ranks #2 and #4, in deference to rank #1, and a pronounced preference for rank #3 (see Figure 9.3b). Thus, the distributions for vision-impaired and sighted groups were heterogeneous, suggesting that either the judging procedure is confounded by variables as yet undetected, or that the mentation reports do not contain all the relevant mentation (i.e., participants may be guided by information other than their verbalization). It may be that mentation reports require a qualitative analysis in advance of the capacity of a single judge. Or it may be that mentation reports are more useful to the participants than to the independent judge.

9.4.4.2 Target ranking by participants and by independent judge: An alternative comparison: There was a positive and highly significant correlation between participants' rank scores and the independent judge's rank scores, $r_s(82) = .31$, $p = .002$, one-tailed. This result suggests that there was some small degree of correspondence between the participants and the judge on the interpretation of the mentation reports, too small, in fact, to expect a large degree of overlap, which might explain why the judge's sums-of-ranks scores were not significant (see Hypothesis 1B). This correlation indicates that the judge, in a sense, concurs to a small degree with the participants' rankings of the mentation

reports, but that there may not be enough information in them to allow highly concordant judging.

9.4.4.3 The totally blind versus the partially blind: A negative correlation was expected, and found, to some extent, between degree of vision (see *9.3.2*) and rank scores by participants, $r_s(82) = -0.17$, $p = .058$, one-tailed. This marginal result, if counted as significant, would suggest that more accurate scoring tended to be achieved as sight 'improved'. From that result, it might be hypothesized that those vision-impaired participants rated as totally blind ($n = 18$), who could not see the pictures at the ranking stage were the most disadvantaged compared to those partially blind participants who had at least some vision and could see the pictures ($n = 24$).

However, an analysis of rank scores for both vision-impaired subgroups showed that the totally blind subgroup got five direct (rank #1) hits (28%), compared to the partially blind subgroup's three direct hits (13%). The direct hit-rate for the blind participants translates as an effect size of $\pi = 0.54$, which is actually greater than the effect size for the remaining 66 participants in the sample on the direct-hit measure ($\pi = 0.51$). Thus, there was evidence that the partially sighted and fully sighted participants had no advantage over the totally blind participants. (Note that, for sighted participants the π value was 0.60, which is not significant, $p = .102$, and for the vision-impaired participants the π value was 0.41, which is not significant, $p = .537$.)

9.5 Discussion

9.5.1 Target Ranking Based on Participants' Data

Using the sum-of-ranks formula, the overall performance of the 84 participants was significant, which supports the conclusion that some kind of anomalous (paranormal) performance took place during the experiment. However, the mean rank scores were higher (worse) for the vision-impaired than for the sighted, but the significant shifts towards ranks #1 and #2 for both groups (as shown in the post hoc analyses—see *9.4.4.1*) imply that anomalous effects were not only produced by participants in the whole sample, and in the sighted group, but in the vision-impaired group also.

Perhaps if the two groups had been truly dichotomous (with the vision-impaired group consisting exclusively of totally blind participants), performance by the vision-

impaired group may have been significant, even surpassing that of the sighted group. This possibility is strengthened by the fact that the 18 *totally* blind participants were not disadvantaged compared to the remaining 66 participants who were *at least* partially sighted—the former group performed better than the latter on the most conservative measure, direct hits.

Note that it was suggested that the mentation reports of all participants be analysed for any qualitative differences between the reports of the sighted and the vision-impaired to see if the nature of the impressions may have differed or not. The nature of the target-mentation correspondence may differ for sighted versus vision-impaired participants (W. G. Braud, personal communication, June 25, 2001). It should also be mentioned that many participants seemed to vacillate over which of the first two pictures should be ranked #1 and which should be ranked #2. All participants were new to this kind of experiment, so perhaps practice makes perfect. With more experience in tasks of this nature, participants may learn to make successful decisions more easily, perhaps by putting greater trust in their first impressions.

9.5.2 Target Ranking Based on the Independent Judge's Data

Performances by (i) the whole sample, (ii) the vision-impaired group, and (iii) the sighted group, as measured by the sum-of-ranks formula, were not significant. As the post hoc analyses show however (see *9.4.4.1*), there were significant deviations in the uniformity of ranks in all three cases, implying that anomalous effects were produced by the various groupings of participants. Note that these effects were determined from mentation reports alone, which seemed to have been a little too ambiguous for the judge, resulting in heterogeneous distributions of target ranks for the vision-impaired and sighted groups, but less so for the whole sample combined. Note that it was not possible to rule out experimenter effect on the part of the judge in the case of any significant effects (see *9.4.4.1*, *9.4.4.2*, and *9.4.4.3*).

As was shown, most of the significant results were found in the participants' data. Reasons have already been given as to why the independent judge may not have done as well.

9.5.3 The Relaxation Treatment

Generally, the evidence was only suggestive (that is, not conclusive) that the relaxation tape helped participants relax, and consequently assisted them in the paranormal

process. The exception was for the vision-impaired group, where the relaxed subgroup performed better than the nonrelaxed subgroup (see *9.4.2*, Hypothesis 3A (ii)).

Some participants did not like the relaxation tape, and this might have disturbed their performance on the task. For example, some participants were distracted by the American accent, or the paradoxical contents of the tape (e.g., instruction to imagine a still pond while gushing water in a stream could be heard in the background!). For the vision-impaired participants, it was highly likely that they were already relaxed, since most were tested at home. Therefore, the relaxation tape may not have had as much of an effect as it could have had.

The results of this experiment say little about the general efficacy of relaxation treatments as being conducive to paranormal effects, since there are many different techniques for inducing relaxed states in participants, relaxation tapes being only one. Alternative forms of relaxation and/or more sensitive measures of the effects of relaxation (besides pulse) should be considered in future experiments of this kind. If the parapsychologist is to gain from using relaxation tapes in the future, the quality and content of the material on the tapes, and/or the length of time that the relaxation treatments are administered must need serious consideration. In this study, the relaxation session lasted 14 minutes. Longer sessions may be preferable to shorter ones to give the participants more time to settle in.

9.5.4 Target Ranking and Belief in ESP

For the whole sample, and for sighted participants, based on participants' data, there was no convincing evidence that belief in ESP was conducive to lower (better) rank scores (Hypotheses 4A (i) and (iii)—see *9.4.2*). However, for the vision-impaired, believers' rank scores tended to be lower than disbelievers' rank scores, but not significantly (Hypothesis 4A (ii)).

For the whole sample, and for the vision-impaired, based on the independent judge's data, rank scores of believers in ESP tended to be lower than rank scores of disbelievers, but again not significantly (Hypotheses 4B (i) and (ii)—see *9.4.3*). However, rank scores of sighted believers tended to be higher than rank scores of disbelievers, but not significantly (Hypothesis 4B (iii)—see *9.4.3*). Thus, in three out of six sub-hypotheses, the directional hypotheses were supported.

9.5.5 Conclusion

In terms of finding support for the theory of psychopraxia, the initial aims of the experiment must be considered. The first aim was to determine if there was a significant difference in regard to paranormal performance between vision-impaired participants and sighted participants. The successful achievement of this aim would support the hypothesis that exo-psychopraxia may act in a compensatory way for a temporary or permanent ostensibly adverse condition of the test participant.

While good evidence was found that the whole sample demonstrated psychopractic effects, the sighted group performed better, although both types of participant, sighted and vision-impaired, seemed to the principal experimenter to show preferences for (i.e., held pro attitudes towards) identifying the picture-target, which manifested as lower (better) ranks. While the pro attitude of the vision-impaired group may have been just as strong, it seemed that, generally, compensatory factors did not manifest for them. Thus the vision-impaired group failed to demonstrate paranormal compensation of their impairment.

It should be pointed out that compensation comes in other, more normal, forms, as many vision-impaired participants attested. The typical vision-impaired individual may compensate for their impairment by improving their sense of smell, or touch, or hearing, or other senses, singly or in combination. It must not be overlooked, however, that although compensation may be effected by way of the normal sensory modalities, there is still reason to suspect that compensation may also come from the paranormal domain, as was the post hoc suggestion that neither the partially sighted, nor the fully sighted participants, had advantages over the totally blind participants. The possible role of limited sample size, which may have produced insufficient power for the tests, should also be considered in relation to this study.

A further two aims were proposed to test the moderating variables of (i) relaxation, and (ii) belief in ESP. These variables were hypothesized as being necessary (or at least conducive) conditions that help bring about exo-psychopraxia. Regarding relaxation, there was statistical evidence that paranormal performance was enhanced by relaxation for the vision-impaired subgroup (based on participants' data), and also suggestive evidence that relaxation helped (results from 4 out of 6 hypotheses were in the right direction: three based on participants' rank scores, and three based on the judge's rank scores). These results suggest at least some tendency towards enhanced paranormal performance by relaxed participants compared to non-relaxed participants, and they indirectly suggest the advantage of having a relaxation treatment before the paranormal task. The success of the ganzfeld paradigm only supports the claims for relaxation made here.

Finally, belief in ESP seemed not to have an enhancing effect at all on exo-psychopraxia. The claims in the literature that have been made for belief in ESP were not generally supported in this experiment. The results of this study, however, may be artifacts of the single-item measure used to gauge belief in ESP. Many participants found it difficult to commit to a forced-choice (yes/no) answer, and ultimately decided, one way or another, after some degree of hesitation. A greater number of intermediate choices may have provided a more sensitive measure.

Overall, the above three psychopractic aims were not achieved due to ambiguous results, statistically speaking, but there was, generally speaking, suggestive evidence that the aims were achieved in terms of trends in the hypothesized directions. In the next chapter, the meta-analyses of the paranormal domains discussed in Chapters 2 and 3 will be reconsidered in terms of the theory of psychopraxia.

CHAPTER 10

THE META-ANALYSES AND THE ESP-PK DICHOTOMY[88]

Meta-analysis . . . is going to help parapsychologists write recipes for successful experiments.

Richard S. Broughton (1991, p. 298)

The grounds for believing in the validity . . . of the evidence arrived at by meta-analytic procedures are slight indeed.

Peter S. Delin (P. S. Delin, personal communication, June, 2000)

10.1 The Meta-Analyses

Chapters 2 and 3 were reviews of the meta-analytic literature. Nine paranormal domains[89] were featured in those chapters: (i) biological systems, i.e., DMILS, (ii) forced-choice, (iii) free-response, (iv) dice-throwing, (v) RNG, (vi) clairvoyance, (vii) precognition, (viii) ganzfeld, and (ix) autoganzfeld (see Table 3.2). The precognition meta-analysis by Steinkamp et al. (1998) was performed on forced-choice studies from 1935 to 1997 that were identifiable as being of the clairvoyance or precognition type. Thus, it is more up to date than Honorton and Ferrari's (1989) meta-analysis (for which the period of analysis was 1935-1987). However, Steinkamp et al.'s meta-analysis refers to two specific domains that may be contrasted with the more general precognition domain of Honorton and Ferrari. There may be a very slight degree of overlap in relation to the precognition-type forced-choice studies, but all three effect size norms are virtually the same, as might be expected (see Table 3.2). Note also that Steinkamp et al. found no significant difference in performance between clairvoyance and precognition studies.

In this chapter, the results of those nine meta-analyses will be considered in terms of magnitude of effect, and it will be argued that the lack of clear differences in effect

[88] This chapter was published as part of a refereed article (see Appendix AD).

[89] In this chapter, only meta-analyses that fall into the traditional ESP–PK categories were used. Correlational meta-analyses on mediating or moderating variables (such as personality and belief) were not used because interest was only placed on "pure" measures of psi performance (ESP and PK), not strength of correlations.

sizes between 'ESP' and 'PK' categorical domains does not lend support to the ESP-PK dichotomy, thus justifying Thalbourne's (2000a, pp. 1-25) statements that unification of the two categories is a terminological imperative for the field of parapsychology.

Some researchers (e.g., Honorton et al., 1990, p. 127; Milton, 1998, p. 279; Utts, 1995, pp. 310-311) have argued that effect-size comparisons between similar domains (e.g., ganzfeld and autoganzfeld, or remote viewing and ganzfeld) are a valid means of determining the efficacy and consistency of experimental methodologies. Under the assumption that experimental methodologies for the domains are similarly efficacious and consistent, it can be argued that, should effect sizes across domains be of similar magnitudes (based on nonsignificant differences between them), one and the same paranormal process seems likely to have been elicited across domains. This single process has already been referred to as psi (Rhine, 1948/1954; Thouless & Wiesner, 1947), though that term was found to be ambiguous (see Chapter 4), and more recently as psychopraxia (Thalbourne, 2000a).

Critics of this view may fail to see how "similarity of effect-size should lead one to suspect similarity of mechanism" (P. S. Delin, personal communication, November 26, 2001). However, it is stressed here that of course the 'effect-size-similarity' criterion does not stand alone as an argument, but is supportive only of an over-arching argument for a unified effect that simply follows the general findings of many researchers, past and present, that there is a unified effect because evidence to the contrary is lacking (see **4.2**). Furthermore, the counter-argument—two similar effect-sizes for two different domains might mean that there are two different mechanisms—is not parsimonious. Thus, given that the rigidity of the ESP-PK dichotomy has already been challenged (see again, **4.2**), there may be just cause in arguing further that qualitative differences between ESP and PK cannot be determined on the basis of magnitude of effect. In fact, Schmeidler (1994) has already stated that the meta-analyses have not yet given any clear evidence that "qualitative differences" (p. 229) exist between ESP and PK. In order to test these statements, consideration is now given to the effect-size 'norms' for each of the nine domains as an alternative attempt at finding evidence that might challenge the ESP-PK dichotomy.

10.1.1 The Effect-Size 'Norms'—Domain Comparisons

By comparing the magnitudes of the effect-size norms shown in Figure 10.1 below, it can be seen that there is no inherent means of demarcating ESP from PK. The

figure shows a dispersion of the effect size norms (calculated using the $z/N^{1/2}$ formula) for all paranormal domains.[90]

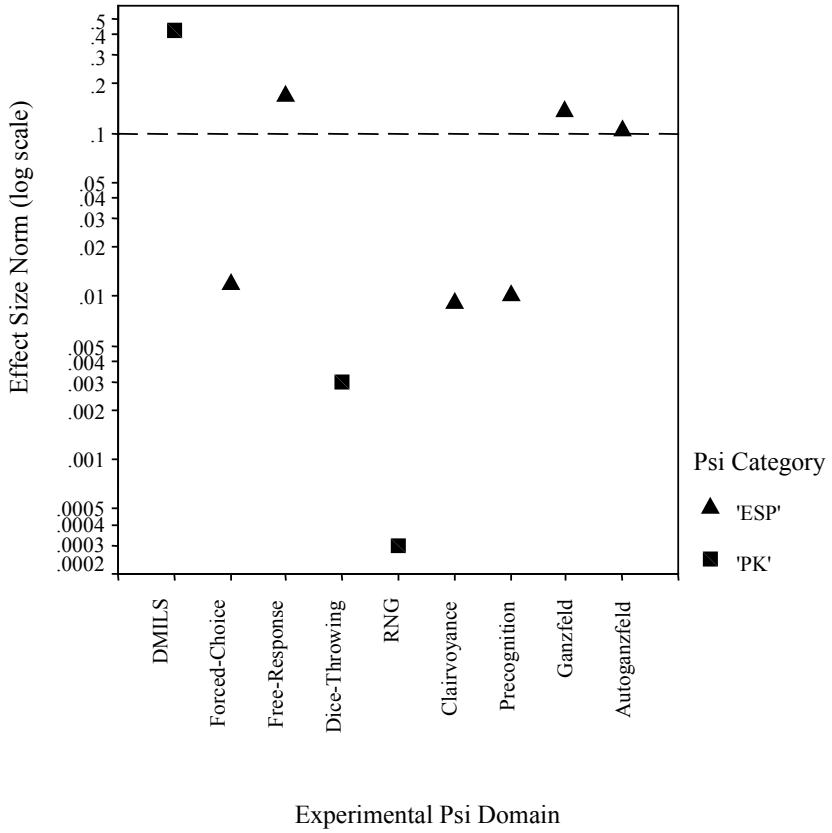

Figure 10.1. Scatterplot graph showing the effect-size norms of the nine major psi domains and the mean effect-size norm reference line (dotted line is the mean $ES_{norm} = 0.10$).

A broad dispersion of the norms can be seen, ranging from 0.0003 to 0.42. The norms for DMILS, free-response, ganzfeld, and autoganzfeld domains, which are relatively large, are at one extreme (at the top of Figure 10.1, above the mean effect-size norm of 0.10), whereas the relatively small norm for the RNG domain is at the other extreme (at the bottom of the figure and well below the mean). The RNG norm (the smallest of the nine

[90] Figure 1 is a graphic representation using a logarithmic ordinate axis for ease of inspection only of the meta-analytic effect-size norms. No law or functional relation between the x and y variables of the form $y = f(x)$ is implied.

norms) is 328 times smaller than the mean effect-size norm, whereas the DMILS norm (the largest of the nine norms) is only four times larger than the mean effect-size norm.

In the light of the above findings, it would appear that the argument for the ESP-PK dichotomy is indefensible. In fact, the three 'PK' domains are widely dispersed amongst the scatter points, two of which demarcate the highest and lowest extremes of the range of norms (DMILS and RNG, respectively). These norm differences may be attributable to methodological differences in the domains.

Recall that in Chapter 2 (see **2.2**, p. 32) it was stated that "should there be a significant effect size difference [between two domains], it would only draw out the distinction that either two types of psi were in operation, or the [experimental] paradigms are more conducive to psi in one domain, and less conducive in the other." Delin (personal communication, November 26, 2001) argued that two different PK effect-sizes could be evidence of two types of psi. This argument is discussed next.

