

ESP RESEARCH  
AND INTERNAL ATTENTION STATES:  
SHARPENING THE TOOLS OF THE TRADE

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Circumstances that favor what Honorton (1977) has called "internal attention states"—circumstances such as ganzfeld and hypnotic induction—have provided some of the most replicable laboratory evidence of extrasensory function (see, for example, Honorton, 1985; Schechter, 1984). Nevertheless, the current evidence fails to establish even that it is the presence of an internal attention state (IAS) in such settings that favors ESP-task success or whether that success is due to something else that is provided by those circumstances (perhaps, for example, expectancy of success or reduction of ego-involvement with the task.). The current evidence is certainly inadequate to establish either that it is an IAS per se that is important to success or that the IAS favors success for particular reasons in such settings (Stanford, 1987a). In the paper just cited, I tried to show that this equivocal state of the evidence should not be a cause for pessimism, rather, that it is precisely what should be expected, given the paucity of any substantial body of methodologically adequate experimental work addressing such problems. There is little reason to suppose, in other words, that the present lack of clear knowledge in this area is because of some kind of intractability of the subject matter being investigated.

That is not, however, to say that productive, replicable, high-quality work in such areas will be easy. There is no doubt that outside parapsychology, research areas such as hypnosis have seen more than their share of controversy and that conceptual disputes about what happens during "hypnosis" still rage in the pages of psychological journals. What is more, the interpretation of particular research findings in the hypnosis suggestibility area is regularly mooted in the pages of nonparapsychological journals. (Some examples of such controversy will be noted and discussed below.) If psychologists are themselves unclear about what is really happening when, say, an individual is "hypnotized" or about what factors are most important in contributing to hypnotiz-

ability, perhaps parapsychologists can feel less abashed about not having made quantum leaps in their work using tools, such as hypnotic induction, that could be said to involve "internal attention states."

However, the purpose of the present discussion is not to provide parapsychologists with a warm, comfortable glow about the present lack of understanding of ESP-task success in ganzfeld and following a hypnotic induction. It is, rather, to suggest ways in which parapsychological researchers might take responsibility for insuring that the present state of ignorance does not persist, either with regard to interpreting ESP-task success in such settings or with respect to understanding the psychology of such settings. The latter is, in my own view, a necessary part of the discussion because (a) psychologists researching such areas might profit by some help and (b) without advances in the psychological knowledge of such areas there is little prospect that parapsychologists will be able to advance in understanding ESP-task performance in ganzfeld or following a hypnotic induction.

First it will be useful to consider what conventional psychology might have to offer the investigator of ESP-task performance in settings such as ganzfeld and hypnosis. Which particular such borrowed methodologies might be of real benefit will obviously depend not only upon the particular objectives of the study to be undertaken, but, more specifically, upon what theoretical concepts are guiding the study.

#### *Hypnosis and Other Internal-States Areas: What Has Psychology to Offer?*

*Measures of Hypnotic Susceptibility (Hypnotizability).* Honorton (Honorton, 1972; Honorton, & Krippner, 1969; Honorton & Stump, 1969) was one of the first parapsychologists to recognize that if one is to attempt to boost ESP-task performance through the use of hypnosis, one must recognize that merely going through a hypnotic-induction ritual with a subject does nothing to guarantee that the subject has been hypnotized or has, responded favorably to the procedure. Consequently, the use of standard scales of hypnotic susceptibility is highly desirable in order to assess the degree to which individuals are hypnotically responsive and, perhaps, to assess their levels of proficiency at several of the tasks that can be tried with hypnotized subjects. This same theme was reiterated by Tart (1980) who made a number of specific recommendations in this regard. [A number of important methodological issues are mentioned and briefly discussed in the Honorton and Krippner (1969) paper; it deserves re-examination by contemporary parapsychologists.]

For preliminary group screening Tart (1980) recommended the Harvard Group Scale of Hypnotic Susceptibility, Form A (HGSHS:A) (Shor & E. C. Orne, 1962), a scale modeled after the individually administered Stanford Hypnotic Susceptibility Scale, Form A (SHSS:A, Weitzenhoffer & Hilgard, 1959), and which, like it, emphasizes motor-response items. However, objective response to the HGSHS:A is judged by subjects themselves after the session is completed. The HGSHS is certainly an adequate preliminary screening tool; indeed, it is almost certainly the most widely used such instrument in the contemporary literature. [Investigators wishing to use the HGSHS:A should, however, consult a study by Kihlstrom and Register (1984) indicating the importance of including reversibility of suggested posthypnotic amnesia as the criterion for scoring posthypnotic amnesia. Their report also discusses scoring criteria for this item.] The HGSHS:A should not, however, be the single instrument used if one is interested in locating hypnotic virtuosi; it simply does not do well in predicting who will perform very outstandingly on an individually administered, more advanced test such as the SHSS:C (Register & Kihlstrom, 1986). These authors also provide evidence that the ability to identify highly susceptible individuals (such as can be identified by the SHSS:C) through the use of the HGSHS:A might be enhanced—if one is in the undesirable position of using only the HGSHS:A—by using measures of subjective response to the test, not just behavioral response reports.

