THE JUNGLE OF HYPNOTIC PSI: PART 2. RESEARCH ON RELATIONSHIPS BETWEEN PSI AND HYPNOSIS

Journal of Parapsychology, 79(1), 37–52

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ABSTRACT: Research on hypnosis and on psi has led to disputable correlational findings. This review indicates all the contemporary studies have major methodological weaknesses and failed to produce replicable findings and to predict future psi scores. A critique is made of the correlational approach as largely relating guessing behavior to weak psychological variables of doubtful validity. The net outcome is that research into hypnotic psi, rather than fulfilling the promise of a functional relationship based on solid ground, has become bogged down with correlational variables, methodological flaws, and what appear to be nonreplicable findings. An argument is made for a refocusing of research efforts on the most promising variables, namely "positive" dissociation" and "positive schizotypy." In the absence of full-scale sophisticated projects, the suggestion is that progress can be best made by the study of selected star participants or even so-called virtuoso performers. In this context the health versus pathology issue is a basic one that remains to be resolved. The issues raised are considered to be fundamental ones for a theory of consciousness.

Keywords: hypnosis, psi, schizotypal personality, trance, consciousness

In the first paper, it became clear that Rhine's assertion that hypnosis is a jungle of variables appears to be well-founded. The review of the 60-year dispute concerning whether or not the hypnotic state exists gave little prospect of penetrating the issues with these variables in the quest to find a psi-conducive state. It is proposed here that we now critically examine the actual attempts to make progress. This involves looking at the mainstream approaches within parapsychology and hypnosis research (or in jungle symbolism, trying to find a main stream through the jungle) and then reviewing the contemporary attempt to re-track approaches using clues left from the history of the hypnosis in the belief that some important leads have been missed.

The Stagnated "Main Stream"

A major mainstream approach in the scientific development of parapsychology involves the focus of process research on finding significant correlations of psychological tests with psi scores. In contrast with the correlational approach, the original attraction of hypnotic induction and of the emergent ganzfeld methodology was the promise of discovering functional relationships. The ideal is that you do something to the participant and the result will be seen clearly in the scores of the psi test. With the ganzfeld, this "something" was hypothesized at various times to enable a state of "noise reduction" or a radical shift in the state of consciousness (Parker, 1994, 2000). Another apparent functional relationship was the presence of target–related imagery that appeared to be synchronously or even causally linked to the occurrence of specific imagery in the film clip. Even unexpectedly changing the sender seemed to evoke an appropriate response in the receiver ín the form of an apparent surprise: "Where have you been?" (Parker, Persson, & Haller, 2000).

The supposed psi-conducive state could be measured simply by requiring participants to respond to taped instructions with numerical ratings during every 10 min of ganzfeld stimulation (Parker, Millar,

& Beloff, 1976) but this proved disturbing and seemed to result in significant psi-missing scores (Parker, 1975). While postsession questionnaires are now often employed, these obviously lack precision in identifying the state or change in state associated with the correct imagery. Future research might employ more sophisticated nonverbal methods such as button-pressing, which could be used to directly assess the state at the actual time of the potential psi experience.

Historically, the ganzfeld technique grew out of the need to specify and lift out the most functional aspects of hypnosis and dream research as regards psi-conduciveness. The search for a *specific* hypnotic psi-conducive state has another history and it is one which has led hypnosis research into uniting with the correlational approach. As a result, hypnotic psi research has encountered some of the hazards of the correlational approach that now need to be described.

In Part 1, research was reviewed showing how it is misleading to avoid the debate over whether the hypnotic state exists or not, because it has led to the rather naive assumption that this state empowers causal effects above and beyond what could be achieved by manipulations of individual variables in the waking state. Even assuming hypnosis is a special state, it appears rather clear from the reviewed research that this state is not a unitary one but is heavily influenced by a multitude of variables that intercorrelate.

There is a further serious and potentially fatal weakness in pursuing psi in the correlational way. This is the risk of falling into the quagmire formed by correlating amorphous sets of error variance. Tests of psi seldom give an effect beyond 10% of the error variance and many psychological tests are anything but pure measures of what they claim to measure. Moreover, the correlations of psi scores with external variables such as hypnotizability rarely, if ever, account for more than 20% of the shared variance. Seen from this perspective, the research data become a quagmire of error variance with little in the way of solid ground to get a grip on the psi, and this may be in part because of its so-called elusiveness. The situation is made even worse when we consider, as I noted earlier, that in hypnosis research the responses to many of these psychological questionnaires, such as those of absorption and dissociation, have been shown to be influenced by the context in which they are given. As we noted in Part 1, when the questionnaires are given separately from the treatment procedure, the supposed significant correlation virtually vanishes.

This is not to say that nothing has been learned or can be learned from the correlational approach. One plausible assertion is that something vital to classical hypnotic phenomena may have been lost by basing contemporary hypnosis research on the study of "normal" students in the psychology laboratory rather than studying special participants, who gave rise to the more dramatic claims for hypnosis.

Back to the Starting Point

An outcome of the intensive debate during the early 2000s in the journal *Contemporary Hypnosis* was the recommendation that efforts by made to rediscover the type of hypnosis that characterized the somnambulists of Charcot's and Janet´s time (Kallio & Revonsuo, 2003, 2005). The somnambulists of today were now to be renamed "virtuosos." It would seem that a major goal of at least some state supporters is now to find these virtuosos. A profile that seems in many ways to define virtuosos has resulted from the efforts to select individuals with high scores on the Dissociated Experience Scale (DES), the Tellegen Absorption Scale (TAS), and even the Transliminality scale.