10.1.2 Extreme Deviations from the Mean: Evidence of a Dichotomy?

Psychopraxia theory holds that the ESP-PK dichotomy is untenable, and this position is defended by the results of the meta-analyses, but would extreme effect-size norms challenge this conclusion? Not if it is possible that unidentified causal factors unique to the relevant domain merely give the impression that the dichotomy exists. If it is possible, then such a view may lack parsimony. The extremely low effect-size norm yielded in the RNG meta-analysis can be used to illustrate this point. It can be argued that there is nothing to be gained in thinking about the weakness of the norm for the RNG domain as indicating a distinct PK process when it may be more constructive to start with the claim that work needs to be done in identifying possible psi-inhibitory factors in RNG experimental procedures that might be responsible for the low effect size. These same inhibitory effects may not exist in other so-called 'PK' experiments.

Decision augmentation theory (DAT; see *4.2.5*), for example, would explain the low RNG effect-size norm. As there are usually only a few hundred information bits available in an RNG trial compared to the many thousands in, say, any given target in a ganzfeld trial, the effect size in a typical RNG experiment is low because the RNG trial, although it needs much less effort, provides much less information compared to a trial in most other paranormal experiments.

Again, according to DAT, the precognitive task of scanning the environment for the right moment to act is also comparatively difficult given that only one decision point is permissible, and the limits of the standard normal distribution put constraints on the *z*

score for each button press. In other paranormal experiments there are vastly more possibilities of making a correct decision. (Note that this dichotomy gives rise to the hypothesis that the more information there is to be processed, the smaller the psi effect becomes. This issue is raised below—see **10.2**.)

Thus, more generally, it is not necessary to argue (using traditional terminology) that PK can be a uniquely deviant and highly variable form of psi compared to the more normal and constant ESP. The extreme differences in the norms of the PK-type domains may be artifacts of methodological differences in those domains. Two possible causes of these artifacts are discussed next.

10.2 Perceived Apparatus Complexity and Task Difficulty

Generally, the DAT arguments are reasonable, and in very real terms either argument on its own, or both in concert, would explain the low RNG effect-size norm. However, these factors not only refer directly to a special class of experimental difficulty specifically related to the nature of the RNG experiment (particularly the apparatus), but they also lead to the inevitable psychological problem that all RNG experiments, regardless of variations around an RNG theme,[91] are likely to be perceived (consciously or otherwise) by the test participant as difficult and complex. That is, RNG experiments are comprised of *inhibiting* features, which are therefore challenging to the participant. The inability, on the part of the participant, to contrast these tasks with other tasks is of no consequence to the analyses in Chapter 11 since a judgement of (say) complexity is not necessarily qualified by *degree* (i.e., 'easier than' or 'harder than'), but by an intuitive impression based on the participant's *feelings* at the time of the task.

It is maintained, therefore, that the issue of (a) perceived (and, in the RNG case, actual) apparatus complexity, and thus, as a possible consequence, (b) task difficulty must also be considered when attempting to account for low effect sizes in RNG experiments. This consideration is in keeping with the broader understanding in parapsychology that psychological and attitudinal variables have been shown to contribute to the variability of results.

The norm for one other PK domain must also be considered in terms of the conditions necessary for psychopraxia to take effect. One can see in Figure 10.1 that the

norm for the dice-throwing domain lies below the norms of all six ESP-type domains, and it is 33 times smaller than the mean effect-size norm. This difference, however, is very much smaller in magnitude than the difference for the RNG norm. In relative terms, then, a certain necessary condition present in the dice-throwing task may not be present in the RNG task. Specifically, the possibility of influencing the fall of a die may not be perceived to be as difficult as influencing an RNG, though this conjecture remains to be tested.

The reason for this perception differential may have to do with the long history of testing with dice, coupled with the fact that the simple die is so familiar to everyone. Regarding the high effect-size norm for the DMILS domain, gerbils and fish are common household pets and, given their docile natures, it would be difficult to entertain the idea that they could have an adverse affect on the psi performance of the average participant on the basis that they represent a threat to the participant's confidence. The RNG, however, is a relative newcomer in the field of parapsychology, and it is suggested that it is a long way from being perceived as a simple device. The "techno-fear" hypothesis targets difficult, complex, human-made apparatus, not natural (i.e., biological) systems. Thus, if an effect-size norm is high for one paranormal domain but low for another, then one parsimonious reason for this difference, given the same underlying paranormal cause, is that the former norm is the result of conditions that were generally good in some way for a sufficient number of participants, whereas the latter norm is the result of conditions that were generally adverse in some way for a sufficient number of participants.

Hypothetically, should there be such a case that the distribution illustrated in Figure 10.1 varied in such a way that a bipolar dispersion was effected, where (say) the 'ESP' effect size norms were 'high' and the 'PK' norms were 'low', the argument against the ESP-PK dichotomy would be refutable. Such a distribution, and the argument that this distribution would support the dichotomy, would of course, be theory-based, depending entirely on the traditional dualistic categories of ESP and PK, whereas the argument against the dichotomy is evidence-based. Nevertheless, the possibility of refutation of the argument against the dichotomy is important in terms if it being falsifiable.

[91] Performance comparisons between mechanical setups within the RNG domain may produce significant differences in scoring between them (e.g., Schmidt, 1973), or nonsignificant differences (e.g., Schmidt & Pantas, 1972) but, relevant to other domains, effect sizes generally tend to be much lower.

10.3 Conclusion

In Chapter 4, it was conjectured that a demarcation between the two traditional categories, ESP and PK, was not necessary or even sustainable. The philosophical, theoretical, and experimental difficulties that have arisen in arguing for an ESP-PK dichotomy were considered. In the present chapter, it was found that strength of effect size (based on the effect-size norms for each paranormal domain, given in each of the respective meta-analyses) seems not to be differentiated by experimental categorization.

Regarding the low effect-size norm of the RNG domain, it was proposed that the complexity issue be reconsidered. It was argued that within the RNG paradigm specifically, circumstances are characteristically different from other domains (as argued in DAT). In addition, the overall system may be perceived to be complex (a psychological state) because actual (technological, not biological) complexity (a physical state) provides the grounds for that perception and, although there may or may not be significant variations in performance between one RNG task and another, in general, RNG paranormal performance will tend to be inferior compared to other domains.

It was then proposed that two conditions conducive to a psychopractic effect in the RNG situation would be (i) lack of perceived complexity of the apparatus (or better, perceived simplicity of the apparatus), and as a possible consequence, (ii) lack of perceived difficulty of the task (or better, perceived ease of the task). Consequently, if there is any validity in the psychopractic hypothesis, insofar as certain conditions may inhibit psychopractic effects, then future parapsychological participants have a necessary educational process of familiarization with RNGs ahead of them. Delin (personal communication, November 27, 2001) pointed out that as people become more familiar with computer-run RNG experiments, techno-fear may subside and effect sizes may increase in the RNG domain, and these outcomes would support this hypothesis.

In the next chapter, the two hypothesized necessary conditions, (i) perceived simplicity of the apparatus, and (ii) perceived ease of the task, are investigated for their psychopractic effects on paranormal performance. While Chapter 11 is not an investigation into the relationships between these hypothesized necessary conditions and performances on RNG tasks compared to other psi tasks, it is still possible to gain evidence indicating that performances within and between psi tasks may differ, due principally to the effects of participants' perceptions about those tasks. Six other conditions are also investigated for their psychopractic influences on task performance.

CHAPTER 11
FURTHER INVESTIGATIONS INTO CONDITIONS CONSIDERED NECESSARY FOR PSYCHOPRAXIA, AND A SUMMARY OF THE FOUR EXPERIMENTS

A major part of the activity of mind is just the apprehension of meaning . . . which can participate in the overall process of the interweaving of mental and physical sides.
David J. Bohm (1986, p. 127, p. 133)

The discussions and hypotheses about psi-conducive psychological conditions basically remain in the domain of lab lore rather than useful, predictive science.
James E. Kennedy (2001, p. 226)

11.1 States of Mind

In Chapter 10, it was argued that perceived apparatus complexity and task difficulty might have adverse effects on paranormal performance. According to the theory of psychopraxia, the adverse effects would result from the lack of conditions conducive to (and possibly necessary for) exo-psychopraxia. Thus, mental conditions inversely associated with apparatus complexity and task difficulty were hypothesized as necessary conditions; to wit, perceived simplicity and perceived ease of task (see **10.2**). In this chapter, these two conditions, actually personal perceptions of, or better, feelings about one's self (i.e., 'states of mind') are investigated as possible conditions necessary for the facilitation of paranormal effects.

Six other states of mind are also investigated as possible conditions necessary for paranormal effects to occur. These states of mind, previously listed in Chapter 4 (see *4.4.4*), are again listed here (slightly modified): (i) 'freshness', (ii) 'sobriety', (iii) 'attentiveness', (iv) 'confidence', (v) 'fitness', and (vi) 'energy level'. The major aim of this chapter is to show that there are relationships between states of mind and paranormal outcomes by conducting performance comparisons within and between the four experiments presented in Chapters 5-9.

The chapter concludes with a summary of success rates, and an evaluation, of all the hypothesized necessary conditions tested in the present thesis.

11.1.1 Perceived Complexity of the Apparatus, and Difficulty of the Psi Task

There has been occasional inquiry into the apparatus used in psi tasks, and the degree of difficulty of those tasks. Some inquiries have focused on the issue of a*ctual* complexity of the apparatus (e.g., Stanford, 1977b, 1978), while others have considered the problem of task difficulty (e.g., Schmidt, 1973; Schmidt & Pantas, 1972—see *10.1.2*, and also p. 227, n91 for comments on task difficulty in RNG set-ups). Stanford (1977b), for example, observed that, for the PK domains, the evidence was not strong that actual complexity might have a deleterious effect on paranormal performance.

Stanford (1990) later claimed that evidence for success on "very complex tasks" was "reasonably good . . . and continues to accumulate" (p. 59). However, he did not consider perceived complexity in his argument—he merely assumed that complexity was an inherent characteristic or feature exclusive to the apparatus that had nothing to do with the way the test participant might construct their version of reality, i.e., might construct their perception of complexity independent of any empirical evaluation.

Kennedy (1995, p. 47), too, never acknowledged that *perceived* complexity (a *psychological* condition) of the experimental setup/apparatus, nor perceived task difficulty, could have effects on paranormal performance (see also Kennedy, 1978; Stanford, 1977b). Kennedy concurred with the general finding that the statistical evidence supports the claim that actual complexity (only a *physical* condition) is not a psi-inhibitory variable, but again, this evidence does not undermine the hypothesis that participants' perceptions of both the apparatus (i.e., the 'setup' in general), and the difficulty or relative ease of the task at any level of consciousness, might influence the psi effect.

For example, Vassy (1986) found that informational complexity resulted in decreased success in precognition tasks. He used a pseudo-RNG and ran sequences at randomly varying lengths, thus being able to characterize the complexity of each of five series (e.g., three digits would be perceived as less complex than five digits). Vassy applied a goal-oriented model to analyze the results (i.e., hit-rate did not depend on sequence length), and he also analyzed the results using a constant-constraint model where the amount of ostensible psi information used did not depend on the length of the sequence. Significant psi effects manifested in four of the five series. Vassy concluded that "a completely complexity-independent model [of the psi process] is not correct and

that differentiation between process and informational complexity of psi tasks in phenomenological investigations is necessary" (p. 235).

More recently, Steinkamp (2001) also used a pseudo-RNG in two modes: clairvoyance and precognition. (It would be reasonable to assume that testing these two types of psi might require experimental set-ups that differed in complexity.) In the clairvoyance condition, participants were required to select their target pictures based on a series of four numbers pre-selected and presented by computer. The target picture (numbered 1, or 2, or 3, or 4), pre-selected by the computer and unknown to the participant, was regarded as correctly identified by the participant if that target picture's number corresponded with one of four randomly-generated numbers that the participant thought was the correct number. Theoretically, participants could use real-time psi (i.e., clairvoyance) to identify the targets in advance of the numbers presented by the computer (this method would not exclude ostensible precognition).

In the second condition—the so-called "true precognition condition" (Steinkamp, 2001, p. 17), which was similar to the above design—participants were required to identify target pictures that corresponded with "stock market and temperature figures on a prespecified future date" (p. 17). Theoretically, the only way a participant could correctly identify the target was to foresee these figures so as to select the correct target on the basis of information not yet available. Results of these two experiments were not significant in the clairvoyance or precognition conditions, although in a similar previous experiment (essentially a precognition study that could not eliminate the possibility of clairvoyance; see Steinkamp, 2000), significant results were reported for the clairvoyance condition.

In a third experiment, using the World Wide Web to test participants 'on-line' in clairvoyance and precognition conditions, the results were also nonsignificant in both conditions. No conclusion could be reached as to whether clairvoyance is a different process from that of precognition. More importantly, Steinkamp (2001) could not show whether the more complex precognition task could be distinguished from the relatively easier clairvoyance task.

Thus, the "complexity issue is itself complex," as Stanford (1978, p. 205) claimed. Nevertheless, it is hypothesized in the present chapter that there is a relationship between the *perception* about the experimental apparatus and psi performance. As regards the related issue of the difficulty of the psi task, it is also hypothesized that there is a relationship between the perception of the psi task and performance in that task.

11.1.2 Other States of Mind

Six other states of mind are investigated in the present study as identified by Thalbourne (2000a, p. 96), with minor changes: (i) 'fresh' (rather than 'weary'); (ii) 'sober' (rather than 'intoxicated'); (iii) 'attentive' (rather than 'distracted'); (iv) 'confident' (rather than 'uncertain'); (v) 'fit' (rather than 'out-of-shape'); and (vi) 'energetic' (rather than 'tired'). Note that freshness may be a state while energy level may be a trait (see the post hoc analyses—*11.3.3*). Thalbourne argued that just as these states of mind can be conducive to normal functioning, it is reasonable to hypothesize that they may be conducive to paranormal functioning. These conditions are now considered.

11.1.2.1 'Freshness': No parapsychological studies were found that focused on 'freshness' or 'weariness' as independent variables. Clearly, however, these terms might come under broader categories such as healthiness or well-being, although the word 'freshness' may not apply in the same sense as healthiness since it is possible that levels of freshness could vary within the healthy group. Regardless of that fact, no studies were found that compared healthy groups of participants with alternative groups on levels of performance on a psi task.

Kennedy and Kanthamani (1995) found that participants who "reported having had at least one paranormal or transcendent experience reported that these experiences increased their interest and belief in spiritual matters and increased their sense of well being" (p. 249). Of course, the causal direction of this relationship does not suggest that well-being might bring about "paranormal or transcendent experience."

Goeritz and Schumacher (2000) found a weak relationship between the same variables (i.e., paranormal belief and well-being) for the females only in their sample. These findings only tentatively suggest links between freshness and psi performance, if it can be taken that freshness and well-being are the same or similar (cf. *11.1.2.5*). As such, the investigation of freshness as a possible necessary condition for bringing about a psi effect is undertaken in the present study for the first time.

11.1.2.2 'Sobriety': Studies were found that featured differential effects of altered states of consciousness (ASC) in the form of intoxication, though these were both studies on marijuana intoxication, not alcoholic intoxication (Tart, 1971, 1993). The vernacular use of the term 'intoxication' implies alcoholic intoxication, and it is likely that the subscale itself ('sober'/'intoxicated') was responded to with that understanding. Suffice it to say, not one

of over 200 participants to whom the scale was administered asked what type of intoxication was meant.

Tart's (1971, 1993) investigations of ASC's and their possible effects on psi performance leads to the conclusion that other forms of intoxication might be conducive to paranormal performance, but no conclusive investigations have been conducted to confirm this broader application of Tart's hypothesis. As it happens, it is not clear whether the effects of alcoholic intoxication would be expected to have psi-conducive or psi-inhibitory effects, since the possibility exists that intoxication may induce a relaxed state, the latter of which is known to facilitate psi (see *9.1.3*), while on the other hand, excessive intoxication may result in the inability to concentrate or focus on the psi task, and therefore this ASC could have adverse effects on psi. (Note that the tests of sobriety in the present study were to be two-tailed—see *11.2.5*, Hypothesis 6.)

11.1.2.3 'Attentiveness': One study was found which specifically focused on 'attentiveness'. Chiarabba (1969), a noted psychic, gained foreknowledge about his wife that came true. He argued that consciousness in such a precognitive state is totally concentrated on the "attentive function" (p. 71):

> The sensorial medium is absent and is substituted by the psyche. The perception cannot be realized without the concurrence of attention; in contrast, [one] can eat without tasting if the attention is absorbed in a dominant thought. The attention of the [subject] must be absorbed toward the object, and this determines the isolation of the psyche (p. 71).

Effectively, Chiarabba took the commonsense view that it is necessary to maintain focused attention on the distant object or event in order for it to be 'perceived' paranormally in consciousness. This same principle underlies the successful ganzfeld domain where the experimenter expects participants to be in a relaxed but focused state, i.e., in an attentive ASC (see Chapter 2). It would be possible, then, for foreknowledge, and by extension psi effects in general, to occur because the psyche would not be flooded by extraneous thoughts ('noise') interfering with the psi 'signal'.

Kennedy (2001), in his general comments about psychological conditions conducive to psi, claims that "attention to the [psi] task without distraction or boredom" is "important for obtaining significant results" (p. 224). His statement is merely a reiteration of a long-held tacit understanding of, and general consensus about, attentiveness as a psi-conducive variable, which may be a concomitant of concentration, focused attention, and

similar states of consciousness. It is hypothesized in the present study that attentiveness is a necessary condition.

11.1.2.4 'Confidence': Although research on confidence in relation to psi performance has been sporadic since the 1940s (for examples, see Freeman, 1961; Price & Rhine, 1944), a consistent level of research took place in the 1970s. Specifically, 'confidence calling', or using confidence ratings to predict paranormal outcomes, featured quite strongly in the literature. Gelade and Harvie (1975) found that "confident choices were associated with significantly above-chance scores [on an ESP task], while uncertain choices were associated with below-chance scores" (p. 209). In Honorton's (1970, 1971) two studies, psi scoring in a card-guessing task significantly correlated with confidence calls (i.e., calls that participants felt relatively confident were correct), if the feedback on calls were true, but not if the feedback was false.

McCallam and Honorton's (1973) replication study also produced significant results, but Jackson, Franzoi, and Schmeidler (1977) failed to replicate the results of the three studies that featured Honorton as main author. Two confidence-calling experiments—Honorton, Tierney, and Torres (1974), and Kanthamani and Kelly (1974)—were variations on the card-guessing theme, and both studies produced significant results. In particular, Kanthamani and Kelly (1974) conducted a series of three card-calling tests using a 'special subject' (Bill Delmore). All three series were independently significant. Confidence calling was associated with psi success.

Karnes and Susman (1979) tested receivers in a remote viewing task by asking them to rate their confidence in their target selections. There was no statistical evidence that confidence ratings attached to correct and incorrect selections could be used to demarcate paranormal ability from paranormal inability.

Schouten (1983) found that belief in ESP positively correlated with confidence in personal success at ESP tasks, but he did not test actual psi ability. Jacobs (1987) compared participants self-rated as 'high' on confidence against those self-rated as 'low'. There was no significant difference between scores on a paranormal waking task, where participants were required to wake at a designated time.

Studies on the effects of confidence on psi performance suggest there is some evidence that confidence is psi-conducive. The form confidence may take is variable as well, given that the studies reviewed above tested participants' confidence of a psi outcome only, whereas the state of mind scale 'confidence'/'uncertain' used in the present study seeks responses from participants that refer to a more general state of mind.

Nevertheless, the above studies are relevant to the 'confident'/'uncertain' dimension because participants are required to respond to that scale in accordance with their states of mind "at the time [they] participate in the paranormal task" (see Appendix A) so that level of confidence is expected to affect performance. Attempts are made in this study to reach an understanding about the role of confidence in paranormal tasks, and it is hypothesized that confidence is a necessary condition.

11.1.2.5 'Fitness': Only occasional research into fitness and psi has been conducted. One very early study on this relationship was that of Martin and Stribic (1938). They tested a special participant who made 25,000 calls of ESP cards, and found that his significant performance over time suggestively correlated with his physical fitness. As was the case with 'freshness', searches for studies on general health and well-being were also conducted. The only similar study was that of Kennedy and Kanthamani (1995) reported above (see *11.1.2.1*; see also Kennedy, 1994; and Kennedy, Kanthamani & Palmer, 1994, for similar demographic studies). It appears that much work needs to be done on paranormal performance as it relates to physical and mental fitness and other health-related issues. In the present study, it is hypothesized that fitness is a necessary condition.

11.1.2.6 'Energy Level': Qian (1982) noticed similarities between the oriental method of qigong meditation ("an ancient system for self-development that involves movement and breath exercises," p. 4) and ESP abilities. Qian states that "Qigong consists of the conscious control of body energy, and its practitioners are able to enter an altered state of consciousness at will" (p. 4). This postulated form of 'body energy' suggests the likelihood that energetic states of the mind/brain complex and consciousness are inter-related, although it does not imply that psi is a form of energy or force that flows, or is released from, the mind or brain.

Some research into the concept of body energy or life force has been conducted (for examples, see Loevland, 1981; Rhine, 1948/1954; and Thalbourne & Fox, 1999), but research into the more commonplace idea that one can be in a state of mind that can be described as 'energetic' is particularly rare. Although based on the laws of thermodynamics, Loevland's research does, however, refer to energy of the mind that is emotionally based, and describes how "energy and matter are interchangeable between the mind and its surroundings, including the body" (p. 20). He proposes an 'energetics' model of emotions that incorporates "mental structures" (i.e., data perceptions) and uses this model to explain paranormal phenomena.

Beloff (1970) raised the issue of energetics, but undermined it by regarding other psi issues as more important. Beloff dismissed the affinities that many psi researchers have for the laws of physics and chemistry (as being possible explanations for psi), and regarded this indulgence as missing the point when the more fundamental "informational aspects of psi" (i.e., being able to isolate the psi target from all other possible psi targets) was more "baffling" (p. 129). Beloff's position is grounded in the idea that 'hard' science may not be able to explain all things, and that the mind/brain complex may work at levels not previously given much thought. Indeed, there appears to be plenty of scope for further consideration of the emotional state of *feeling* energetic, rather than focusing on the concept of energetics in terms of physical laws. It is hypothesized that feeling 'energetic' is a necessary condition.

Using the states of mind data collected from the four experiments featured in previous chapters (Chapters 5-9), which includes measures across those four experiments on the states of mind scales for 'between-subjects' and 'within-subjects' comparisons, this study sought to show that states of mind can affect performance on paranormal tasks.

11.2 Method

11.2.1 Participants

A grand total of 265 participants drawn from the four experiments featured in previous chapters in the thesis completed Thalbourne and Storm's states of mind scales (SOMS). No participant was used in more than one experiment. Table 11.1 gives the numbers of participants in each experiment who completed these scales.

Table 11.1
The Four Experimental Groups

Experiment	No. of Participants
I Ching	30
Vision-Impaired/Sighted	13
Skeptics (includes Believers)	124
Gambling	98
Total	265

Chronologically, testing of all participants in the *I Ching* experiment was completed first, followed by the Vision-Impaired experiment, then the Skeptics experiment, and finally the Gambling experiment. Not all participants in the *I Ching* experiment were used in this analysis because the eight states of mind were not hypothesized as necessary conditions for exo-psychopraxia until after the experiment began. In the Vision-Impaired experiment, the SOMS was administered to sighted participants only because it was not possible to administer the visual analogue scales to the vision-impaired participants.

11.2.2 Measures

Only one set of measures was used (the SOMS), which was specifically devised for this comparative study (see Appendix A). These scales are based on Thalbourne's (2000a, p. 96) consideration of conditions that were argued as being necessary for success at normal tasks, and by implication (in accordance with psychopraxia theory), paranormal tasks.

In Chapter 4 (see *4.4.4*), six states of mind were identified as being conducive to normal and paranormal functioning. These factors, this time expressed as dimensions, are listed here (slightly modified): (i) 'fresh'/'weary', (ii) 'sober'/'intoxicated', (iii) 'attentive'/'distracted', (iv) 'confident'/'uncertain', (v) 'fit'/'out-of-shape', and (vi) 'energetic'/'tired'. These six factors were represented as bi-polar visual analogue scales.

There are another two visual analogue scales that represent (i) participants' perceptions of the mechanical set-up of the task (i.e., perceived apparatus simplicity), rated on the dimension 'simple'/'complex', and (ii) participants' perceptions of the nature of the task, rated on the dimension 'easy'/'difficult'.

11.2.3 Procedure

At the time the four experiments were conducted, participants in each experiment were instructed to make a cross on each of the eight visual analogue scales of the SOMS according to how they 'situated' their minds at the time of (i.e., just prior to) the paranormal task. Each scale is a visual analogue representing bi-polar states of mind. For the *I Ching* experiment, the Vision-Impaired experiment, and the Gambling experiment, the scales were four 'imperial' inches long, graduated later by the experimenter (L.S.) in tenths of an inch. Thus, scores out of 40 were assigned for each measure, the theoretical range being 0 to 40. These measures constitute the independent variables.

The Skeptics experiment was presented via computer, requiring participants to rate these same scales on an eleven-point Likert scale (the theoretical range of scores was between 0 to 11). Ratings were converted to scores out of 40 using SPSS Transform-Compute analysis and the formula: $S_{40} = 40(R_{11})/11$, where S_{40} is the score out of 40 and R_{11} is the Likert rating out of 11 (S_{40} scores were rounded up or down to the nearest whole number).

11.2.4 Psychological Hypotheses

The following psychological hypotheses were proposed:

1. There will be differences between the four experiments on perceived simplicity of the experimental apparatus.

2. There will be differences between the four experiments on perceived ease of the paranormal task.

The one-way ANOVA test was used to test each of these hypotheses.

11.2.5 Parapsychological Hypotheses

The following parapsychological hypotheses were proposed:

3. There is a positive relationship between perceived 'simplicity of the apparatus' and paranormal success.

4. There is a positive relationship between 'perceived ease of task' and paranormal success.

5. There is a positive relationship between 'freshness' and paranormal success.

6. There is a relationship between 'sobriety' and paranormal success.

7. There is a positive relationship between 'attentiveness' and paranormal success.

8. There is a positive relationship between 'confidence' and paranormal success.

9. There is a positive relationship between 'fitness' and paranormal success.

10. There is a positive relationship between 'energy level' and paranormal success.

11. Across the four experiments, there is a positive relationship between mean scores on simplicity of apparatus and mean scores on the paranormal tasks (across four experiments) expressed as proportions correct (*P*).

12. Across the four experiments, there is a positive relationship between mean scores on ease of task and mean scores on the paranormal tasks (across four experiments) expressed as proportions correct (*P*).

Pearson *r* tests were used to test Hypotheses 3 to 12. Hypotheses 3 and 10 were tested for each of the four experiments. Measures of psi hitting are direct hits for the *I Ching* and Vision-Impaired experiments, and trial-based hit-rates for the Skeptics and Gambling experiments. Regarding Hypotheses 11 and 12, the only way the four experiments could be validly compared on the independent measures was by having a dependent variable (i.e., a psi measure) that was commensurable for the four experiments. Thus, the measure used was the proportion correct (*P*) found by dividing number of hits by total number of trials.

11.3 Results

11.3.1 Preliminary Analyses

Since the SOMS are a new set of scales, a reliability analysis was conducted on the six scales (with and without the 'simplicity of apparatus' and the 'ease of task' scales because these two scales are not exclusively assessments of inner states of mind, but also include judgements about external conditions—these two different measures are referred to as SOMS-6 and SOMS-8, respectively). The SOMS-6 proved to be quite reliable with an alpha coefficient of 0.81, while the SOMS-8 had a lower alpha coefficient of 0.73. When the simplicity of apparatus scale was removed from SOMS-8 (thus forming SOMS-7) the alpha rose to 0.80, which is similar to the alpha for the SOMS-6. The simplicity of apparatus scale appeared to be a major cause in the reduction of the reliability of the SOMS overall.

The eight scales were analysed again, this time for homogeneity, by inspection of outliers in the respective distributions of scores.[92] Only 'freshness' and 'energy level' had skews that were not significant due to outliers. The remaining six scales had heterogeneous

[92] See Chapter 5, p. 105, n53, for the formula for this calculation.

distributions. Accordingly, the appropriate outlier cases were removed from the data set so that the distributions of scores for the respective scales would be homogeneous.

Table 11.2 gives the descriptive data for the two untrimmed scales and the six trimmed scales (all experiments combined). For the scales that did need trimming of outliers, the sample was reduced in size by as little as 15% in the case of the 'confidence' scale, but by as much as 89% in the case of the 'sobriety' scale, thus yielding an exceedingly diminished *n* of 30 for the latter scale. The sobriety scale was, therefore, deemed unsuitable for further analysis because of the extreme trim.

The next largest trim of the sample was for the simplicity of apparatus scale, with a trim of 55%. However, with an *n* of 120 participants, it was deemed still suitable for further analysis. This trim may not be so severe, and in support of this decision it is noted that Hedges (1987, cited in Radin & Ferrari, 1991, p. 71) trimmed 45% of a database of studies in order to achieve a homogeneous distribution of effect sizes. By that standard, all other trims in the present study were regarded as acceptable since the third largest trim was only 23% for the 'fitness' scale).

Table 11.2
Descriptive Statistics: States of Mind Scales (Homogeneous Data set)

Visual Analogue Scale	*N*	Mean	*SD*	Skew	*SE*
Simplicity of Apparatus (145 excluded cases: scores of 2, 4, 5, 7, 11 or 15)	120	24.09	10.06	0.39	0.22
Ease of Task (35 excluded cases: scores of 36, 38 or 39)	230	26.96	8.94	–0.29	0.16
Freshness (no excluded cases)	265	22.85	9.72	–0.02	0.15
Sobriety (235 excluded cases: scores of 33, 36, 37, 38, 39 or 40)	30	21.50	10.76	–0.63	0.43
Attentiveness (50 excluded cases: scores of 33)	215	28.18	9.23	–0.32	0.17
Confidence (39 excluded cases: scores of 33 or 39)	226	27.76	8.86	–0.22	0.16
Fitness (60 excluded cases: scores of:33, 36 or 39)	205	23.92	10.07	–0.09	0.17
Energy level (no excluded cases)	265	22.17	9.75	0.01	0.15

11.3.2 Psychological Hypotheses

Hypothesis 1: There will be differences between the four experiments on perceived simplicity of the experimental apparatus. Table 11.3 gives the descriptive statistics of perceived simplicity for the four experiments. Note that the Vision-Impaired experiment was rated the simplest as far as apparatus and mechanical set-up were concerned and the Skeptics experiment was rated the most complex.

The Levene statistics showed that the homogeneity of variance rule was not violated (p = .143). The ANOVA test showed that there was a significant difference on simplicity between the four experiments, $F(3, 119) = 15.88$, $p < .001$. The post hoc Tukey's test showed that the differences were between (i) the Skeptics and *I Ching* experiments ($M_{diff.}$ = 11.13, $p < .001$), (ii) the Skeptics and Vision-Impaired experiments, ($M_{diff.}$ = 14.39, p = .001), and (iii) the Skeptics and Gambling experiments, ($M_{diff.}$ = 13.17, p <.001).

Table 11.3
Perceived Simplicity Scores for the Four Experiments

Experiment	Means	*SD*
Vision-Impaired (n = 7)	28.57	11.97
Gambling (n = 64)	27.36	8.76
I Ching (n = 22)	25.32	7.59
Skeptics (n = 27)	14.19	7.93

Note: Sizes of n are reduced due to trimming

The Skeptics experiment was perceived to be the least simple (i.e., most complex) experiment of all, and any differences between the other three experiments appeared to be due to chance. The results of this hypothesis are pertinent to Hypothesis 11.

Hypothesis 2: There will be differences between the four experiments on perceived ease of the paranormal task. Table 11.4 gives the descriptive statistics on ease of task for the four experiments. Note that the Gambling experiment was rated the easiest to do as far as procedure is concerned and the Skeptics experiment was rated the most difficult to do.

Table 11.4
Ease of Task Scores for the Four Experiments

Experiment	Means	*SD*
Skeptics (n = 102)	30.55	8.29
Vision-Impaired (n = 11)	28.45	10.05
I Ching (n = 28)	25.29	6.84
Gambling (n = 89)	23.18	8.53

Note: Sizes of n are reduced due to trimming

The Levene statistics showed that the homogeneity of variance rule was not violated (p = .192). The ANOVA test showed that there was a significant difference on ease of task between the four experiments, $F(3, 229) = 12.97$, $p < .001$. The post hoc Tukey's test showed that the differences were between (i) the Skeptics and *I Ching* experiments ($M_{diff.} = 5.26$, $p = .017$), and (ii) the Skeptics and Gambling experiments, ($M_{diff.} = 7.37$, $p < .001$). The Skeptics experiment was perceived to be the easiest experiment to do, and any differences between the other three experiments appeared to be due to chance. The results of this hypothesis are pertinent to Hypothesis 12.

11.3.3 Parapsychological Hypotheses

Hypotheses 3 to 10: For the four experiments, it was proposed that there are positive relationships between psi success and all but one of the states of minds scale scores (i.e., 'sobriety'). Since the sobriety scale was excluded from the planned analyses, only seven scales were tested for each experiment. Thus, a total of 28 Pearson *r* tests were run. The results are presented in Table 11.5.

Table 11.5
Correlations Between SOMS and Psi Hitting for the Four Experiments

Visual Analogue Scale	*I Ching*	Vision-Impaired	Gambling	Skeptics
Simplicity of Apparatus	$r(20) = –0.19$	$r(5) = –0.207$	$r(62) = 0.10$	$r(25) = 0.23$
Ease of Task	$r(26) = –0.13$	$r(9) = 0.04$	$r(87) = 0.17$	$r(100) = –0.18$*
Freshness	$r(28) = 0.30$*	$r(11) = 0.17$	$r(96) = –0.04$	$r(122) = –0.11$
Attentiveness	$r(25) = –0.11$	$r(11) = 0.13$	$r(86) = –0.05$	$r(85) = –0.15$
Confidence	$r(24) = 0.32$*	$r(10) = 0.31$	$r(86) = 0.06$	$r(98) = –0.16$*
Fitness	$r(25) = 0.01$	$r(11) = 0.14$	$r(85) = 0.13$	$r(76) = –0.18$
Energy Level	$r(28) = 0.20$	$r(11) = 0.11$	$r(96) = 0.06$	$r(122) = –0.11$**

Note: All probabilities are one-tailed
* $p \leq .05$; ** $p \leq .01$

Table 11.5 shows that only two correlations with psi hitting were positive and significant (freshness and confidence) and these were both in the *I Ching* experiment. Two significant correlations in 28 tests are 7% of all tests run. Correlational analyses of the seven scales revealed all but the simplicity of apparatus scale correlated positively and significantly with each other, with *r* values ranging between 0.40 and 0.80 ($p < .001$). This

low outcome of 7% may therefore be the result of the fact that the scales are not independent variables. An inspection of Table 11.5 also shows that 16 of the 28 correlations (57%) were in the directions hypothesized, which is about chance.