In using dissociation as a key concept and measure, a key aspect that has not been addressed is the question of whether or not virtuosos are psychologically disturbed individuals or unusually gifted healthy individuals? We recall how leading researchers such as Charcot and Bernheim had opposing views on this issue, and this historical division set the scene for the future of hypnosis research, with the issue now reappearing in the present context. If we are searching for healthy and gifted individuals experiencing high dissociative states then the DES is not the appropriate instrument. The DES was developed as a clinical instrument to aid in the diagnosis of dissociative identity disorder (Bernstein & Putnam, 1986). Individuals who score high on the DES usually have severe relationship problems, report having fearful parental attachments, and have a low sense of coherence in life (Ray, 1996). DES scores of 20 or more are usually regarded as the cut-off in the general population for psychopathological responding. Wright and Loftus (1999) have

been outspoken about the skewing of the DES when it is used on the normal population and accordingly developed an alternative form (the DES-C). However this was not used in some of the major contemporary work on hypnotic psi.

The Search for the Elusive Psi-Conducive Hypnotic State

In view of the enormous complexity of hypnosis, reviewed in Part 1, it is not surprising that any effort to elucidate the literature on psi and hypnosis as to their possible causal connections becomes, if not an already forlorn quest, at least an extremely daunting one.

Such efforts benefit considerably from the meticulous meta-analysis published by Stanford and Stein (1994). The authors found 25 studies that used hypnosis along with control comparison conditions such as relaxation. The main finding was a strong experimenter effect amongst the 12 investigators involved in these studies. As far as the role of the hypnotic state was concerned, 21 of the 25 studies used a within-subjects design rather than a between-subjects design, which made the outcomes liable to expectancy and order effects. Worse, many of the studies showed gross flaws in design and absence of precautions. In some cases these deficiencies were even associated with the differences in psi scores between the hypnosis and control groups that seriously weakened any definitive conclusions about the role of hypnosis. The order effects in one major study led Stanford and Stein to write: "The hypnosis-comparison contrast was significant only when the comparison condition preceded hypnosis. The significance was due, substantially, to psi-missing in the comparison condition" (Stanford & Stein, 1994, p. 235). It has been supposed that psi-missing can operate as an effect of the holding back of best performance when the control condition comes first (see Parker & Millar, 2014).

As many of the studies used only a few highly selected and highly hypnotizable participants, there are several alternatives to consider: Hypnotic psi may be part of the virtuoso performance; it may be that the hypnotic procedure with its positive expectancy is a means of enhancing an already existing potential; or as Stanford and Stein seem ready to endorse, hypnotic induction per se may have no effect beyond a person-by-situation interaction (Stanford & Stein, 1994, p. 261). This focus on the "person" factor in the equation brings in the relevance of the three factors that were highlighted above in the ongoing controversy over hypnosis: fantasy-proneness, absorption, and dissociation.

Taking the first of these, Barber's fantasy-proneness, questionnaires have been developed that quantify both the frequency and the type of anomalous experiences and altered states in fantasy-prone individuals (Pekala, 1991). Cluster analysis of the types of individuals sharing responses showed that about 10% of the "hypnotically sensitive" report paranormal experiences (Pekala, & Forbes, 1997; Pekala, Kumar, & Cummins, 1992). This and the finding that the correlations between hypnotizability and reports of psychic *experiences* account for about 9% of the variance, led the authors to favor the view that psychic experiences do actually occur more often in those who have a special hypnotic ability (Kumar & Pekala, 2001, p. 275).

However, the earlier problems we encountered in Part 1 while trying to make sense of such relationships reoccur when examining hypnotizability and psi experiences. Could absorption underlie the relationship between fantasy proneness and "hypnotic ability"? Are the correlations reliable indexes of the strengths of the relationships given that the context of administering tests on the same occasion gives stronger correlations than administering the tests on separate occasions? Worse, several items on the absorption scale can be interpreted as relating to paranormal experiences and thereby creating a degree of tautology or overlap between the scales.

Experimental studies should help settle the issue. However there is apparently only one such major study relating absorption to psi scores—in this case with the ganzfeld as the method of inducing psi (Dalton, Zingrone, & Alvarado, 1999). Participants were highly selected, with a background in art, music, drama, and even meditation, all of which might easily have contributed to the outcome. Three variables highly relevant to the debate on the hypnotic state were included: absorption, dissociation, and altered state. The results using overall first rank hits as the outcome measure were rather spectacular in that the number of hits was more than double the chance expectancy.

With such a clear effect, there can be some confidence that the correlations actually relate to psi rather than to extraneous factors. Decisively, the psi scores failed to relate to dissociation as measured by the DES, but they did show a moderately significant relationship to absorption. On the other hand, the attainment of an altered state through the use of the ganzfeld did not relate significantly to the psi scores, which was contrary to what had been predicted. Considering the scarcity of such strong psi effects, it is regrettable that the report (as appears to be the case with most of the work under the leadership of Dalton) is not published in a peer-reviewed journal.