Note that the size of *n* in as many as 7 of the 28 tests was quite small (see the Vision-Impaired column in Table 11.5), so that the nonsignificant results in those 7 tests may have been artifacts due to inadequate sample size. Those 7 tests may have been redundant. On that basis, the success rates may be more favourable to the psi hypothesis with 2 in 21 tests (approximately 10%) positive and significant, and 10 in 21 (48%) tests in the direction hypothesized. Three significant correlations were not in the directions hypothesized—(see Skeptics column in Table 11.5).

It was stated above (see *11.1.2*) that freshness may be a state while energy level may be a trait. It was decided to test these two variables for construct overlap. They correlated so highly, $r(263) = 0.80$, $p < .001$, that either energy level or freshness might well have been omitted from the above analyses. There is, however, no grounds for deciding which of these two should be omitted, and the fact that freshness correlates significantly with the psi task (see Table 11.5), whereas energy level does not, suggests that there may still be some difference between the two. Thus, an arbitrary decision, based purely on hindsight, to omit energy level from the above analyses is unwarranted, so those analyses have been retained.

Hypothesis 11: There is a positive relationship between mean scores on simplicity of apparatus and mean scores on the paranormal tasks (across four experiments) expressed as a proportion correct (P). Table 11.6 shows hit-rates, proportions correct, and mean complexity scores for the four experiments (values of *n* are trimmed—see Table 11.5). Since the data set was trimmed, those participants not excluded in the *I Ching* experiment produced the greatest proportion correct, and participants in the Gambling experiment produced the smallest proportion correct.

Table 11.6
*Trials, Hit-Rates, Proportion Correct, and Mean Simplicity Scores (*SD*)*

Experiment	Total Trials	Total Hits	Proportion Correct (*P*)	Mean Simplicity Score	*SD*
I Ching (*n* = 22)	22	8	0.364	25.32	7.59
Vision-Impaired (Sighted only; *n* = 7)	7	2	0.285	28.57	11.97
Skeptics (1st 50 RUNS only; *n* = 27)	1350	281	0.208	14.19	7.93
Gambling (*n* = 64)	320	66	0.206	27.36	8.76

The Pearson *r* test result *was* positive, but not significant, $r(3) = 0.39$, $p = .303$, one-tailed. The hypothesis was not supported, so that there is only the suggestion that perceived simplicity has a positive effect on task performance. Note that the small *N* of 4 does not provide a good indication of any real relationship. Nevertheless, this exercise was necessary in showing that perceptual differences across experiments may influence psi performance.

Hypothesis 12: There is a positive relationship between mean scores on ease of task and mean scores on the paranormal tasks (across four experiments) expressed as a proportion correct (P). Table 11.7 shows hit-rates, proportions correct, and mean ease of task scores for the four experiments (values of *n* are trimmed—see Table 11.5). Since the data set was trimmed, those participants not excluded in the *I Ching* experiment produced the greatest proportion correct, and participants in the Gambling experiment produced the smallest proportion correct.

Table 11.7
*Trials, Hit-Rates, Proportion Correct, and Mean Ease of Task Scores (*SD*)*

Experiment	Total Trials	Total Hits	Proportion Correct (*P*)	Mean Ease of Task Score	*SD*
I Ching (*n* = 28)	28	11	0.393	25.29	6.84
Vision-Impaired (Sighted only; *n* = 11)	11	3	0.273	28.45	10.05
Skeptics (1st 50 RUNS only; *n* = 102)	5100	1058	0.207	30.55	8.29
Gambling (*n* = 89)	445	92	0.206	23.18	8.53

The Pearson *r* test result was not positive, $r(3) = -0.30$. The hypothesis was not supported. There was no evidence that ease of task had an effect on task performance.

11.3.4 Post Hoc Analysis

A Median Split Analysis was performed on freshness and confidence to determine if the either of them was a necessary condition. Using the *I Ching* data only ($n = 28$), the median score for freshness is 16. There were 15 high-scorers (> 16) with a significant overall hit-rate, $P_{obs.} = 0.53$ ($p = .017$). There were 15 low-scorers (< 16). The hit-rate for low-scorers was not significant, $P_{obs.} = 0.27$ ($p = .539$). This evidence suggests that freshness was a necessary condition that brought about hexagram hitting.

Using the same *I Ching* data only (n = 24), the median score for confidence is 28. There were 11 high-scorers (> 28) with a nonsignificant overall hit-rate, $P_{obs.}$ = 0.45 (p = .115). However, this very high hit-rate, translates as an effect size π = 0.71, which has a Z of 2.49, p = .006 (using Rosenthal & Rubin's, 1989, p. 334, formulae). There were 12 low-scorers (< 28). The hit-rate for low-scorers was not significant, $P_{obs.}$ = 0.25 (p = .649). This evidence suggests that confidence was a necessary condition that brought about hexagram hitting.

11.4 Summary of Findings: Chapters 5 to 11

The four experiments conducted in this thesis have produced a panoply of results that need summarizing in order to come to some overall conclusions about the respective investigations. (Note that the focus is on paranormal findings only.) Table 11.8 lists the four experiments and the psychopractic propositions investigated in those experiments. Based on evidence from the respective chapters, each proposition is rated dichotomously in terms of whether there was any evidence to support it. Table 11.9 lists all the hypothesized necessary conditions tested in the five studies, with dichotomous ratings for each condition in terms of whether there was evidence that each condition was necessary and sufficient in bringing about paranormal effects in the respective samples.

Table 11.8
The Four Experiments: The Major Propositions of the Theory of Psychopraxia

Experiment	Major Psychopractic Propositions	Evidence	
		Yes	No
I Ching	ESP-PK dichotomy is unworkable (see *5.7.5*)	✓	
I Ching	Paranormal effects require a pro attitude (see *5.7.5*)	✓	
I Ching	Psychopraxia is a cybernetic process (see *5.7.5*)		✓
Gamblers	Pro attitudes can be mutually opposed (see *7.7.2*)	✓	
Skeptics	The pro attitude is changeable (see *8.4.4.4*)	✓	
Vision-Impaired	Psychopraxia compensates for impairment (see *9.4.2* & *9.4.3*)		✓
Totals (4 + 2 = 6)		4	2

Table 11.9

The Five Experiments: 'Necessary' Conditions for Bringing About Paranormal Effects

Experiment (Dependent Variable)	Necessary Condition (Independent Variable)	Evidence Yes	Evidence No
I Ching (Hexagram Hitting; see *6.3.4* and *6.3.6*)	Transliminality	✓	
	16PF Factor F	✓	
	16PF Factor H	✓	
	16PF Factor Q_2		✓
	16PF Factor Q_4	✓	
	16PF Factor EX	✓	
	16PF Factor IN	✓	
I Ching (Changing Lines; see *6.3.5* and *6.3.7*)	Transliminality		✓
	16PF Factor A		✓
	16PF Factor C		✓
	16PF Factor M		✓
	16PF Factor Q_2		✓
	16PF Factor EX		✓
	Belief in 'Possibility'		✓
Gambling (Aces of Spades; see *7.7.2*)	GAS (General Gambling)		✓
	GAS (Horse racing)		✓
	GAS (Casino)		✓
	GAS (Lotteries)		✓
	BIGL		✓
	SL-TDI Type Mode: EN		✓
	SL-TDI Type Mode: IN		✓
Gambling (Aces of Clubs; see *7.7.2*)	GAS (General Gambling)		✓
	GAS (Horse racing)	✓	
	GAS (Casino)		✓
	GAS (Lotteries)		✓
	BIGL		
	SL-TDI Type Mode: EN		✓
	SL-TDI Type Mode: IN		✓
Skeptics (Zener Symbol Hitting; see *8.4.3*)	Belief in ESP-PK (Believers)		✓
	Belief in ESP-PK (Converted Skeptics)	✓	
Vision-Impaired (Picture Targeting; Participants' ratings—see *9.4.2*)	Relaxation (whole sample)		✓
	Relaxation (vision impaired)	✓	
	Relaxation (sighted)		✓
	Belief in ESP (whole sample)		✓
	Belief in ESP (vision impaired)		✓
	Belief in ESP (sighted)		✓
Vision-Impaired (Picture Targeting; Independent Judge's ratings—see *9.4.3*)	Relaxation (whole sample)		✓
	Relaxation (vision impaired)		✓
	Relaxation (sighted)		✓
	Belief in ESP (whole sample)		✓
	Belief in ESP (vision impaired)		✓
	Belief in ESP (sighted)		✓
States of Mind Scales (SOMS) (All paranormal tasks; see *11.3.3*)	Perceived simplicity of apparatus		✓
	Perceived ease of task		✓
	Freshness	✓	
	Attentiveness		✓
	Confidence	✓	
	Fitness		✓
	Energy Level		✓
Totals (11 + 37 = 48)		**11**	**37**

Table 11.8 shows that four of the six psychopractic propositions were supported. While the results are reasonably favourable, the downside of the results is that there is no evidence that (a) psychopraxia is a cybernetic process, and (b) psi can have a compensatory effect. Evidence for both would have shown that (a) the pro attitude is mutable, and therefore, that participants appear to be causally involved in directing the psi process, and (b) normal and paranormal processes are opposite in degree, not kind—compensation would be diasomatic psychopraxia at work.

Table 11.9 shows that 11 of 48 conditions (23%) were statistically supported as necessary in bringing about paranormal effects. While this success rate is relatively high for parapsychology, the failure rate (77%) is quite high by any standard. The major contributors to this failure rate come in the form of nonsignificant correlations of number of changing lines with various 16PF factors (as tested in the *I Ching* experiment) and nonsignificant correlations of Aces of Spades (and Aces of Clubs) with GAS subscales, the BIGL scale, and the SL-TDI subscales (as tested in the Gambling experiment). This high rate of failure, however, may be grossly inflated since many of the correlations may be nonsignificant as a result of nonindependence of the relevant variables (for example, see *7.7.3*, Table 7.4).

11.5 Discussion

11.5.1 The States of Mind Scales

Eight measures were used in the present study, which comprise the states of mind scales. The scales consist of eight measures of dimensional mental states, each represented as a bipolar visual analogue scale. The dimensions are as follows: (i) weary/fresh, (ii) intoxicated/sober, (iii) distracted/attentive, (iv) uncertain/confident, (v) out-of-shape/fit, (vi) tired/energetic, (vii) simple/complex, and (viii) easy/difficult.

These dimensions were converted to general states as follows: (i) freshness, (ii) sobriety, (iii) attentiveness, (iv) confidence, (v) fitness, (vi) energy level, (vii) simplicity of apparatus, and (viii) ease of task. It was hypothesized that there would be positive relationships between paranormal performance and seven of these states of mind (which excludes sobriety). It was also hypothesized that there would be differences on simplicity scores, and ease of task scores, between the four experiments. There was only limited support for these hypotheses.

In Hypothesis 1, a significant difference was found between the Skeptics experiment and the other three experiments on simplicity scores. Assuming that judgements made by participants about the four experiments are homogeneous across the four experiments, and assuming adequate randomization in the experiments, it is evident that participants perceive differences between experiments in terms of complexity. These perceived differences are not chance fluctuations, but are attributable to participants' apperceptions about the apparatus (i.e., the mechanical set-up) and the procedures involved in these experiments, regardless of the fact that actual complexity may not exist by other, more objective standards or measures (e.g., as measured by time constraints, physical and mental exertion, degree of concentration and focus, IQ or EQ levels, etc.—see also **10.2**).

Research into the complexity issue has produced mixed results (e.g., cf. Schmidt, 1973, with Schmidt & Pantas, 1972). However, some researchers (Kennedy, 1995; Stanford, 1990) believe that complexity has no effect on participants' performances on psi tasks. There is only suggestive evidence that the most complex tasks appear not to affect performance, but this research focused mainly on RNG experiments. If there comes to be more consistent evidence that this is the general case for all types of psi experimentation, there is still, however, no good evidence that perceived complexity will have anything to do with those performances until participants are actually asked what they think about the apparatus and the procedures.

If actual complexity is concealed from participants, they can only judge from the appearance of things, and given the weak and elusive nature of psi, there is no guarantee that a majority of participants will find out paranormally just how complex the apparatus really is. For the most part, participants will make conscious judgements about complexity, and they may well perceive two experiments, though vastly different in complexity, to be about the same in complexity, and such a perception may well result in matched performances on both experiments. In fact, the suggestive result in relation to Hypothesis 11 may mean that paranormal performance from experiment to experiment is modified by perceptions of complexity, and that the experiment itself has a key influence in determining paranormal performance. Hypothesis 11, however, was tested with a small N of 4, which is insufficient data to expect that the relationship between complexity and psi might be a significant one, or that the test of the hypothesis was even reliable and valid, but the positive effect (though weak to moderate; $r = 0.39$) was in the direction hypothesized. The result is presented for further consideration of a most important issue, that of experimental design and its relationship to psi.

In Hypothesis 2, a significant difference was found between the Skeptics experiment and the other three experiments on ease of task scores. Ironically, the results of testing Hypotheses 1 and 2 suggest that the perception of complexity may not have an influence on how easy participants thought the task would be. In particular, the Skeptics experiment was rated the most complex, but this fact did not seem to disturb participants at all. On the contrary, the Skeptics experiment was rated the easiest to do. Thus, it cannot be taken for granted that there is a commonsense relationship between complexity and ease of task. Once again, Stanford's (1978) words are highly appropriate: the "complexity issue is itself complex" (p. 205).

The parapsychological Hypotheses 3 to 10 concerned relationships between paranormal performance and each of the SOMS. Some evidence was found that the two states of mind, freshness and confidence, help modify paranormal outcomes, but the evidence was not consistent across the four experiments as the correlations were only significant in the *I Ching* experiment. No evidence was found that the other five states of mind (which exclude sobriety) are conducive to psi. Finally, in terms of the theory of psychopraxia, evidence supporting the hypothesis that there are necessary conditions for bringing about paranormal effects applies only to freshness and confidence in the *I Ching* experiment.

11.5.2 Summary of Results (Chapters 5 to 11)

The results of the four experiments featured in the present thesis, as well as the study featured in the present chapter, were tabled and evaluated. Regarding the six psychopractic propositions, the ESP-PK dichotomy was found to be problematic. Evidence was also found that pro attitudes are concerned with achieving goals. There was evidence that there may be conflict between opposable pro attitudes (i.e., compliant and noncompliant pro attitudes), and there was post hoc evidence that a primary pro attitude could be changed. There was little evidence that the diasomatic hypothesis is correct, since compensation of an adversely affected normal process (i.e., vision) by a paranormal process could not be shown.

In terms of conditions necessary for bringing about paranormal effects, the evidence suggests that chance alone does not explain quite a number of relationships (in fact 23% of relationships hypothesized in the present thesis—see Table 11.9). While moves towards establishing an understanding of process-oriented psi have been made in the present thesis, the results are not altogether groundbreaking. Where the findings are in the affirmative, they tend mostly to be confirmatory of past findings (e.g., belief, relaxation

and extraversion, or components of extraversion), and where the findings are in the negative, they tend to be for variables not previously tested in the literature, as far as paranormal effects are concerned (e.g., the GAS subscales, the SL-TDI type modes: EN and IN; and the SOMS). However, testing all these variables as hypothesized 'necessary' conditions is an altogether new approach to process-oriented research, and the 'successes' in Table 11.9 suggest that for samples in general (but certainly not for all participants) there are clearly conditions that were not only necessary, but were also sufficient in bringing about paranormal effects.

In Chapter 12, conclusive statements about the theory of psychopraxia will be made in terms of the theoretical and experimental findings of the present thesis, and the major advantages and disadvantages of the theory will be discussed, with advice on modifications of, and/or additions to, the theory as also determined by the findings. Recommendations for future research will be made.

CHAPTER 12
DISCUSSION

He who dwells in all beings but is separate from all beings, whom no being knows, whose body all beings are. . . . He, the Self, is the Inner Ruler, the Immortal.
The Upanishads (cited in Edinger, 1996, p. 163)

[Parapsychology] is trying to prove that consciousness really does have power.
Susan Blackmore (2001b, p. 27)

12.1 Overview of the Discussion

In this last chapter, a two-stage analytical assessment of the theory of psychopraxia is undertaken to ascertain its viability as a useful theory for parapsychology. First, psychological, philosophical, theoretical, and practical assessments of the theory will be conducted, based on issues raised, and findings reported in the present thesis. The psychological, philosophical, and theoretical issues related to the theory will involve a consideration of the definitions of self, pro attitude, necessary conditions, goal orientation, and the three ontologies (Materialism, Dualism, and Idealism). Clarification and evaluation of each of these concepts is necessary to come to a conclusion about the merits of the theory of psychopraxia. The practical assessment is based on the experimental findings (Chapters 5 to 9).

Second, an appraisal is made of the theory of psychopraxia in terms of its possible contribution to parapsychology. As no theory can exist in a vacuum, this task involves a comparison of the theory with two conceptually similar paranormal theories. These are Jung's (1960) synchronicity theory, and Stanford's (1978, 1990) psi mediated instrumental response (PMIR) model. Although a comparative approach necessarily exposes the potential strengths and weaknesses of all three theories under scrutiny, it will ultimately and ironically yield an objective assessment of the theory of psychopraxia from the perspective of comments made about the other two theories under scrutiny. It will be seen that there is some complementarity between these theories (i.e., some degree of agreement *about* certain theoretical aspects of paranormal phenomena), but there is some disagreement as

well. It will be shown that different theories can lead to different interpretations of the proposed phenomena, as was argued in Chapter 1 (see *1.3.4*).

Third, based on all these findings, the present chapter will conclude with a consideration of future paths for research and experimental investigation that may prove fruitful for the theory of psychopraxia and for the field of parapsychology.

12.2 Psychological and Philosophical Considerations of the Theory of Psychopraxia

12.2.1 The Self

Thalbourne (2000a) has referred to the self throughout his monograph. Before dealing with the psychopractic aspects of the self, a distinction must be made between two types of self commonly referred to in the literature, often under very different guises. These distinctions follow Western empirically based models, rather than those drawn from, say, Eastern introspectively derived models that have not been experimentally tested. First, there is the *empirical* self, also referred to as the ego, or the 'I', by the psychoanalytic schools (cf. Freud, 1949; Jung, 1987). The empirical self is mental (i.e., of the psyche) rather than corporeal. It is characterized by its central relationship with consciousness, that is, it has a pivotal role as sole arbiter of experience. This self, also referred to as ego, has acknowledged links with the body and body consciousness, as indicated in the early stages of psychoanalytic history, by Adler (1928) and Freud (1949), and later by the learning theorists Miller and Dollard (1941).

Experimentally, this self is empirical because it is verifiable, having been tested using all manner of measures, such as attitude scales, IQ tests, personality tests and the like (cf. Reber, 1985). The empirical self, therefore, has reached general acceptance, whether taken as a construct or entity, simply because it is empirically verifiable as the sole agent of thought and deed. More recently, the empirical self has encouraged much speculation as to its constructed nature and purpose (cf. Markus & Kitayama, 1991; Neisser, 1988; Spinelli, 1989).

Second is the broader concept of self that is hypothesized to extend beyond the limits of the ego, and is therefore not entirely empirical because it is claimed to have an unconscious component. Like the concept of the ego, this self too originates in the psychoanalytic schools (again, see Freud, 1949; Jung, 1987). For Freud (1949), the self is a composite of the ego, superego, and the id, therefore coming close to Jung's (1987) self,

which is a concept that "designates the whole range of psychic phenomena in man [*sic*]" and "expresses the unity of the personality as a whole" (para. 789). This self is "*potentially* empirical" because of the unconscious component, also referred to as the *Cognitive Unconscious* (cf. Parker, 2001, p. 233). As a concept, no direct equivalence can be made with the psychoanalytic self and the corporeal body, and Jung, for example, only makes reference to the relationship of self with body when he refers to the influence of the self at the 'organic level' (i.e., on the organs of the body). Note that the concept of, and evidence for, psychosomatic illness extends from this hypothesized relationship.

Conventional psychology (for recent examples of its representatives, see again Markus & Kitayama, 1991; Neisser, 1988; Spinelli, 1989) has always been criticized by 'depth' psychologists (cf. Edinger, 1996; von Franz, 1980) for failing to venture beyond the realms of consciousness into the domain of this other aspect of the self, by assuming that there is only the individual and his/her choices made in consciousness. This attitude appears to eliminate the need for any further debate about other possible determinants of action and thought. In fact, it is possible that these determinants have sources within the same mental or brain structure as consciousness, but nevertheless remain unconscious to the individual. The outcomes of unconscious processes suggest this other, hidden component of the self (for examples, see Edinger, 1996).

As far as the theory of psychopraxia is concerned, Thalbourne (2000a) regards the self as the causal agent of psychopractic action, but his statements about the self sometimes appear ambiguous, so that it is not always clear where this action supposedly comes from. Thalbourne refers to:

(i) the self as 'ego': "psychopraxia . . . [is] a principle underlying all actions between the self, or ego . . ." (Thalbourne, 2000a, p. 76). It is not clear in the text whether "self, or ego" in this context means 'either one or the other', or that it means 'self, which is to say, ego' (that is, both self *and* ego). If the latter is meant, this self would be the empirical self *qua* the psychoanalytic tradition, which is what Thalbourne (personal communication, December 12, 2001) had in mind.

(ii) the self as 'I': "let us simply be content with the designation of the self as the 'I' of personal identity" (Thalbourne, 2000a, p. 102). Tentatively, this self could also be the ego, given that 'I' in Latin is *ego*, which might refer to the empirical self, or it may refer to the total personality, or perhaps the person as physical being, but see (iii) next.

(iii) the self as a 'transcendent figure': "the self is a metaphysical entity" (Thalbourne, 2000a, p. 103). Thalbourne hypothesizes that this self may be able to "survive the death of the physical body" (p. 103), and that it must therefore exist independently of the body. It is beyond the scope of the present thesis to propose whether self as ego or 'I' may survive bodily death, but Thalbourne seems to imply it in (i) above, *if* he equates self with ego, and in (ii) above where he does equate self with 'I'. It is not clear whether this self includes the unconscious domain.

(iv) the self as other than 'mind': "the self . . . [is] the witness of mental and physical events, not to be identified with mind" (Thalbourne, 2000a, p. 102). This self, appears to be conceptual, like the mind, but is not the mind. It is not clear how this self should be able to "witness" itself, that is, *apperceive* (which is a mental event and therefore of the mind), but apparently the distinction derives from introspective experience (2000e, p. 1). To clarify, Thalbourne (personal communication, December 12, 2001) claims that the self associates with the mind, and he (Thalbourne, 2001) broadens out this concept by identifying the self with a heightened state of consciousness distinct from the mind, but infused with it. This self may therefore include the unconscious domain, but would not be the empirical self only.

Thalbourne (2000a) also talks about the 'person': "a person may be said to have a pro attitude . . ." (p. 65) and "the *person* is 'causally central' in all types of psi event" p. 68). Like the self, the person is considered a causal agent, but if the two are equated, then self transgresses, or crosses over into, the domain of the body. This 'equation' is not problematic if it is accepted that Thalbourne does not mean the psychoanalytic 'selves' (either type), but means, all along, that *the self is the person*, as in the vernacular statements: 'I hold my*self* responsible for my actions," or "I am not my*self* today" (cf. Thalbourne, 2001, p. 4). In that case, the psychoanalytic empirical (or *potentially* empirical) self would certainly not be equated with the person as a "living body" or an "individual human being," by which is meant a physical body (Hughes, Michell, & Ramson, 1992, p. 844). However, this conclusion undermines (iii) above, which has self as a metaphysical entity that can survive the body and therefore cannot be the body.

Elsewhere, Thalbourne (2000a) refers to the person's "motives, desires and goals" (p. 65) in consciousness, as well as those that are "*less* conscious" (p. 65) all of which may

predispose the "organism to prefer some state **S** rather than **–S**" (p. 66). Here the (biological?) organism would be equated with the physical body, and therefore the person, which appears to *incorporate* the construct of the self and its varying levels of consciousness. There is no real contradiction here, but Thalbourne is now definitely talking about either the *potentially* empirical self of the psychoanalytical schools, or the metaphysical self of (iii) above. To support that conclusion, Thalbourne (2000e) later makes "a distinction between the self, and the mind and body" (p. 1).

Thus, there is a degree of ambiguity in some of the above. It seems, at one time or another, that all 'selves' (with no reference to the mind) may be the cause of psychopractic action. Thus, from the perspective of locating the *source* of action, the 'psychopractic subject' could be either (a) the body of the individual (i.e., the biological organism along with its inherent needs, desires, and drives), (b) the empirical self as ego, or 'I', or even (c) the "*potentially* empirical" self that is inclusive of the unconscious. It is not clear why the mind is not capable of initiating action (note that in an update of the definition of the self, Thalbourne, 2001, again makes no reference to the mind as a possible source of psychopractic action). Of course, the self may be all three, (a), (b), *and* (c), and in some sense, they can be seen as collapsing together as an integrated whole for the most part, with the differences existing only in discourse. However, the ambiguity is still there when the experimenter is asked to consider who or what Thalbourne's (2000a) preferred term "agent" (p. 70) actually refers to, even though Thalbourne is certain that the agent is the 'I' who, in his example, "set[s] off the process" of information-acquisition (p. 72, n72).

Further consideration reveals that it is not adequate to dispense with all the above terms and speak only of agents, experimental participants, individuals, or even human beings. More recently, Thalbourne (2001) even points out the poverty of such nebulous terms as "individual" and "organism" (p. 3). This ambiguity is not unlike the problem that arises when we ponder whether a test of a person's sporting ability tells the same thing about the *processes* of the 'self' as does a test of that person's intelligence. Clearly, one and the same participant (or agent, if preferred) is being tested in both cases, but the causal factors underlying both organic processes are not the same, thus indicating that intelligence is *not* causally located in the musculature, just as physical strength is *not* causally located in the brain. In selecting a student for higher education by their intelligence alone, or selecting a player for a football team by their strength alone, these issues do not matter, but experimentally, when limits to agency must be set, self does not denote a subject or an object of investigation. In such cases, the agent of the self as causal factor of both physical and mental processes, no matter how well defined (cf. Thalbourne, 2001, pp. 3-6), does not

inform our knowledge of process. Certainly, an appeal to a self that is, or may be, transcendent only shifts causal agency even further away from experimental bounds, beyond the falsifiable.

To clarify this point, if psychopractic action has an agent; if a pro attitude can be held; if some entity or other can hold a goal, the experimenter needs to be sure what he or she is testing from one moment to the next in a given experiment (e.g., mind, or autonomic systems, or semi-autonomic systems, or executive functions of the brain, which may or may not equate with ego functioning, and so on). If the specific causal components that underlie agency cannot be identified during an experiment, the experimenter can never claim knowledge or understanding of (say) paranormal process. Thus, the problem is not merely philosophical or discursive, but the self as a concept, as useful as it has been in establishing a dimension to personality heretofore undiscovered and now unbounded, has created a dilemma in experimental research. This is not to criticize the theory of psychopraxia because the problem is broader than that. In parapsychology, knowing that the potential of the participant may be unbounded does not help isolate the cause of paranormal processes, but merely increases the uncertainty of inner processes. Self as causal factor underlying psychopractic action can only be a working hypothesis until the experimenter can really point to a more sharply defined centre of the cause of action.

12.2.2 The Pro Attitude

The concept of the pro attitude has had considerable experimental treatment in the present thesis, but the results of these experiments will not be discussed here (see *12.4.2*). Instead, only the definition of the pro attitude will be considered. To restate the definition of the pro attitude: "a person may be said to have a pro attitude towards state **S** when they would consciously prefer **S** rather than **–S** [not **S**] *if* those two alternatives were to be brought to their attention" (Thalbourne, 2000a, p. 65, and also Thalbourne, 2000e, p. 1). In subsequent publications, the word "consciously" was deleted from the definition (see Storm & Thalbourne, 2000a, p. 280; 2001b, p. 117; Thalbourne, 2001, p. 6). This deletion was a necessary step, since conscious preference may not always be involved in psychopraxia. Thalbourne recognized the fact that an unconscious pro attitude may exist, which may remain forever unconscious (supposedly **–S** as far as the person is concerned, *were it conscious*, but actually **S** as far as the unconscious self is concerned).

This unusual state of affairs may be more common that might at first be assumed. For example, a consciously preferred pro attitude to win at a gambling task may be usurped by an unconscious pro attitude to lose (see Chapter 7). This situation may be typical of the

compulsive gambler who simply cannot prevent himself or herself from indulging in games of chance in which he or she is fated to fail due to an unconscious pro attitude towards making unwise decisions in situations where wiser decisions are possible (e.g., safe betting, knowing when to stop. Cf. Ibanez et al., 2001, who found that "gamblers with comorbid psychiatric disorders had gambling scores and psychological scale scores indicating greater severity of gambling and psychopathology," p. 1733).

Since some individuals are defined by their failures, it may be more important for them to fail and preserve what little identity they have, than succeed and risk an identity crisis (cf. Ingram, 1985, who claims that the "losing behavior of pathological gamblers serves to fulfill a life script as a loser. . . . [The] script is a decision . . . made early in life to accommodate to parents perceived as threatening and omnipotent. The decision, once made, is carried unwittingly into later life and is reenacted repeatedly," p. 89). Given psi, such failure may be more often than chance would explain (i.e., what is traditionally called psi missing). Thus, a pro attitude towards failure may be explained by the possibility that reinforcement can take forms other than cash prizes. It must, therefore, be acknowledged that pro attitudes may be intimately enmeshed with concepts involving issues of self-image and self-esteem (or lack thereof), and the maintenance of these concepts (recall the claim made of the pro attitude in Chapter 8 where skeptics' disbelief in psi governed their paranormal performance—see *8.2.1*). The pro attitude may be adjusted to meet the criteria of *a priori* prerequisites that are unconscious. An unconscious pro attitude for failure may be stronger than a conscious pro attitude for success. Thus, the 'person' may consciously prefer success, but the self may not.

To continue this train of thought, Thalbourne (2001) has thus located the cause of action in the agency of a self-system where conscious and unconscious or "less conscious" processes can be applied. Thus, he considers the approach-avoidance conflict, where "a pro attitude to *achieve* a desired goal state" may be in conflict with "a pro attitude towards the absence of that achievement" (p. 7), as driven by, for example, fear of psi. (Note that Kennedy, 2001, has listed fear of psi as a prime possible cause of the elusiveness of psi.) This conflict is key in determining the final psychopractic outcome. In addition, there is the possibility that pro attitudes may come from sources other than the participant (e.g., the experimenter, or group of researchers involved in the experiment, or persons aware of the research). Relevant to this claim is Kennedy's (2001) hypothesis that the elusiveness of psi is caused by the "net result" of "all the people who eventually learn about or would have an interest in a psi result" (p. 234). These people could have positive or negative effects on psi.

Since, Thalbourne (2000a) makes it clear that conscious *and* unconscious processes underscore the pro attitude, the self he refers to must be the "*potentially* empirical" self, defined as "the whole range of psychic phenomena" in the individual, which is taken to be the unified or whole personality (Jung, 1987, para. 789). Therefore, as comprehensive as the above definition of the pro attitude is in its current form, it is only accurate insofar as the "person," *which incorporates the potentially empirical self*, is the causal agent. In fact, the actual agent may be the unconscious aspect of the self, while the person (unconscious of any hidden agenda) may be a mere proxy to the self. This specification of actual agency is only tacit in the current definition of the pro attitude.

From another perspective, there is a commonsense aspect to the term pro attitude that defies criticism. On the basis that the pro attitude is itself the primary necessary condition that defines the causal chain that ends in goal achievement, it is hard to imagine how an organism could act of its own volition in the paradoxical situation of it not being the agent of its own goal achievements (notwithstanding the possibility of experimenter effect!). If it does not decide, devise, implement, or design, its own actions and goals, who or what does?[93] Given the implication of the concept of the pro attitude in terms of its relevance to causal factors, it is not surprising that the idea of the 'pro attitude' has its origins in philosophy. Thalbourne admits borrowing the term pro attitude from Davidson (1966), who applies it in a common sense way, as does Thalbourne. Apart from the common sense application of the term, Thalbourne (personal communication, November 16, 2001) borrows very little from the arguments put forward by Davidson.

Davidson (1966) coined the term 'pro attitude' to describe "someone [doing] something for a reason" (p. 221). The person thus has a "pro attitude towards actions of a certain kind" (p. 221). Davidson regarded the reason (i.e., he regarded *rationalization*) as an "ordinary causal explanation" in the sense that "reason *rationalizes* the action" (p. 221). Thalbourne (2000a) states that he uses the term in a different sense to describe "a positive preference for a particular end state of affairs" (p. 65, n4), rather than just an action. Thus, Thalbourne (personal communication, November 16, 2001) sees the person as having some sort of pro attitude towards a *goal* rather than towards an *action* that leads to a goal. For Thalbourne, this goal is only achieved if all the *necessary* conditions are present, the pro attitude being the first necessary condition in a series of conditions necessary in bringing about a goal.

[93] Note that Thalbourrne explains cases of poltergeists and RSPK ('recurrent spontaneous PK'), which appear to be causeless, as being due to unconscious pro attitudes (for examples, see Roll, 1977).

Davidson (1966) claims that: "Giving the reason why an agent did something is often a matter of naming the pro attitude" (p. 222). Naming the pro attitude, however, may be harder than Davidson thinks. He goes to great efforts to show how constructions in language can represent pro attitudes, and he points out the ambiguity that may arise, and the absence of *rationalization* that may be implied, if syntactical representations are poorly expressed. While such efforts are essential in philosophy, since philosophers depend on the precision of words (there not being other instruments and measures), the empirical psychologist may find great redundancy in this effort. For example, child psychologists will be well aware of the ongoing stream of non-syntactical desires, 'wantings', and needs entertained by the 'polymorphous perverse' pre-linguistic infant. Suffice it to say, the infant holds pro attitudes regardless of its inability to express them.

The only way the psychologist (or indeed, parapsychologist) can proceed in the enterprise of understanding more fully the pro attitude as a first condition is to adopt the idea that 'mental representations' (e.g., images, scenarios, etc.) can be pro attitudes too. In fact, it may be better to dispense altogether with philosophical 'syntax criteria' and describe a pro attitude, not by its representation in language, but by the implied form it may, or must, take in the experimental situation. In speaking of preferences, which take the forms of targets or goals, rather than word statements, Thalbourne has probably circumvented the verbal problems towards which Davidson's theory necessarily inclines. As has been proposed in the present thesis, the experimenter may only need to content himself or herself with two types of pro attitude (i.e., compliant and noncompliant; see Chapter 7), in the knowledge that the Aristotelean 'excluded middle' is guaranteed in that there is no other alternative.