It is also relevant to mention here that our Gothenburg real-time digital ganzfeld study was not successful in that the absorption scale did not distinguish psi scores, but it should be noted that most of the participants were already high scorers on absorption, thereby rendering the test insensitive as a predictor of scoring (Parker, Grams, & Pettersson, 1998).

Despite the above failure to find support for dissociation (DES) scores in predicting psi scores, this relationship may not be entirely invalid. An analysis of the DES scores for attendees at conferences on psychic experiences showed that they correlated moderately with the self-reported frequency of subjective psi experiences. However, the respondents here claimed that the psi experiences did not occur in truly dissociated states but "rather as an intrusion into an otherwise normal, conscious state" (Richards, 1991, p. 87).

It may be that there is an interaction between scores on hypnotizability, dissociation, paranormal belief, and psi experiences. The statistical analyses of scores on the Anomalous Experiences Scale indicate that these factors interact in a way that make some individuals more sensitive than others to psychic experiences and abilities (Kumar & Pekala, 2001). A skeptical explanation would of course be that hypnotizability together with dissociation enhances fantasy proneness, and paranormal experiences are merely a feature of this fantasy proneness leading to the delusionary belief of being psychic.

Experimental studies are clearly much needed if we are to come closer to any definitive answers. Four such studies have been reported in the literature since Stanford and Stein carried out their meta-analysis. A problem with all these studies is that when they followed the design described earlier of comparing low and high hypnotizable groups, these and further subdivisions left a very small number of participants in the final comparison groups. Applying statistical analysis to this kind of data often results in "power failure," which typically arises through the use of small samples, low power, flexible designs, and flexible statistics. This increases the likelihood of false positive results, and the subsequent failure of such findings to replicate has been called the "winner's curse" (Button et al., 2013).

One of the first of these contemporary hypnotic psi studies, by Del Prete and Tressoldi (2005), used "hypnotic sessions" to produce a "hypnagogic state" in order to facilitate scoring on a forced-choice ESP task using static targets. The study involved 12 participants previously selected for their high scores on the absorption and transliminality scales. Two conditions were compared for their effect on psi scores. One of these conditions was what they called the "hypnagogic state" whereas the other condition was "self-induced relaxation." The hypnagogic condition gave a significant hit rate of over 37% whereas the control procedure (self-induced relaxation) gave results close to the chance expectancy of 25%. Although most contemporary authorities on hypnosis would clearly avoid confusing the hypnagogic state and hypnosis, the hits of the hypnagogic group correlated positively and significantly with the scores on the absorption scale (r = .76) and on the transliminality scale (r = .71). This might be considered surprising given that the participants had already been selected for high scores. The study is clearly remarkable given the significant findings with so few participants.

Tressoldi and Del Prete (2007) carried out a second study which can be regarded as an attempt at replicating and extending the above design. The difference was that the hypnotic instructions were supplemented with suggestions that were focused either on the use of ESP or the induction of an OBE to view the target. Although the authors concluded that their findings replicated the earlier ones, it is evident from the results that formally this was not so, because the scores in both conditions were close to chance expectancy, except for what would seem to be a post-hoc session effect and marginally significant (by two-tailed tests) correlations of hits in the ESP condition with absorption and transliminality scores.

It is unfortunate that in both the above studies rather scanty details of the target selection and security aspects of the procedure are given. Moreover, it would seem following further inquiry that the same

target selection may have been used for all participants (Tressoldi, personal communication, March 10, 2014). If so, this would be a serious flaw as it would allow for order effects and possibly even stacking effects to occur in the data.

A further study was reported by Parra and Argibay (2013) using a design which compared the psi performance of groups selected on hypnotic suggestibility and subjective psychic experiences. The highly suggestibility group (n = 20) scored at 40% where MCE was 25%, which was significantly higher than the low suggestibility group. It is unfortunate given the success of the experiment that the possibility of handling cues in the judging procedure was not eliminated (Parra & Argibay, 2013).

The method of splitting participants into contrasting groups on the basis of scores also formed the basis for a study of dissociation and hypnotizability by Cardeña and co-workers in Lund. Their first study compared the psi scores of participants who had been selected for their high or low scores on hypnotizability. The aim of this study was to make a further division amongst the participants into those with high versus low DES scores and evaluate the psi scores of this successive division. Although some minor post-hoc significant results were found, the crucial and major hypothesis, that high hypnotizables and high dissociaters would excel over the low hypnotizables and low dissociaters, failed to gain any support from the data (Cardeña, Marcusson-Clavertz, & Wasmuth, 2009).

A further study was carried out by Marcusson-Clavertz and Cardeña (2011) along similar lines, but this time the ganzfeld state was used as an intended means of producing and evaluating psi performance. The study merits some detailed scrutiny given that the claims that were made for it ignited an academic and a public controversy (Halle et al., 2012). There appears to have been logistical problems from the beginning: a handful of participants, a study overloaded with variables and hypotheses, and a design that was part of a conventional study concerning "mind wandering." The overall results, based on the use of both direct hits and sum of ranks in the ganzfeld, lay close to chance expectancy. For most critics of the ganzfeld, this would be the beginning and the end of the significance of the study. This was, however, not a standard ganzfeld experiment with relatively normal individuals but used hypnotizability and dissociation in order to arrive at contrasting groups of individuals.