12.2.3 Necessary Conditions

Following on from the pro attitude in the normal chain of psychopractic events, are the so-called "*necessary conditions*" needed to bring about the paranormal effect (Thalbourne, 2000a, p. 96). Thalbourne's definition of psychopraxia stipulates that "certain conditions" (p. 96) must be in place before a pro attitude can be fulfilled in reality. This component of the theory delivers the theory from out of the domains of 'magical causation' and 'teleological causation' into the realm of testable science. As long as it is possible to explain a paranormal result in terms of the conditions that brought it about, there is a demonstrable causal chain of conditions that allow for the establishment of predictable models of the psi process. Thalbourne (2001) emphasizes this aspect of the necessary condition by pointing out that "we need to assemble so far as possible the conditions

necessary to provide the sufficient condition" for exosomatic psychopraxia (p. 9). Each of these conditions is seen as causally relevant in bringing about an effect.

However, Thalbourne (2000a) notes too that these necessary conditions are not fully specified as yet, but points out that they are "probably psychophysiological" (p. 76). This proviso might be taken as a kind of disclaimer, thus introducing a complication into the theory. The investigator of psychopraxia is called upon to hypothesize and/or discover which (of possibly many) conditions must be sought out and tested as being necessary for a psychopractic effect. These conditions may be predictors, or moderating and modifying variables, or psi-conducive conditions, or they may even take the form of the negation of, or reduction in, the effects of certain psi inhibitive factors (see examples in *4.4.4* from Stanford, 1990). While the term 'psychophysiological' helps limit the investigator's search to the test participant, thereby excluding external factors, the human subject, together with all its typical and not so typical behaviours, is still too broad-based a domain for the search to be all that easily and fully accomplished. Usually a theory imposes certain restraints or parameters that restrict the investigator to specific lines of inquiry. Thus, a theory may only have a short 'shelf life' if a point is reached where there is absolutely no question that all of its formal expressions and terms have been tested and found wanting. The theory of psychopraxia may never meet this fate, not because it will ultimately be shown to be valid, but because it can never be wrong.

It might not be such a problem if a list of proposed necessary conditions were exhaustible, even though that might keep testing of these conditions going almost indefinitely, since this circumstance is true of many theories. For example, Einstein's theory of relativity was proposed near the turn of the 20th century, but some of its basic tenets were not proved until decades later. Delays in validation of a theory do not reflect badly on a theory. The shortcoming of the theory of psychopraxia is in the demands it places on the investigator. Thus, the researcher is required by the usual methods of scientific endeavour (such as intuition, deduction and induction, and even trial and error), to not only test hypothesized necessary conditions, but to find them as well.

Deficits like these might, however, simply be *de rigueur* for parapsychology given the exceptional and elusive nature of psi. Thalbourne (2000a) seems to think such a deficit is not out of the usual when he says: "It is, therefore, the task of parapsychologists to discover those conditions which are conducive to or which inhibit the production of the effect" (p. 98). And Thalbourne is not the first to propose an incomplete list of necessary, or psi-conducive, or psi-inhibitive conditions (again, see *4.4.4*). Some critics, however, might regard Thalbourne's request as an open admission of his falling short of the task of

theory making. One referee of a published article that first introduced the theory of psychopraxia to the parapsychological community (see Storm & Thalbourne, 2000a) thought psychopraxia was "better" described as a model of the psi process given that he or she was "unsure that psychopraxia is well-enough developed to merit the term theory" (Anonymous referee, personal communication, August 2, 2000).

One other problem that must be surmounted by the theory of psychopraxia is the degree to which it can or should be stated that a condition is truly necessary. W. G. Braud (personal communication, June 25, 2001) pointed out that relaxation is more like a psi-conducive condition than a necessary condition, and this is a reasonable assumption to make. Granted many psi experiments did not introduce relaxation as a treatment (see Table 9.2 above), but psi effects were achieved nonetheless. In other words, in those experiments, relaxation was psi-conducive, but not necessary, or even sufficient, so that it can never be claimed that relaxation is *always* necessary, or always sufficient. The difficulty with the use of the word 'necessary' stems from the fact that Thalbourne uses it to mean an ensemble of necessary conditions must be assembled to provide the sufficient condition. Under such circumstances, it becomes difficult for the investigator to make clear statements that any given hypothesized and tested psi-conducive condition is unequivocally necessary, in the sense that it is sufficient or not. It may only be circumstantially sufficient (see especially, *4.4.3*). Thus, as Delin (personal communication, November 30, 2001) points out:

> A "necessary" condition is one without which an outcome cannot occur. Its presence by no means guarantees the outcome. A "sufficient" condition is one which, even if it is not necessary, *does* guarantee the outcome. Of course, some conditions can be both. A condition may be "conducive to" an outcome without being either necessary *or* sufficient.

It should be noted, however, that in the sense that a psi effect was made stronger, it was *necessary* to have the psi-conducive condition. To use relaxation again as an example, it was not demonstrated *beyond a shadow of a doubt* in the Vision-Impaired experiment (see Chapter 9) that relaxation brought about psi or lowered the heart rate of participants. Reasons were given for these failures (see *9.6.3*), but it must be stated that the general finding for relaxation based on the literature (see *9.1.3*) is that, while it has been shown to be psi-conducive, it has never been shown to be *always* necessary.

12.2.4 Goal Orientation

The term goal-orientation is used to describe psi as a teleological process—a process that involves an end or goal that is achieved. For Kennedy (1995), the "basic goal-oriented psi hypothesis assumes that psi phenomena (a) depend on a person's motivation for or benefit from the outcome of a random event, and (b) do not depend on the complexity or information-processing aspects of the random process" (p. 47). Theories that fulfill these criteria are described by Irwin (1999, pp. 168-170) as "noncybernetic," that is, they "abandon the notion of information flow" (see *5.1.2*). Irwin has (1999, pp. 169-170) classed only a couple of theories as goal-oriented: Jung's (1960) synchronicity theory, and Stanford's (1978) *conformance behavior* model (these have been discussed previously—see *4.2.3* and *4.2.4*, respectively).

Thalbourne (2000a) reports that the term goal-oriented can be used in two ways: (a) "psi is goal-oriented in the sense that it tends to bring about a 'goal' " (p. 55), and (b) "*psi effects constitute the fulfillment or satisfaction of an intention, goal, need, or 'disposition' of the organism*" (p. 55). The former refers to the actual outcome or "specific state of a target-system," while the latter refers only to a "mental representation," but not the achieved or manifested state. Thalbourne uses goal-orientation in the latter sense.

In the theory of psychopraxia, therefore, the goal *per se* is something akin to an image of a goal or need, etc., that is yet to be achieved, but is not the manifestation of that goal or need. Given the examples presented by Stanford (1974a, pp. 35-38) of psi goals achieved, it becomes apparent that many goals *per se* cannot be visualized in explicit detail. In some cases, the time and place of their occurrence cannot be imagined. Hence, in conceptualizing psi, Thalbourne shows a preference for "pro attitude serving" and marginalizes the term "goal-oriented" due to its occasionally misleading connotation (p. 66). In many cases, we can regard manifested psi as an achievement that is beyond the ken and the acumen of the individual in terms of visualization in all its detail, planning and execution of the goal, yet nonetheless, psi manifests in, literally, the most *unimaginable* ways.

Though the term goal-orientation has been used occasionally throughout this thesis, its poverty in characterizing psi as something that can be planned in consciousness and executed in accordance with that plan becomes apparent. If goals can be set, they may exist in consciousness only as a sketchy idea (e.g., as a pro attitude), but it belongs to the machinations of a higher, undoubtedly unconscious, process for its ultimate conceptualization and subsequent execution. The hypothesis that this unconscious process describes the workings of the *potentially* empirical self is made with good reason once the

limits of the conscious ego are realized (see *12.2.1* above). It is perhaps unsurprising too that Stanford (1990, pp. 58–59) ultimately became critical of goal orientation in his revived PMIR model.

Thus for Thalbourne, even though he tentatively adopts and then drops the term goal-oriented, it is wise to use the term 'goal' with due care. There is a sense that the typical psi target in most experiments can be visualized (notwithstanding the complexity that may entail in its representation), and is therefore a goal clear and proper, but in other cases, the goal, like the self concept, is irrepresentable in consciousness, or nearly so, relying entirely on symbolic representation. Goals as mental representations, therefore, may often be mere symbols only of more complex, inexpressible, realities that psi is nevertheless capable of achieving. Thus, terminologically, if need be, the word goal can and should be referred to in a figurative, metaphorical, and/or implicit sense, as opposed to a (not altogether inaccurate) literal, analogical, and/or explicit sense.

12.2.5 The Ontological Question Revisited

The three major philosophical systems—Dualism, Materialism, and Idealism—were introduced in Chapter 4. Each system defines the world in a specific context, specifically in terms of the way each system defines its subject (see Table 4.3). Any given phenomenon can be described in at least three different ways according to these three ontologies. Sometimes there may be a shared use of terminology, but even then, there will be a dispute over the definitions and meanings of these terms (e.g., mind is a fundamental principle in Dualism and Idealism, and in both cases, is defined as 'real', but mind may take on epiphenomenal or interactionist dimensions in some versions of Dualism).

To go further, terminological and definitional problems that exist in one ontology may not even exist in another. For example, the Materialist sees the substantive nature of matter as a very real problem when applied to the physical world, whereas the Idealist is not necessarily impelled to regard matter as having existence in any materialistic sense at all, believing that matter is an experience of the senses possessed of conceptual dimension only. Thus, to a large degree, each system relies on its own terminology. The various philosophical viewpoints, therefore, shape the worldview of a culture, a society, or even a scientific community.

The full effect of just how much influence a philosophical system can have on scientific understanding and the progress of science itself is given by the fact that the ontological mindset of a scientific community shapes the way in which it will perceive its

subject matter. So, the philosophy shapes the mindset, and the mindset, in turn, shapes the way a phenomenon will be perceived. Thus, phenomena that are to be classed as being worthy of investigation, because they are deemed valid by nature of their specifiability or repeatability, etc., (see *1.3.1*), as against those phenomena that are to be marginalized because they defy these criteria, are classed as one or the other by the ways in which the scientific community view those phenomena.

Often, however, this kind of polarization may be the result of both proper and improper uses of the philosophical terminology, but that problem is itself just another factor illustrating the socio-empirical reality underlying so-called objective science (for a general treatment of this problem, see **1.3**). And, what holds true for any given phenomenon also holds true for any given theory, old or new. A theory is a cultural (scientific) phenomenon after all, subject to the same critical treatments as any other phenomenon. The very survival of a theory depends on how well it adapts to the scientific milieu.

One of the initial, but major, stipulations of the theory of psychopraxia was that it be ontologically 'neutral'—it should be accessible to any given philosophical paradigm. This stipulation gives Thalbourne's theory a 'fighting chance' in the philosophical and scientific stakes in the sense that it does not exclusively endorse the precepts of any given ontological system. Critics may regard the apparent neutrality of his theory as a strategically constructed political maneuver—a built in adaptive advantage—that might assure its survival in a science that has constantly seen theories come and go. Others might point out the fact that this benefit is merely a default, or better, an imperative theoretical adjustment resulting from the more important criterion of highlighting the flaws in traditional scientific thinking. Certainly, the latter view is the one espoused by Thalbourne (2000a, pp. 15-25). It is a view that deserves due consideration because Thalbourne's theory necessarily faults a reigning scientific hegemony designed to legitimize or exclude psi on the basis of purely arbitrary scientific principles that have their history in, likely as not, outmoded philosophical prejudices that hark back to ancient Greece.

Whether or not the critic can see either one or both of these agendas, hidden or concealed in the fundamentals of Thalbourne's theory, will depend on the nature of the critic, as much as it may depend on any psychopractic text. The position in fact tells more about the critic than it does the theory of psychopraxia. Ultimately, however, such speculation belongs to philosophers and critics of science who usually have a penchant for, or better, a training in, sociological thinking. Nevertheless, no matter how we wish to consider the paranormal, we must recognize that psychopractic effects, like psi, exist

discursively—we rely on language to represent these effects in ways that can be generally understood. The ontological problem leads us directly to some brief examples of how psi is treated in terms of perceiving it from anything but an ontologically neutral position.

12.2.6 Ways of Perceiving Psi: Insurmountable Dualism?

The psychopractic terminology introduced in Chapter 4, and appraised in the forgoing subsections, has raised some problems that are manifestly real in terms of the way in which theories are derived, which in turn, dictate the way in which experiments are designed and operationalized. Related to these issues, the previous subsection (see *12.2.5*) dealt with the influence of the various ontologies on theory and experimentation. In this subsection, Dualism is given a special focus due to its sovereignty in the sciences (see also, Chapter 4, as well as *5.8.1.2*, *10.1.1*, and *10.1.2*).

Edge (1985b) noted that current thinking in parapsychology is essentially Dualistic. Some forms of Dualism, such as Substance Dualism and Interactionism, posit a nonphysical property of mind, which thus allows for the nonspatial/nontemporal effects of ESP and PK. However, the other two ontologies, Idealism and Materialism, also feature in the debate over terminology from time to time, but it is argued here that the tendency is to defer to the more favoured Dualistic model. The following two examples illustrate this Dualistic bias.

King (1996) has argued that Kant's Critical Idealism would allow for some types of paranormal phenomena because feelings, sensations, images, etc., "may bypass the iterative synthesis of the understanding" (p. 246) deemed a necessary mental process by Kant, since all experience can only become informational after the a priori function of categorization has taken place. But, there is no need for such processing of ESP-type 'information' if Kant's "Anschauung" ("sensibility") only is operative: "We could establish a superluminal telegraph by using psi to alter the random behavior of photons" (p. 246). Thus, King (1996, pp. 246-249) looks for support for his model in the discipline of physics, which would ordinarily describe all paranormal effects as forms of PK in accordance with Thalbourne's system of categorization (see Table 4.3).

King discusses PK in one context (a quote from von Lucadou, 1987, p. 247; cited in King, 1996, p. 247) and ESP in another (i.e., the ganzfeld paradigm; p. 247). In fact, it is difficult to see where King is going with his theory—that is, whether he believes PK is a form of ESP, as an Idealist would, or vice versa, as a materialist would believe, or whether he believes there are still two distinct phenomena, as a Dualist would believe. The latter

would appear to be the logical choice. The blend of Idealistic (Kantian) thought with materialistic science ultimately requires a resolution in Dualism.

In this second example, Materialism best allows for the possibility of paranormal phenomena by way of the 'hard' science of Quantum Mechanics, where anomalous non-local effects have been observed hundreds of times in the laboratory (Bell, 1964, pp. 195-200; Noble, 1988, pp. 179, 181). 'Materialistic' ('physical') theories of psi based on quantum developments have already been posited (cf. Jahn & Dunne, 1986, 1987; Josephson & Pallikari-Viras, 1991). The debate, however, between physicists is far from over.

As an example, noted physicist Evan Harris Walker (E. H. Walker, personal communication, November 15, 2001) lists many problems that Physics still faces, including finding (ironically) a proof "that there is a dualistic mind/matter reality—physically and fully described—and make it consistent with most all of modern philosophy that says there can be no duality." From Walker's statement it can be seen how dependent upon Dualism physicists may be in regard to viewing psi. It remains to be seen whether exclusively Idealistic and/or exclusively Materialistic treatments of psi will be on offer in the future, or whether Dualism will remain an insurmountable philosophical force in the sciences.

12.3 Theoretical Considerations of the Theory of Psychopraxia

In order to assess the possible contribution the theory of psychopraxia can make to parapsychology it is helpful to crystallize the main thrust of the theory into fundamental statements that can be compared with current theories on offer in parapsychology. Only theories that bear some interesting theoretical similarities with psychopraxia theory need be considered in such a comparison. These are Jung's (1960) synchronicity theory, and Stanford's (1974a, 1974b, 1990) PMIR model.

12.3.1 Psychopraxia and Synchronicity

Jung's theory of synchronicity has been described previously (see *4.2.3*). Irwin (1999, p. 169) notes, how in Jung's mind, synchronicity dispenses with causes. Psi events (since Jung classed ESP and PK as special cases of synchronicity—Jung, 1960, para. 840, 863, 977-979) happen as chance events, and their occurrence is characterized only by their meaningfulness. Storm (1999) argued that archetypes (see *4.2.3* for a definition of archetype) are contingent with paranormal effects that are not mechanically causal in their

own right, but nevertheless work in a "meta-causal" way (p. 259). He further argued that psi-conducive and psi-inhibitive conditions were also meta-causal in that they relate to the facilitation of psi, but are not causes in themselves:

> If . . . a cause is only a cause when it 'contains' the conditions necessary [i.e., sufficient] for an effect to take place, then psi-permissive and psi-conducive conditions are meta-causal, being the necessary conditions of an actual cause or causes still unknown. It is these and possibly other conditions that give causes their causal properties. (Storm, 1999, p. 260)

This argument fulfils the requirements of conventional physics, which only requires that the philosophical 'if A, then B' axiom be sufficiently demonstrated. Conditions, therefore, are scientifically causal (i.e., meta-causal) but not mechanistically causal.

Contingence of archetypes in synchronicity parallels the 'sufficient' condition, or meta-cause necessary, for the psychopractic pro attitude to be achieved. (Note that archetype and psychopractic condition are not always equivalent.) Therefore, it appears that the fundamental psychopractic principle of the necessary condition is not dissimilar to the synchronistic principle of contingency. This parallel between crucial factors of psychopraxia theory and synchronicity theory may be of considerable interest to parapsychology since Storm (1999) has argued that experimental synchronicity is possible and, in fact, has already taken place (*pace* Beloff, 1990, pp. 23, 171; and Irwin, 1999, p. 169).

By way of the above feasible reworking of the terminologies used in both theories, it is suggested that they have in common the meta-cause. These two theories thus share a common ground—in synchronicity, there is the meta-causal archetype; in psychopraxia there are the meta-causal conditions. To the degree that the parapsychologist identifies a meta-cause to be both an archetype and a condition which brings about a goal (be it a psychopractic or synchronistic goal), synchronicity and psychopraxia are more than compatible—both theories are essentially speaking to the same phenomenon: the meta-casual factor underlying the psi process.

There is another similarity between synchronicity and psychopraxia, and this is that both Jung and Thalbourne regard the paranormal process as goal-oriented. (Note, however, that Thalbourne prefers 'pro attitude serving' to 'goal-oriented'—see *12.2.4* above.) Jung (1960) gave a clear indication that the goal of synchronicity was primarily a transformation of personality. Storm (1999) clarifies this point:

> . . . the likelihood of healing or change in outlook may result [when synchronicity occurs]. . . . Ultimately the result is a transformation of personality in some way, which Jung saw as the embodiment of the individuation process, "having for its goal the development of the individual personality" (Jung, 1987, para. 757). (Storm, 1999, p. 251)

Few would argue that paranormal experiences do not affect personality, and this would include experiences of exo-psychopraxia. The question is: "What is the goal being served?" Thalbourne's definition of a goal refers to a mental representation of the goal rather than the manifested goal itself. Jung would be in agreement with Thalbourne because the transformation of personality was always seen as an ongoing process of 'individuation' (becoming whole or integrated) rather than something achieved in a span of years or a lifetime. Therefore, for Jung, the goal would be a mental representation of what could be achieved, without any guarantee that the person would actually reach that goal.

However, since Jung was more interested in the development of the personality, the interesting psi phenomena that occur along the way may be secondary to the enterprise of individuation (Jung gives examples of ostensible psi phenomena that merely serve as meaningful interpretations of how the inner life is enmeshed with the outer world—for examples, see Storm, 1999, pp. 251-253). The difference here is that the parapsychologist, in the role of 'proponent of synchronicity', would regard psi as the goal (a paranormal outcome), whereas the psychologist/psychiatrist and/or analyst, also as 'proponents of synchronicity', would regard individuation as the goal (a normal outcome).

This difference reflects more than just personal prejudice about the nature of goals, but also the very concept of how a goal should be defined and interpreted. This conundrum may be just as much a problem for the theory of psychopraxia as it is for the theory of synchronicity, or for personality theory. In any case, whatever the perspective, we can talk of goal achievement if we do not get too specific! This problem does not mean synchronicity and psychopraxia are necessarily incompatible on this point, but whereas experimental synchronicity will look for meaningfulness in the synchronistic event, psychopraxia looks for a pro attitude that is focused towards bringing about a goal, whatever that may really be. (Note too that Stanford, 1990, p. 58, had a problem with the vagaries of the goal *per se*: "What really represents a goal"?—see *12.3.2* below.)

To summarize, neither theory can really replace the other. Synchronicity requires that meaningfulness be identified, whereas psychopraxia is not focused on identifying a meaningful relationship between an internal image and a goal. But, to the extent that a manifested psychopractic goal is identified as being meaningful because an internal image

has been categorically identified (as, for example, a pro attitude that is either conscious or, by the evidence, unconscious), psychopraxia and synchronicity become one. In such cases, then, as Jung believed, ESP and PK (and, therefore, exo-psychopraxia) share the same phenomenology as synchronicity. Thus, on occasion, the traditional (but open-minded!) parapsychologist may be given the option of calling his/her psi effect an example of ESP, PK, exo-psychopraxia, or synchronicity depending on the circumstances!

12.3.2 Psychopraxia and the Psi Mediated Instrumental Response Model

Stanford's PMIR model has been described previously (see *4.2.4*). Stanford (1978) describes situations that are similar to synchronicity, in which "intriguing spontaneous occurrences, while they provide no compelling evidence that they are anything other than 'coincidences', might be instances of psi-mediated responses working in the service of the individual's needs" (p. 201). In fact, Stanford (1977a) very clearly shows that PMIR experiences are meaningful: "Odd coincidences of everyday life . . . seem to have meaning and importance for persons involved in them" (pp. 840-841). Stanford regards such "fortuitous coincidences" as "psi mediated" (1977a, p. 841); hence the term psi mediated instrumental response (PMIR). Thus far, the parallel of PMIR with Jung's synchronicity theory is clear (in fact, Stanford, 1977a, p. 851, admits this parallel), and it will be seen that there are also occasional parallels between PMIR and exo-psychopraxia. However, there are some differences between PMIR and synchronicity, and PMIR and exo-psychopraxia.

Stanford (1977a) initially proposed that "the organism uses psi (ESP), as well as sensory means, to scan its environment" (p. 841) in order maximally to fulfill its needs, but he (Stanford, 1990) later rejected the "scanning component" (pp. 57–58). Although Stanford (1978) never did offer a reason "*how*" the organism brings about a favourable change of circumstances, but only stated that it "*does* occur" (p. 208), there was at least some suggestion of a psi mechanism in the scanning component. Nevertheless, as it stands the PMIR model might be classified alongside Stanford's (1978) *conformance behavior* model (see *4.2.4*) as another example of a noncybernetic theory (Irwin, 1999, pp. 169-170.).

Following directly from the above, the PMIR model (Stanford, 1990, p. 59) describes psi as a goal-oriented process, yet Stanford is critical of the goal-orientation hypothesis (while, ironically, still showing some sympathy for it). He sees a need for "considerable high-quality evidence" before "goal-orientedness" can be endorsed with greater confidence (Stanford, 1990, p. 59), and thus he keeps the goal-oriented hypothesis

as a "working assumption." But Stanford also states that he would "develop mechanisms for the occurrence of PMIR that do not depend on this assumption" (p. 59). He actually goes further than that by clearly stating that "the psi factor, whatever that is, supplies information that can influence the organism" (p. 60). Thus, on the one hand, the deception in the PMIR model is that it *is* an information-processing model, whereas, on the other hand, Stanford has "considerable misgivings [about the goal-oriented assumption] because of its vagueness and magic-sounding qualities" (Stanford, 1990, p. 157). The attraction of goal-oriented psi, of course, lies in its elegance as a model that eliminates the problem of positing and describing a "filtering mechanism" that cuts off extraneous, redundant, and irrelevant noise, and lets in only the psi signal. But Stanford (1990, p. 59) cannot escape resorting to the evidence to the contrary (for example, see Vassy, 1986).

While Jung saw synchronicity as goal-oriented, and Stanford holds an 'each-way' bet, Thalbourne clearly allows for the possibility that information may facilitate psi. Thalbourne does not put himself in the same position as Stanford because he defers to the unique position of calling psi a "pro attitude serving" process as opposed to it being a "goal-oriented" process (see again *12.2.2* above). Thalbourne argues that some paranormal phenomena merely present in such a way as to suggest the goal-orientation hypothesis, leaving open the possibility that subtle information processes may well occur in psychopractic action, but may not be detected in any measurable way. It can only be concluded that the PMIR model leaves the researcher unclear as to whether cybernetic or noncybernetic processes underscore the psi process, whereas the psychopraxia model suggests a cybernetic process.

Continuing from previous statements above, even if the self does not 'scan', Stanford does not reject unconscious agency (cf. Stanford, 1977a, pp. 844-845; see also Stanford's, 1974a, references to intentional and nonintentional, i.e., unconscious, psi), which therefore does not conflict with Thalbourne (2000a, p. 65), who is clear that the pro attitude can be held consciously or unconsciously. For Jung (1960, para. 912) too, unconscious agency is inherently self-evident in synchronicity, where archetypes are concerned.

There is a sense in which there is little point in arguing over whether there is any disagreement amongst the three theorists on conscious and unconscious agency, but it cannot be settled here whether the self (a) can scan unconsciously (using normal senses and psi), or (b) does not need to scan to 'perceive' its proximal and distal environment because it is relatively omniscient (i.e., the '*Cognitive Unconscious* never ceases to function'. Cf. Parker, 2001). However, there is a terminological problem that must be

resolved. Stanford's relative concept of 'nonintentional' psi is only correct insofar as the organism is unconscious of the psi process that is about to occur. Certainly, for the purposes of experimentation, it is necessary to qualify which aspect of the agent—the conscious ego, or the '*potentially*' empirical self that uses the 'cognitive unconscious'—is being tested. But there are situations in which the term becomes problematic.

There would be cases where the self, in its unconscious aspect, is quite clearly intentional of a psi effect. Thalbourne's idea of the pro attitude allows this possibility because pro attitudes can be unconscious. In fact, there is no psi event that could be regarded as nonintentional because, in terms of the organism's preference for an outcome, the successful pro attitude is *always* the pro attitude most preferred by the self-system, which is not limited to the conscious ego. An example illustrating this point is given in the normal case of rolling over in one's sleep. The behaviour is pro attitude serving because it serves to minimize cramp and discomfort during sleep. Even though it is unconscious behaviour, and therefore, not intentional, it is at another level, never *nonintentional*. It is fully intended by the organism, or more precisely, by a self-system that is capable of perceiving and monitoring its environment to a degree far superior to that of the sleeping ego.

Thus, it appears the concept of intention is related to the concept of the pro attitude. Insofar as PMIR is "disposition-serving" (Stanford, 1978, p. 197) and psychopraxia is "pro attitude serving" (Thalbourne, 2000a, p. 66), Stanford and Thalbourne come close to agreeing with each other. Stanford originally (1977a) referred to "need-strength" or "need-importance" of achieving a "goal-object" or "goal-event," and proposed a relationship between these needs and the "disposition" of the organism. He then generalized the term 'need' to "response disposition" (p. 851), although he did not drop the term 'need' altogether (see Stanford, 1990, p. 91). Stanford introduced the term 'disposition' on the grounds that some manifestations of psi do not necessarily occur just to fulfill needs *per se*, but the organism may nonetheless be disposed to receiving psi information because it has relevance to that organism (1990, p. 60). Thalbourne (2000a) agrees, and gives an example where a coincidence was a "result of psi," but was apparently *not* the result of a present volition or motive or *need* [emphasis added] on [his] part, yet it *was* something towards which [he] can be said to have had a pro attitude" (p. 66).

Finally, a consideration of the psychopractic necessary (and conducive) condition from the perspective of Stanford's PMIR model is needed to bring these two theories into perspective. The concept of necessary conditions has already been discussed (see *12.2.3*). Stanford not only mentioned inhibitive conditions (see *4.4.4*; Stanford, 1977a, pp. 848-850,

1990, pp. 138-153), but he also allowed for the fact that certain conditions bring about, or help being about PMIR (Stanford, 1977a, pp. 842). For example, he refers to "emotional arousal," "attention-focusing," and other psychophysiological motivations that may be, or are, triggered to make "*preparation for or production of* PMIR" (p. 842; emphasis added), and he cited evidence for these effects (Stanford, 1977a, p. 842). Given that these conditions are deemed preparatory or productive, they must be necessary or conducive since it is implied that their absence halts or hinders PMIR. Thalbourne (2000a) expressly supports the idea that necessary and conducive conditions may be "psychophysiological" (p. 76; this issue has been dealt with above—see *12.2.3*). Thus, there is agreement between Stanford and Thalbourne on the issue of conditions.

* * *

In summary, the three theories discussed above tend to vary in subtle and not so subtle ways. Similarities have been drawn between exo-psychopraxia and synchronicity, and between exo-psychopraxia and PMIR. Stanford, while uncertain, tentatively prefers that PMIR be regarded as a goal-oriented, noncybernetic theory (as is synchronicity), but Thalbourne regards exo-psychopraxia as cybernetic. However, there seems to be contradictions in Stanford's statements, leading us to wonder if he prefers that PMIR be described as cybernetic or noncybernetic.

All three theories acknowledge unconscious processes, but the PMIR concept of nonintentionality might, on occasion, be misleading. Disposition-serving (PMIR) seems similar to pro-attitude-serving (the theory of psychopraxia). The disposition and the pro attitude are both seen as necessary, and Thalbourne and Stanford agree that there are psychophysiological conditions that can be regarded as conducive, necessary, or even sufficient. Synchronicity theory speaks of archetypal patterns underlying, but not causing, the synchronistic event. There may be a sense in which all manner of conditions are meta-causal, and may even be underscored by archetypal structures that extend our concept of the self beyond the limits and definition of a biological organism.

Thus, *again*, the traditional (but open-minded!) parapsychologist may be given the option of calling his/her psi effect an example of synchronicity, exo-psychopraxia, or PMIR, provided certain criteria are met. There must be a meaningful component in the psi effect for it to be called synchronicity. If one cannot be found, one resorts to either exo-psychopraxia or PMIR for an explanation. If the effect seems to be noncybernetic, the indecisive researcher might like to regard it as evidence of PMIR. But that would be saying

nothing in itself because the conclusion is merely cashing in on Stanford's 'each-way' bet—if, rather, the psi effect seems to be cybernetic because "the psi factor . . . supplie[d] information that can influence the organism" (Stanford, 1990, p. 60), the indecisive researcher could still regard it as evidence of PMIR. A theory must propose that psi is a cybernetic process (or not) in advance so that the experimenter can test this hypothesis. Otherwise, the theory tells nothing of the psi process—it stands to reason, *if there are only two choices*, that the effect could be goal-oriented or mediational, cybernetic or noncybernetic, without even considering PMIR. Alternatively, if the psi effect is cybernetic, it would support the theory of psychopraxia, but if it is not, at least there would be no room for doubt. *Theoretically*, in terms of the fundamental propositions of both psychopraxia and PMIR, there are no other major differences.

12.4 Experimental Considerations of the Theory of Psychopraxia

A brief report of the four experiments reported in the present thesis was given in the previous chapter (see *11.5.2*). In the following section, the ESP-PK dichotomy, and the two concepts of pro attitude and 'necessary and sufficient' conditions, will be reviewed in the light of the experimental findings reported in those chapters.

12.4.1 The ESP-PK Dichotomy

It was concluded in Chapter 5 (see *5.8.1.2*) that the significant result relevant to the paranormal task of throwing coins to generate a hexagram hitting in the replication study of the *I Ching* experiment, challenged the traditional view that ESP or PK may have taken place in that experiment, or indeed, that both ESP and PK may have taken place simultaneously. In the interests of scientific parsimony, we need to be convinced that this 'trichotomous' situation is acceptable for parapsychology. According to psychopraxia theory, this ambiguous state of affairs is in fact unacceptable, and is created by the dualistic construction of seeing mental and physical events as belonging to two mutually exclusive categories (for examples, see *12.2.6*).

If philosophical persuasion governs scientific convention and consequently the consensus of the parapsychological community, process-oriented interpretations of psi phenomena will revolve around discursive constructs that may not get to the core of the psi process itself. Thalbourne (2000a), however, proffers psychopraxia theory, which moves the theorist beyond this debate. The ESP-PK conundrum is resolvable in terms of psychopraxia theory because it avoids dealing with the psi process as a dichotomy.

Thalbourne argues that it may be more productive for parapsychology that psi mechanisms not be proposed if such mechanisms cannot lead to an equitable solution.

12.4.2 The Pro Attitude

Assuming that the necessary and sufficient conditions must have been in place, it could be argued that pro attitudes were demonstrated in all four experiments, in the form of significant results found from hypothesis testing and/or replications of effect sizes (see *5.8.1.1*, *7.4.4.2*, *8.7.4.1*, and *9.4.2*). Insofar as it is possible to actually measure pro attitudes in participants, the various belief questions/scales, and/or attitude scales, and even the Transliminality Scale, may not be perfect, but will suffice at this early stage (see *5.7.5*, *6.3.4*, *6.3.5*, *7.7.2*, *7.7.4.3*, *8.4.3,* and *9.4.2*). A scale which measures pure pro attitude was not used in this series of experiments because of potential problems with demand characteristics. Future research may be able to resolve this problem.

In Chapter 5, the issue of whose pro attitude was being served was raised. If participants are capable of holding pro attitudes, then so is the principal experimenter, as are other interested parties, such as supervisor(s) or other parapsychologists. It was proposed that Jung's (1960) theory of synchronicity suggests that the dominating pro attitudes are those of the participants because the physical outcomes are meaningfully related to the mental (cognitive/emotional) states of those participants. In Chapter 5, the nature of the pro attitude was also tested (see *5.7.5*) in order to determine if 'pro attitude serving' is mediational (i.e., involves information processing), but no evidence was found to that end. It could not, however, be concluded that the effect was nonmediational (i.e., was goal-oriented).

In Chapter 7, a negative relationship between Ace of Spade hitting and Ace of Club hitting was found (see *7.7.2*). Attempts to achieve successful spade-hitting and club-hitting in these two tasks were described as being initiated by compliant and noncompliant pro attitudes, respectively. Both forms of hitting produced effect sizes that were comparable to the effect size norm of the forced-choice domain (see *7.7.4.2*). It was concluded that the relationship between the two types of psi effects was compensatory, rather than being constant for a given outcome when the other had some degree of variance (if compliance is present, noncompliance tends not to be present as hypothesized earlier; see **7.2**). Therefore, the relationship between compliant and noncompliant pro attitudes is one of compensation (more on this in **12.6**).

In Chapter 8, attempts were made to see if the primary level pro attitudes of skeptics could be changed. This attempt failed, but there was evidence that the pro attitude

could be maintained over a period of time (see *8.4.3*). Successful scoring for so-called converted skeptics increased from the 1st to the 2nd run (see *8.4.4.1*), but this increase was not significant. Nevertheless, the effect may have been the result of a 'conversion effect', and is thus suggestive evidence that pro attitudes can change. A positive conversion effect (in the case of a participant changing from skeptic to believer) is here defined as a burst of newfound enthusiasm and increased motivation that may be temporary or sustained, and it is hypothesized that the effect results in increased psi performance. The conversion effect, at least in this particular form, has not been reported in the literature since attempts at altering the performances of skeptics in the way outlined in Chapter 8 (see *8.3.2* and *8.3.4*) are original to Thalbourne (2000a, p. 60-63). The conversion effect may take reverse form (believer to skeptic) as (say) the result of being convinced that psi was not taking place in a paranormal process. The negative conversion effect might result in discouragement and decreased motivation, leading to deterioration in psi performance. The conversion effect is a form of pro attitudinal change.