The above problem concerning "power failure" due to the low number of individuals in the various groups becomes then extremely acute. Moreover, given that individuals were chosen from the upper and lower ends of already skewed distributions, it is likely that some of the tests chosen by the authors violated the rules for the use of parametric statistics. When numbers are reduced to between 5 and 9 in the various groups, the investigation moves dangerously close to a case study. In this instance, the report is devoid of the qualitative information about individuals that such a study can provide, so it unfortunately falls between the two worlds.

The main hypothesis was that the combination of high hypnotizables and dissociaters would give superior psi scores, but this was not confirmed. Neither did, as predicted, the dissociation scores relate significantly to the psi scores. However, after controlling for the sheep-goat effect, a significant *negative* relationship was found between psi scores and hypnotizability, a finding that was counter to the hypothesis being tested. After looking at the distribution of scores on the Phenomenology of Consciousness Inventory (PCI), the authors then proceeded to speculate that some of the high dissociaters may have failed to engage their psi because they were unable to use the ganzfeld to enter an altered state. Although dissociation clearly did not relate to psi scores, the authors reported (apparently based on five participants) that the high dissociative-high hypnotizables seemed to be correct when they relied on their "hunches" rather than their imagery".

In terms of the prior hypotheses, the only indisputable positive finding is a form of sheep-goat effect in which belief in the individual's own success correlated significantly with the psi z scores (although the specific measure used to test this hypothesis may not have been prespecified).

The study appears to present many post-hoc findings although they were not identified as being post hoc. Perhaps the most promising one of these concerns the previously described PCI, which in this experiment was used to access the altered state experienced during the ganzfeld sessions. Scores on the PCI were found to correlate significantly with the psi z scores, but only amongst the high hypnotizables. However,

as the authors freely admit, there are serious problems with this finding because the PCI was administered after the first (nonpsi) ganzfeld session as part of a "mind wandering experiment" and not after the actual psi-ganzfeld session. This makes it rather unclear as what lies behind the significance. Was it the actual state during the psi experiment, the PCI scores, the expectancy created by the nonpsi ganzfeld session, or the "winner's curse" referred to above?

It is difficult to go further with such data but we can try to answer the fundamental questions raised earlier concerning the nature of dissociation: How do we measure it and what do we mean by it?

Is Dissociation a Healthy or Pathological State and Does Dissociation Relate to Psi?

If we look at the psychometric market we find there are several different questionnaire measures of dissociation (Carleton, Abrams, & Asmundson, 2010; Harrison & Watson, 1992; Körlin, Edman, & Nybäck, 2007; Riley, 1988) as well as some potentially direct measures (Palmer, 1994**,** 2011, 2013).

We can begin with the previously described experiment by Marcusson-Clavertz and Cardeña (2011) which used the more pathologically oriented Dissociated Experience Scale (DES) for selecting high dissociaters. Although their report regrettably does not supply any information about their mental health, it should be added that far from all the high scorers met the criterion for a DSM diagnosis (Ray, 1996). On the other hand, the series of psychological studies of hypnosis at Lund found that the high-dissociation-high-suggestible group had "elevated pathological dissociation and fantasy proneness and reported a greater history of exposure to stressful events (Terhune, 2010, p. 31). It is worth remembering the study by Barrett (reviewed in Part 1) in which he interviewed highly hypnotizable persons and described about half of them as "fantasizers" and the other half as "dissociaters" (Barrett, 2010). The majority of the dissociaters reported nightmares and memories of childhood trauma and abuse (although no attempt was apparently made to check on the validity of these experiences). As the dissociaters were found to be more likely than the fantasizers to believe in the reality of their hypnotic hallucinations, this study provided some support for the idea mentioned earlier that virtuosos were hallucinating rather than merely imagining the suggestions.

The explanation for finding any potentially "normal" or "healthy" individuals amongst high DES scorers, such as may or may not have been the case in the Marcusson-Clavertz, and Cardeña (2011) study, can lie in the apparent bimodality of the DES. More disturbed patients are found to fall on the part of the curve based on responses affirming the presence of amnesia, depersonalization, and derealization, and even post-traumatic stress disorder. By contrast, apparently normal and healthy individuals with high DES scores mainly affirm the items concerning absorption and imaginative experiences (Körlin et al., 2007; Ray, 1996; Waller, Putnam, & Carlson, 1996).

This suggests that using dissociation in this latter restricted sense of absorption and imaginative ability is a more promising route forward, and this is especially so when we bear in mind the fairly consistent results which relate absorption to subjective psi experiences.

The obvious conclusion that emerges is that if we wish to pursue further the quest for the psi-conducive state there may be good reason to focus on the concept of dissociation, but then it is necessary to distinguish the positive and healthy aspects versus the pathological aspects (Wright & Loftus, 1999). In searching for an instrument to measure dissociation in the normal population, there exist, as well as the earlier mentioned DES-C, several other variations that seem to share the common view of relative healthy dissociation as encompassing absorption and attentional distraction (Carleton et al., 2010; Harrison & Watson; 1992; Riley, 1988). The most established of these test instruments is the Dissociative Processes Scale (DPS), which is derived from the DES. It shows good reliability by having very high alpha coefficients, and it is adapted to the normal population. In addition to the normality aspect, what speaks for the future use of the DPS is that it has as its main factors: absorption, obliviousness, and detachment (Watson, 2001).

Palmer used and evaluated a range of dissociation measures as part of a relentless effort to find a reliable means of detecting psi at an unconscious level. His rationale is that psi functions best when tested subliminally and in a dissociated state (Palmer, 1994**,** 2011, 2013). In the initial series of explorative studies

Palmer carried out, he designed a means of testing psi, the Perceptual ESP Test (PET) that might be sensitive to spontaneous dissociations or mind wandering. The test used shaped carets (directionally placed V-shaped symbols) to reduce the response bias that occurs with traditional ESP card symbols. To measure dissociation, Palmer used one of the above scales designed partly for the normal population, the Questionnaire of Experiences of Dissociation, and supplemented this with ongoing dissociative state reports during the responses to the PET. This essentially assessed the degree to which the receivers felt suddenly compelled to look at the specific area of the screen at the point in time when the sender was viewing the target carets. The experimental designs also incorporated various subliminal effects on ESP scores based on Palmer's earlier work with Martin Johnson using the Defense Mechanism Test. These were found to be effective only in participants who reported relatively high scores on the dissociation measures (Palmer, 1994).

The notion of utilizing dissociation as an outside force for influencing choices in ESP experiments then became a distinctive feature of Palmer's later work (Palmer, 2011). To make fuller use of this, he used a form of the Ouija board in which receivers were to try to identify the letters that composed one of the five possible target words being viewed by the sender. Palmer used this time as the "trait measure" of dissociation the Complex Partial Epileptic Signs scale with the TAS partialled out. For a "state measure" of dissociation Palmer simply asked about the degree of outside force participants felt that appeared to be directing the pointer across the Ouija Board. The main incontrovertibly significant finding was a curvilinear relationship between the ESP scores and the influence of an outside force. This meant that those who felt the influence up to 40% of the time scored significantly on the psi test. Although this was a post-hoc finding, it is one that made sense, as those with very high scores could be conceived of as being unrealistically overconvinced of their ability.

In Palmer's current effort in this area dissociation was used again to predict psi scores with another derivation of the Ouija Board (Palmer, 2013). In this case the Ouija Board took the form of a modern computer tablet divided into squares where participants were required to locate clairvoyantly the square designated as the target (reminiscent of the childhood game of cruisers and battleships). For the trait measure, Palmer now turned to the above recommended Dissociative Processes Scale but retained the same state measure concerning the degree of influence of an outside force on the hand**.** When reconsidering the degree to which this force was felt, Palmer later thought that because the new participants seemed to differ in certain key aspects from the earlier ones, those reporting an influence over 40% of the time should now be included. With this criterion in place, the DPS proved to be the most significant predictor of ESP scores. The unique and positive feature of Palmer's work, although it suffers from multiple analyses, post-hoc findings, and ad-hoc hypotheses, is that it combines a correlational approach with a novel functional state approach and focuses on normalcy.

These studies with this revised "normalization" of dissociation seem then to offer some land winnings but the revision may also lead to some conceptual reformulations. To grasp the wider applications of the proposed revision of the dissociation concept, we need to look at a third area of dispute concerning altered states: namely, the findings on sleep and dream processes. A similar, often heated, debate to those over the hypnotic state and the existence of psi is found here. In this case the dispute is over whether the dream states have a psychological meaning or if from a skeptical reductionist standpoint they are mere epiphenomena. This is a debate that reflects many of the same parallel issues that have occurred in the psi and hypnosis debates. Readers who have an insatiable interest in controversies are referred to the *Brain and Behavioral Sciences* issue on the topic (Hobson, Pace-Schott, & Stickgold, 2000) and the three-volume *The New Science of Dreaming* (Barrett & McNamara, 2007).

Entering this, the third potential jungle of research findings, is beyond the scope of this review so we choose to keep within the secure path mentioned above, that of the positive measure of dissociation. What speaks for this choice of path is that a positive concept of dissociation has given rise to a theory in dream research that promises to bring together the many disparate and incongruous findings, not only in dream research, but also in hypnosis and psi research. This approach is termed the "continuity of conscious states theory," because it highlights the degree of continuity in the human capacity for experiencing various altered states of consciousness (Watson, 2001, 2003).

The Continuity of Consciousness Theory

The theory proposes that individuals who have intense, novel, and unusual experiences are more likely to recall their dreams and integrate them into their waking life. This capacity enables a link between sleep and waking states and even becomes expressed in semidissociated states such as absorption in fantasy and daydreaming. The term "sleep-related experiences" is used to include a variety of altered states that may occur nocturnally. The continuity of consciousness is said to occur potentially in the recall of nightmares, vivid dreams, recurring dreams, lucid dreams, and problem-solving dreams. The theory has support in research findings that show dream recall to be related to fantasy proneness (Blagrove & Hartnell, 2000), transliminality (Soffer-Dudek & Shahar**,** 2009), and absorption (Schredl, Jochum, & Souguenet, 1997; Watson, 2003). With a singular exception (Knox & Lynn, 2014) the theoretical implications of this work do not however appear to have reached either hypnosis researchers or, to my knowledge, psi researchers.

What gives this continuity of consciousness theory some plausibility is the evidence that dreams have a direct influence on waking experiences as part of a reciprocal flow of consciousness in which the waking experiences influence dream content and these in their turn come to influence waking life (Schredl & Hofmann, 2003). There is here a tangible clue for finding the way forward, because sleep-related experiences appear to have a positive relationship to measures of "transliminality": the ease with which previously unconscious psychological material enters consciousness (Soffer-Dudek & Shahar, 2009).