12.4.3 Necessary Conditions

In the present thesis, hypothesized necessary conditions were tested using various questionnaires and scales (i.e., the Transliminality Scale, the 16PF, the GAS, the BIGL, and the SL-TDI). Characteristics of personality, etc., were hypothesized as being necessary and sufficient in bringing about a paranormal effect in a given experiment. (Note that this line of reasoning is questioned in **12.6** below.) The 23% success rate shown in Table 11.9 is a reasonable feat in parapsychology (notwithstanding the criticism of this success rate in *11.5.2*).

In Chapter 6, transliminality was confirmed as a possible necessary condition (high scorers tended to psi hit), but the reversal effect at one stage of experimentation (see *6.5.1*) undermined this positive relationship. It may be possible that some kind of interaction effect took place during the experiment that caused a negative relationship, which nevertheless returned to the positive relationship reported in the initial *I Ching* study (Storm & Thalbourne, 1998-1999).

The six hitting/16PF correlations were found in Storm and Thalbourne (1998-1999), then lost (see *5.7.5*), and then found again, less one (see *6.3.6*). There is a strong call for replication studies before any conclusions can be made about these 16PF predictors and the possibility of their being necessary conditions, although they were later found to be necessary and sufficient (see *6.5.2* and *6.5.3*). Replication is particularly needed to decide

the fate of the five Changing Lines/16PF correlations, which were found in Storm and Thalbourne (1998-1999), then lost (see *5.7.5*), never to return (see *6.3.7*).

In Chapter 7, it appeared that a moderate attitude towards gambling allowed participants to use their intuitive functions more efficaciously. On that basis, high scorers on IN and EN were the most likely to score high on aces of spades, and ironically, aces of clubs. No attempt was made to isolate the factor that determined the preferred target, but it seems likely to be related to how and why certain pro attitudes are formed, and thus relates directly to the compliance/noncompliance issue.

In Chapter 8, a positive and moderate relationship was found between scores on the Australian Sheep-Goat Scale and symbol hitting for converted skeptics. Belief measures tend to be good predictors to psi performance, as this correlation suggests. So far, belief in psi has been shown to be conducive to psi effects (see Lawrence, 1993), and therefore is not necessary in any absolute way in bringing about paranormal effects.

Finally, in Chapter 9, the conditions of relaxation and belief were tested as being necessary for psi effects. On this occasion, only one out of six tests revealed relaxation to be possibly necessary for, but more likely to be conducive to psi. The debate between conducive and necessary conditions reflects on these findings (see *12.2.3* above).

12.5 Psychopraxia and its Contribution to Parapsychology

Having presented and deliberated on the considerations pertinent to the theory of psychopraxia (see **12.2**, **12.3**, and **12.4**), this section seeks informed answers to the following questions in order to assess the theory's contribution to the field of parapsychology:

1. Does the theory of psychopraxia avoid saying essentially the same thing as synchronicity theory and/or the PMIR model?
2. Does the theory of psychopraxia simplify our understanding of psi compared to synchronicity theory and/or the PMIR model without losing any qualitative detail?
3. Does the theory of psychopraxia introduce terminology that more accurately describes psi than does synchronicity theory and/or the PMIR model?
4. Does the theory of psychopraxia offer methods of testing and interpreting psi that synchronicity theory and/or the PMIR model do not?

Answers in the affirmative to one or more of these questions may be sufficient in themselves to draw a conclusion as to the merits of the theory of psychopraxia.

Question 1: Does the theory of psychopraxia avoid saying essentially the same thing as synchronicity theory and/or the PMIR model? (Answer: 'Uncertain'.) This question seeks an answer to the possibility that the theory of psychopraxia is "operationally equivalent" to other paranormal theories (Stokes, 1987, p. 78). It must not be forgotten, of course, that Thalbourne's theory explains normal phenomena as well (see *4.4.1*). If a theory is found that is similar to Thalbourne's theory, it would necessarily have to speak to normal phenomena. Thus, synchronicity and PMIR are excluded. These facts notwithstanding, there is nothing stopping parapsychologists from preferring a workable theory that explains psi only, and many may not see the relevance of a paranormal theory that speaks to normal phenomena as well, regardless of the fact that it unifies the two types of human action. Be that as it may, the theoretical component of exo-psychopraxia (ostensibly the component of the theory of psychopraxia that speaks to paranormal phenomena) can still be compared to synchronicity theory and the PMIR model.

It was already stated that exo-psychopraxia and synchronicity describe psi in similar ways (see *12.3.1*), but synchronicity stands alone in proposing that meaningfulness must be prevalent alongside the psi phenomenon. Psychopraxia, on the other hand, proposes a more sophisticated mechanism than synchronicity, which seems to be a phenomenon that just happens. However, given the strong similarities between Thalbourne's and Stanford's theories, parapsychologists might have difficulty expressing a preference for either, and they might argue that the two theories are operationally equivalent to a large degree. Stokes (1987), however, points out that "to regard such theories as equivalent may be a bit chauvinistic on our part" (p. 78)—our "limited senses and investigative techniques" may not be able to draw a distinction at this stage, but it may eventuate that "some privileged observer may be able to distinguish between them" (p. 78) at some other time.

Question 2: Does the theory of psychopraxia simplify our understanding of psi compared to synchronicity theory and/or the PMIR model without losing any qualitative detail? (Answer: A tentative 'Yes'.) Stokes (1987) refers to the fact that if two theories are similar, at least one of them should seek "not to multiply entities beyond necessity" (p. 79). The theory that achieves that end may be the theory that draws more attention. In the case of PMIR, Stanford (1990, pp. 62-153) spends an inordinate amount of time on qualifying aspects of his theory. There may be no dispute amongst theorists that these measures are

necessary, but one's deeper feeling cannot help suggest that such an exercise leaves Stanford more vulnerable than other theorists who have less to say about psi, simply because Stanford may have multiplied his entities beyond necessity. Stanford may be right about all he says, and his contribution would therefore be worthy by embracing all aspects that surround psi phenomena, but it also means that Stanford has not allowed for the possibility that psi may prove to be less complicated than he thinks.

Jung's synchronicity theory, on the other hand, may be a simpler concept in the sense that it requires an *understanding* of only one single concept—that of meaningful coincidence—regardless of whether it can be *accepted* by theoreticians as really saying something about how psi might work or what psi really is (for contrasting arguments on this aspect of the theory, see Braude, 1979, and Storm, 1999). The theory of psychopraxia, with its four basic concepts (i.e., the self, the pro attitude, necessary conditions, and the goal-state), may also be simpler to understand than the PMIR theory, with the many diverse contingencies surrounding the occurrence of PMIR, as expressed by a panoply of possible scenarios in which psi may be mediated (in particular, again see Stanford, 1990, pp. 62-153).

However, there is no guarantee that Thalbourne's concepts are easier than Stanford's in essence. The ontological question, for example, is an enormous challenge to conventional investigators who cannot get out of the dualistic mindset (for examples, see *12.2.6* above). Also, as far as both theories are concerned, the nature of the goal needs further work. Parapsychologists have also to decide whether to investigate and/or support the simpler 'pro attitude serving' model of psychopraxia, or wait until the goal-oriented/information-based dichotomy of PMIR is resolved. At the moment, Stanford's 'each-way bet' on the issue of goal orientation (see *12.3.2*) works against, not for, the PMIR model.

Question 3: Does the theory of psychopraxia introduce terminology that more accurately describes psi than does synchronicity theory and/or the PMIR model? (Answer: A tentative 'Yes'.) Synchronicity theory has one major term in its description of psi, and that is the concept of 'meaningfulness'. Apart from the usual psychological terminology used to present his theory, Jung has kept things comparatively simple although he does borrow Rhine's terms (i.e., ESP and PK), if only to subsume them under the rubric of synchronicity. Like Jung, Stanford also borrows Rhine's terms, ESP and PK. The mistake in using these terms lies in the way Stanford (1974b, pp. 326–328) criticized them, but failed to address the ESP-PK dichotomy successfully (see *4.2.4*), so that the PMIR model says nothing new

about ESP and PK in this regard. Thalbourne only refers to Rhine's terms to illustrate the point that the ESP-PK dichotomy can be unworkable.

Thalbourne presents two major terms (i.e., 'pro attitude' and 'necessary conditions'; his use of established terms, such as 'self' and 'goal-oriented' are not original). Thalbourne's two original terms seem logically to describe psi as a process inherently dependent upon the agent, but the difficulties associated with the concept of agency, and whether conditions are necessary, or conducive, or sufficient, have been discussed (see *12.2.2* and *12.2.3*).

While synchronistic meaningfulness accurately describes psi (as agreed by Stanford, 1977a, pp. 840-841), and meaningfulness may well depict a crucial aspect of psi, it is too much to claim that Jung's description of psi is more accurate than Stanford's description. Stanford relies on the terms 'psi-mediated instrumental response' and 'goal-orientation'. Stanford's original term, 'psi-mediated instrumental response', seems tautological in that its definition describes what psi brings about through the processes of psi, without explaining how, as Stanford admits (see the next question). The other term 'goal orientation' is still being tested experimentally, so we are no wiser as to the hypothesized goal-oriented nature of psi just yet.

Question 4: Does the theory of psychopraxia offer methods of testing and interpreting psi that synchronicity theory and/or the PMIR model do not? (Answer: 'Yes'.) It has already been stated that experimental synchronicity is possible (Storm, 1999), and Stanford's (1990) theory has proven itself to be testable. The present thesis has shown that psychopraxia is testable. As far as interpretation is concerned, however, Stanford's explanation of how PMIR works is vague (see, in particular, Stanford, 1990, p. 102). A close reading of Stanford does not actually give any idea of a mechanism (see also his admission in *12.3.2*). He is really setting up the conditions conducive, and/or necessary, and/or sufficient, for psi (much as Thalbourne does), and it is almost as though (in some cases of PMIR) we are required to accept without question that we must invoke a trust in our intuition without consciously knowing why, and act on it, in order for psi to be manifested always and without fail (viz., his 'expressway' story—Stanford, 1990, pp. 102-103). In a *sense*, intuition (at least in the examples given by Stanford) comes over as an infallible function, but there must surely be cases where trust in the supposedly unquestionable reliability of intuition was a fatal mistake.

On this same point, Stanford's theory relies heavily on anecdotal material in its construction. Thalbourne (2000a, p. 66) does this on occasion, but Stanford (1974a, pp. 35-

38; 1978, p. 201) depends on it continually, yet such theory-building ultimately seems to come over as generalized or formalized statements about psi that are more descriptive than explanatory. Thalbourne's theory sometimes presents in the same way, but at least his propositions are often openly speculative (see especially, Thalbourne, 2000a, Chapter 5).

It is also important that a theory gets attention from investigators on the basis of its terms and definitions being clearly understood so that the operationalization of experiments is easily enabled. If a theory does not stimulate interest it may be because its terms are unclear. Synchronicity has received little attention over the decades, but operationalizing experiments that test meaningful coincidence have not proved to be difficult at all (cf. Braud, 1983). Stanford, though, has criticized experiments that have been conducted to test nonintentional psi (1990, pp. 96-102). It seems it is very difficult to really know what Stanford means, if experimenters keep getting it wrong, in spite of his great efforts to clarify every last detail. Of course a new theory cannot be judged by such standards—psychopraxia has not been around as long as PMIR, but the findings in the present thesis have not put psychopraxia in a bad light, and the relative simplicity of the theory compared to PMIR must work in its favour.

There are other questions that may be asked of a theory to ascertain its merits, but the above four would be amongst the most important. It is debatable whether the actual task of assessing theories this way is even a fair or ethical thing to do. However, in time, all theories are assessed one way or another, and no theory can escape this process. It is, of course, noted that there would be other ways of arguing the case for each of the three theories. Thus there is no guarantee that the answers would come out the same way if the questions were put to another critic. The truth of the matter is that parapsychologists should be free to test any theory they choose, and there should be no limits on which theory is or is not suitable for testing. Nevertheless, from the arguments put forward above, the suggestion is that the theory of psychopraxia deserves some kind of attention from parapsychologists, but the criticisms that were made about it should be given just as much attention. In the next section, an even closer focus is given to the theory of psychopraxia.

12.6 The Future for the Theory of Psychopraxia in Parapsychology

The future of the theory of psychopraxia will be determined by a number of factors. While it is true that consistently nonsignificant results may, after some initial curiosity about a given theory, result in certain dismissal (or at least marginalization) of that theory by a broad range of investigators inside and outside the discipline of parapsychology, it is

naïve to assume that significant and consistent results will ultimately lead to an immediate and general acceptance of the theory. If the latter scenario were the case for the theory of psychopraxia, there is still no guarantee that it will not meet the same reception as, for example, the ganzfeld paradigm (see Chapter 2). In the case of the ganzfeld, there was much opposition to results, which led to accusations of selective reporting, methodological flaws, etc.

The theory of psychopraxia has been tested in the present thesis, experimentally, theoretically, and philosophically. Experimentally, the theory has not met with general support for all of its basic propositions (see Table 11.8), including the claim that certain conditions were necessary for bringing about exo-psychopraxia *in a given experimental circumstance* (see Table 11.9), but by the same criteria, it has not failed completely. In fact, support came more often than not for the propositions (evidence supported four out of six propositions).

The nature of the pro attitude and its relationship with goals needs greater clarification. Given the long-term commitment that Stanford has made to parapsychological theorizing, and the high regard he has gained in the parapsychological community, his uncertainty about being able to specify a goal in clear terms (Stanford, 1990, p. 58) is a sobering thought, and a wake-up call to those who have neglected the problem of the nature of the goal and its possible multi-dimensional aspect (see *12.3.1*). For example, given the long-standing hypothesis of the unconscious (also referred to as the *Cognitive Unconscious* and identified with the *potentially* empirical self—see also *12.2.1*), it is certain that gaining knowledge about the functioning and the dynamics of the ego/self relationship will be a challenge for the investigator of all parapsychological theories, including the theory of psychopraxia. Work in this area might, for example, be of the nature of correlational studies in which conscious and unconscious determinants can be more readily ascertained (the transliminality concept may be useful here—see **5.3**).

Still on the topic of the pro attitude, it will be necessary to undertake neuro-biofeedback studies to gain insights into the brain processes that take place when the compliant pro attitude, (a) is held in consciousness, (b) slips into and out of consciousness (again, the transliminality concept may be useful), (c) is unconscious, and (d) conflicts with noncompliant pro attitudes. In fact, an answer to the question of what can and cannot be achieved by the pro attitude is crucial in understanding the limits of the human psyche when it goes about its everyday *praxia* (Thalbourne, 2000a, Chapter 5, borrowing from Beloff, 1973, p. 303, gives some idea of these limits by indicating the various levels that may be reached as psychopractic ability evolves or grows).

In regard to hypothesized necessary conditions, investigation into the nature of *necessity* is needed since there is some ambiguity over whether conditions should be regarded as necessary, sufficient, or conducive. Some attempt to reach a conclusion was made (see *12.2.3*), where it was stated that some necessary conditions can be sufficient, and *vice versa*, and it was shown in this thesis that this was the case (see again, Table 11.9).

While it may be relatively easy to show that some conditions can be psi-conducive (or psi-inhibitive), it may prove very difficult to show that the same conditions are *always* necessary or sufficient. For example, the Vision-Impaired experiment showed that relaxation was psi-conducive, but not necessary or sufficient, for a psi effect. More work needs to be done in identifying 'variables' (i.e., conditions) that are absent or not, rather than 'maximal', in the test participant to be better able to confirm whether these variables, as conditions, are really necessary rather than just psi-conducive. Thus, future investigation will require experimental designs that do not merely test psi-conducive conditions that have been well accepted as being such, but will test treatment groups in conditions that result in psi, as against control groups who do not receive the same treatments that result in chance effects only.

Until conditions are really shown to be necessary the proposition that there are certain conditions necessary in bringing about paranormal effects remains untested. The most that can be said is that a certain condition was necessary in delivering a psi effect some percent above the baseline psi effect found in the control group. Thus far, based on the literature reviews, personality tests, attitude scales, belief measures, and the like have been good in drawing out psi-conducive and psi-inhibitive variables, but the work on finding necessary conditions is in its infancy. The experiments reported in the present thesis are an attempt to rectify that situation, and in those experiments, median-split analyses were conducted as one means by which these hypothesized necessary conditions could be found.

Finally, although it was stated that there was not much encouragement for future researchers using the GAS, the SL-TDI and the BIGL (see *11.5.2*), the work with these scales is far from over. Specific subpopulations should also be targeted by investigators with the aim in mind of administering these scales. For example, the post hoc analysis in the Gambling experiment (see *8.7.4.2*) revealed that moderate scorers on the GAS were more likely to produce significant EN/hitting and IN/hitting relationships for both types of hitting (i.e., spade-hitting and club-hitting). Such results suggest that it is vital for the

future of parapsychology that more research using special (i.e., selected) groups is undertaken, and certainly the present thesis attempted this.

In conclusion, the theory of psychopraxia proposes some interesting and challenging concepts, which may be met with critiques, both good and bad. This is not unusual for any theory, and it will not necessarily stand as a lasting indictment for or against Thalbourne's theory that it is right or wrong always, simply because it is right or wrong today. The most valid criticisms will be those that are founded on the results of future research. On that basis, the present thesis may come to represent the first step only in the direction of continued investigation and inquiry into the theory of psychopraxia.