Clearly, a large degree of individual variation is expected to occur in the propensity for experiencing the continuity of various altered states. For instance, the recall of sleep-related experiences significantly positively relates to dissociation (Giesbrecht & Merckelbach, 2006; Giesbrecht, Smeets, Leppink, Jelicic, & Merckelbach, 2007).

However some skepticism concerning the continuity and reciprocity of relationships here seems appropriate: Do sleep related experiences occur due to transliminality or do sleep disturbances create the dissociative experiences (Lynn, Lillienfeld, Merckelbach, Giesbrecht, & van der Kloet, 2012; Soffer-Dudek & Shahar, 2012)? Statistically, in terms of multiple regression equations, this should be a relatively easy question for future research to answer but it does lead us further into the clinical area.

In the clinical area, there is a contradiction as to what might be expected from the pigeonholing of medical diagnosis. This concerns the evidence that symptoms of dissociation correlate significantly with some of the signs of schizophrenia (Spitzer, Haug, & Freyberger, 1997). The relationship between dissociation and signs of schizophrenia in the normal population appears relatively strong, but the strength of that relationship depends on the inclusion or exclusion of the common factor of depersonalization (Watson, 2003). This finding can be readily understood if we take a dimensional view of psychosis and schizophrenia rather than the traditional medical one (Bentall, 2009). The dimensional concept allows for the existence of so-called healthy, positive individuals whose symptoms often are limited to anomalous experiences (Claridge, 1997; Nelson & Rawlings, 2010). Whilst the occult beliefs of such individuals can merit at least in psychiatric circles a diagnosis of schizotypal personality, many of these individuals appear to be well-organized and structured, so as not to be of much clinical concern (Goulding, 2004). As such they often are called "happy schizotypes," although the expression "schizotypy" may be a misnomer considering the apparent healthy status of such individuals (Claridge, 1997).

Claridge, who carried out the pioneer work on "positive" or "happy" schizotypes, joint-authored a paper reporting that the schizotypy measure correlated with distressful nightmares (Claridge, Clark, & Davis, 1997). Yet, surprisingly, schizotypy also correlated significantly with dream enjoyment, leading the authors to construe "schizotypy as an essentially neutral personality characteristic, which includes among its cognitive features the tendency towards a wide range of unusual perceptual experiences and thought styles (Claridge et al., 1997, pp. 384–385). This line of reasoning may explain why schizotypy, assessed on the basis of the Unusual Experiences Scale, has shown moderate relationships with both creativity and with absorption (Nelson & Rawlings, 2010).

Similar conclusions concerning the relationship of anomalous and psi experiences to schizotypal personality were reached by Simmonds-Moore (2009) but by a different route. Instead of relating these

experiences to a common ground in absorption, Simmonds-Moore explored the relationships between psi experiences and just about all the variables that have been implicated at one time or another as having a potential relationship to psi (dissociation, transliminality, boundaries, temporal lobe lability, and creativity). She concluded that thinner boundaries would naturally allow less filtering of information to the brain and form the basis of what she preferred to call the "anomaly prone personality." This filtering of extrasensory information can, however, go in a positive or negative direction depending on the degree of control over the anomalous experiences. Control is then the hallmark of healthy functioning (I would add "integration") that distinguishes this personality from the disturbed schizotypal personality. The problem is that while some of these variables may show weak correlations with anomalous experiences (Irwin, Schofield, & Baker, 2014), there is little reason to believe they can function as predictors of actual psi performance in the laboratory. Even Simmonds-Moore's own results on this cast serious doubt on the utility of these variables (Simmonds-Moore & Holt, 2007).

There can be many reasons for this failure, some of which were mentioned earlier. Even if the evidence is lacking, it might still be that the successful participants in psi experiments, especially those in hypnosis and ganzfeld experiments, belong to the "positive schizotypy group," rather than showing the more extreme symptoms of dissociation and psychosis proneness. It was after all from groups of "positive schizotypes" with their own psi experiences that we recruited most of our successful participants in our psi-ganzfeld experiments. They were as a group characterized, as might be expected, by high scores on magical ideation and absorption (Parker, 2000; Parker et al., 1998). By contrast, Simmonds-Moore and Holt (2007), who failed to replicate the relationship to schizotypy, recruited their relatively few volunteers from university staff and students.

Whatever the reason for this failure, it serves as a reminder to take heed of the demands for purely predictive findings—"psi on demand"—made by the critic Alcock (2003). It seems evident that correlational findings fall far short of this, if they are not in fact steps in the wrong directions.

What Can Be Learned From the Failure to Predict?

It is possible that an intensive study of virtuosos and star participants might turn the situation around if we use another approach focused on functional relationships. As we related in Part 1, for instance, the intervention of secretively and suddenly changing the sender in the ganzfeld procedure produced a seemingly appropriate response ("Where have you been?") in the receiver (Parker et al., 2000). This indicated, at least for these researchers, that psi is more than a statistical anomaly, in that it follows mental causality. An earlier study with two star performers gave some further apparent insights into the nature of psi beyond what could be achieved by correlational studies. A companion paper to this (Parker & Millar, 2014) related how two successful psi performers continued to score high with experimenters who previously had a track record of being so-called "psi-inhibitory." Moreover, for the few trials that I had time to carry out, I reversed roles with one of the star participants, and in the role of receiver I then obtained high scores. This all suggests that with the right atmosphere and right expectancy, there does appear to be a degree of robustness in the psi scores of some star performers that can potentially fulfill the demands of critics such as Alcock.

In the third of the Gothenburg ganzfeld-psi series of experiments, seven pairs of participants who had made direct hits were invited back, and four of these repeated their success in making again direct hits (Parker et al., 1998). With hindsight, we should have made an intensive study of these individuals, but the lack of funding led to the diversion of resources to other areas.

This was a missed opportunity but what we may want to learn from the above is that both correlational studies using tests of transliminality, dissociation, and schizotypy and studies applying functional methods to the ganzfeld and hypnosis should focus on following up the selected high psi performers. It is also clear that we need to be more innovative. It is often thought of as taboo to vary the conditions during the running of experiments, but as the anecdotal examples given earlier suggest, it is by such making such unexpected interventions that we can actively explore causal effects on psi scores. Perhaps some of these suggestions will help free research from a morass of error variance.

The fundamental issue, which was also raised in Part 1, is are we prepared to consider that hypnosis and placebo effects can potentially engage awareness beyond its normal constraints and thereby produce what appear to be remarkable phenomena? If so then concepts such as transliminality, meditation, and mindfulness may offer alternative explanatory systems for the more dramatic effects. This is a line of thinking that a recent spate of articles seems to support (e.g., Alladin, 2014; Halligan & Oakley, 2014; Krippner, 2014). Nevertheless in fairness and completeness it should be mentioned there are of course even other views—for instance that hypnosis is a species of trance phenomena with diminished rather heightened awareness (Crabtree, 2012).

Just how hypnosis relates to other so-called mediumistic trance states is obviously a matter that should be resolved by empirical research. Some preliminary modern work was, recently reported that has relevance here (Roxburgh, 2007; Roxburgh & Roe, 2011). Significantly, mediums were found to be higher than nonmediums on psychological wellbeing and to experience lower distress. No significant differences were found as regards boundary thinness or dissociation, but it should be noted the researchers used the pathological measure of dissociation, the DES, rather than the normality oriented DPS. Regrettably, beyond these studies, there is mainly historical material to go on (Garrett, 1938; Sidgwick, 1915).

In the debate over how certain forms of dissociation and schizotypy may relate to hypnosis, the health versus pathology issue achieves considerable theoretical importance. In focusing on the healthy and positive aspect of altered states, an argument can be made for the viewpoint, consistent with the above literature review, that these states enable the expression of latent potential (Tart, 1977), especially latent creativity (Lynn & Sivec, 1992).

Conclusions

Recently I discovered how the Swedish physician and foremost hypnosis researcher, John Björkhem, came more than 60 years ago to similar conclusions to the above and those of the earlier papers in this series. Björkhem's conclusions were apparently based on 30,000 experiments with 3,000 individual subjects carried out between 1930 and 1950 at Uppsala and Lund universities (Björkhem, 1953a, 1953b).

Some of the studies have been part of one of his doctorates and his subsequent work (Björkhem, 1943; Björkhem, 1951) but no systematic evaluation appears to have been published in peer-reviewed journals. Even so, in terms of the numbers of experiments and participants, all contemporary psi studies fade into insignificance by comparison. Björkhem's conclusions, given the enormity of his experience and of his data, are worth considering. Moreover, his conclusions correspond closely with those being arrived at here. They can be summarized as follows:

1. The most psi favorable state is not deep hypnosis but slight changes in consciousness.
2. Those subjects engaged in somnambulistic states have to learn to distinguish the psi state from that of fantasy and hallucinations.
3. Other persons present may exert an influence on the outcome.
4. Observations of other successful experiments promotes the appearance of the phenomena.
5. To produce the right state of mind for psi in participants, the experimenter must make deep psychological contact with the participants.
6. Extremely subtle testing conditions can also influence the nature of the phenomena.
7. The psi factor is a gift and appears to have a genetic endowment.
8. The way forward is to use gifted mediums.

Some of the above conclusions have been previously discussed but most merit further comment. If the first claim concerning slight changes in consciousness being a psi conducive state has validity, then any exclusive reliance on hypnosis may indeed be a false lead or an unnecessary complication. This point emphasizes not the state per se but slight changes in it. What has been termed the "change in state hypothesis" has up to now only been briefly studied with the ganzfeld with inconclusive results (Parker, 1994).

The hypothesis deserves more decisive investigation not only with the ganzfeld and hypnosis but in the field as a whole. In view of the findings relating psi data to absorption, it may be the case that participants show spontaneously changes in state that are psi conducive. In earlier unpublished work I carried out with Brian Millar, we individually tested a high scoring participant whom I had just discovered. The participant was asked to give state reports (as described by Tart, 1972) for each run of 25 trials with ESP cards. These state reports failed to predict scoring, but in making our analyses we were in fact entering the mire of meaningless correlations described earlier in this paper. Today, further examination of all the scores from the 20 runs carried out reveals there were only three scores of (9, 10, and 13 where MCE = 5) that we could be reasonably certain gave hits that were not due to chance. For the two highest of these scores (and none of all the other scores) the participant remarked that he noticed a sudden change from relaxation to tension. What is then needed is a study of the cases of other high scoring participants, with the specific objective of lifting out only the occasions when they are producing high scores or remarkable correspondences in the content of their mentation that cannot easily be attributed to chance.

When Björkhem (1953b) additionally adds: "In certain cases an influence from the experimenter seems to condition the emergence of the psi factor" (p. 86), he appears to have pre-empted the current focus on the experimenter effect and possibly experimenter psi. As far as we can judge his own role as an experimenter, it has to be said that by any measure Björkhem was an extraordinarily gifted and charismatic individual. He gained doctorates in theology and psychology, published five books, and became a licensed physician. He was said to be the most educated in Sweden of the time and that it would appear that it was only his interest in researching psi which excluded him from an academic position (Stolt & Björkhem-Bergen, 2004). Björkhem's remarkable success teaches us that we need to select and study the gifted experimenter as well as the gifted participant but it teaches us more than this. As it was noted in Part 1, Björkhem's success even with hypnosis itself failed to work in a change of culture and in the ambience of Rhine's laboratory at Duke University. Clearly, we need also to examine the context and ambience in which psi-conducive experimenters best function.

The third point that Björkhem raised can be expressed in contemporary terms as that of "observational learning." This aspect has been nearly totally neglected in parapsychology but is a major principle in the teaching skills and self-efficacy, especially in the field of sports psychology. It was because of this that in initiating the Gothenburg ganzfeld work we arranged for a visit by the successful ganzfeld researcher Kathy Dalton accompanied by her videotaped recording of her best ganzfeld hits. It seems also relevant to mention how the PK success of Felicia Parise was inspired by observation of Nina Kulagina's apparent PK performance (Honorton, 1993).

The existence of a genetic influence on psi, which Björkhem testified to, has also received little investigation unless we include twin studies (Brusewitz, Cherkas, & Parker, 2014; Jensen & Parker, 2012; Playfair, 2013). In Björkhem's own case the claim may actually have an anecdotal aspect. Besides his academic and clinical achievements, Björkhem fathered five children all of whom became academically successful. One of these was Örjan Bjorkhem, who seemed to possess his father's ability to find and even produce psi phenomena (Johnson, 1998). Interestingly, shortly before his premature death Örjan Björkhem gave an account for the first time in English of his PK successes, along with a detailed theory that proved to be remarkably similar to that of Batcheldor, although he was unfamiliar with that work (Björkhem, 1994). What appeared to unite both the Björkhems in their view of hypnosis is that it was a culturally available and effective procedure, which allowed the participants to shift their view of reality to include psi.

So is the search for the hypnotic psi-conducive state, independent of culture and the historical era, a futile search such as being lured by fools' gold? Perhaps so, but some valuable findings and more sophisticated ways of thinking have emerged. In studying apparently psi-gifted individuals, the 60 years of "hypnotic state wars" have given us greater precision in measurement as well as a deeper and broader perspective on these issues and, perhaps, some consensus. In terms of consensus, it is hard not to agree with the two major protagonists in the hypnosis debate, Kirsch and Kihlstrom, on the importance of rigorous methodologies and on the relevance of hypnosis for the wider understanding of how human cognition works (Kihlstrom, 2014; Kirsch, Mazzoni, & Montgomery, 2007; Lynn, Woody, Montgomery, & Gaudiano; 2014). Likewise,

I find myself agreeing with Cardeña (2014) as to the relevance of both hypnosis and psi for understanding the nature of consciousness and perhaps even for solving some of the major problems of psychology.

As outlined earlier, besides the methodological weaknesses, the disagreement concerns the complexity of the variables involved in psi and hypnosis, which, in a time of limited funding, creates a series of intractable problems. Expressed in the Rhineian symbolism chosen here, it bogs down research in the correlational quagmire. Accordingly, should further expeditions be made into this area, much better equipped and sophisticated designs with large groups of individuals will be required.

A more viable alternative might be the repeated testing of selected performers, who are gifted at altering their state of consciousness. This would enable a focus on the personal history and characteristics of individuals who produce high-quality psi. In view of what was presented in a previous paper concerning the success of psi-conducive experimenters as their own successful subjects (Parker & Millar, 2014), then self-experimentation is a legitimate option that psi research needs to embrace. Luke (2011) succinctly expresses this in his praiseworthy presidential address to the Parapsychological Association: "experiential reclamation" is now part of the agenda. This can potentially not only generate new and valuable findings but can in the long term fulfill the currently vocal demands for replication.

Finally, it seems appropriate to reveal my own experience and bias. Although I have worked for many years with hypnosis in clinically and research contexts, I confess to having no final and precise opinion as to what hypnosis is. I do however have a strong opinion that an understanding of hypnotic and psi experiences goes hand-in-hand with learning about what consciousness is. It is often said (and attributed to Richard Feynman) that those who believe they understand quantum mechanics, do not. I assert that the same thing can be said about consciousness and its altered states.

Perhaps we can go one step further. If as is often claimed in contemporary physics and astronomy, our human consciousness potentially possesses the ability to fully understand the universe and is capable of a "theory of everything," then this naturally presupposes that the complexity of the universe has evolved along with our consciousness. Our consciousness, and the understanding of it, would thereby appear to be a vital and an integral part of the sought-after theory of everything.

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Acknowledgements

The author wishes to thank in alphabetical order: Brian Millar, Nemo Mörck, Donald West, and the two anonymous reviewers for their comments.

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