Tart (1980) has properly suggested that for adequate, in-depth study of hypnotic susceptibility as a factor in psi-task performance, additional evaluation is needed beyond the HGSHS—as well as subjects becoming accustomed to the laboratory hypnosis setting and to hypnosis itself. For such purposes, the use of individually administered scale(s) is desirable, including the SHSS:A, SHSS:B (intended as a parallel form for SHSS:A, in order to allow retest studies), and SHSS:C (a nonparallel form that, much more strongly than either A or B, emphasizes cognitive-perceptual items). Items included in C that are not included in A concern taste hallucinations, dreaming within hypnosis, age regression, anosmia for household ammonia, response to a hallucinated voice, and a negative hallucination (not seeing something actually present) (Hilgard, 1977; Weitzenhoffer & Hilgard, 1962).

Tart does not explicitly say so, at least in the published abstract of his convention presentation cited above (1980), but an advantage in using a scale such as SHSS:C with its advanced list of items is that one is able to identify persons who definitely have the cognitive skills (and inclinations) necessary for cognitive-perceptual effects in the hypnotic context. As a consequence, they are individuals likely to exhibit what

Weitzenhoffer (1974, 1980) has called the "classical suggestion effect," that of experiencing response to suggestions, including motoric ones, as involuntary, as happening "on their own" in response to suggestions, rather than being "made to happen" by the subject in order to comply with the hypnotist's suggestions. (For evidence that strong imaginal [cognitive-perceptual] involvement in suggested effects favors involuntariness see Spanos, 1971; Spanos & Barber, 1972; Spanos, Rivers, & Ross, 1977; Spanos, Spillane, & McPeake, 1976; but see Spanos & McPeake, 1977 for a failure at conceptual replication which, however, probably had an adequate explanation. There is also considerable further evidence to this effect that will not be cited here.)

For hypnosis-ESP (or -PK) work, there may be a distinct advantage in utilizing subjects who have the cognitive capacities to make experientially real to themselves the things that the hypnotist suggests in the study (subjective response) and who can experience (or interpret) those suggested events as involuntary (involuntariness of response. The experience or attribution (interpretation) of involuntariness (combined, of course, with a sense of subjective reality of effects) may be associated with a feeling that one is in a special state, under the hypnotist's benevolent aegis, in which things tend to happen just because they are suggested. If so, this almost magical attributional framework might favor psi events. The rationale for this hypothesis is as follows.

Elsewhere, I have theorized and provided evidence to support the claim that freedom from egocentric involvement with the task favors psi events (Stanford, 1974, 1977a, 1977b), and work on spontaneity and ESP-task performance suggests, at least indirectly, that freedom from the need to manage one's responses on an ESP task allows the spontaneity that favors psi-mediated response. (See Stanford, 1975, 1987a for relevant reviews; also, Stanford, Kass, & Cutler, 1988b, for a recent study of this effect in ganzfeld.) Consequently, the use of subjects in hypnosis-psi work who can develop a solid sense of involuntariness might be important. It might mean, in essence, that they adopt a more passive, less egocentric, less managing, more "let-it-happen" attitude toward the task. The result could be a heightened spontaneity that favors ESP-task success. This is an idea well worth examining, though, to my knowledge, it has not so far been examined in the hypnosis-ESP literature.

The use of scales such as the SHSS: A and B is for prior selection or evaluation of general hypnotic ability. The SHSS:C is appropriate for a more advanced level of screening. The still more advanced scales, namely the Stanford Profile Scales of Hypnotic Susceptibility: Forms I and II (SPSHS: I and II), can be used to identify subjects with par-

ticular hypnotic skills (or even patterns of those skills) that might be relevant to one's research. If, for example, one wished to use positive hallucinations as a mediating vehicle for extrasensory information, one might wish to use the SPSHS with special emphasis on its subscale HP (Hallucinations: positive). The SPSHS includes subscales related to (a) agnosia and cognitive distortions; (b) positive hallucinations; (c) negative hallucinations; (d) dreaming and re-experiencing past events; (e) response to posthypnotic suggestions (and evaluations of amnesia concerning such suggestions); and (f) various motor items (including, but not limited to, items involving challenge to start motion inhibited by suggestion) (Weitzenhoffer & Hilgard, 1967). Selection of subjects with high scores on any advanced scale (SHSS:C or SPSHS:I or II) could reasonably be expected to provide individuals who would experience involuntariness. The same might be said with less certainty for scales such as SHSS:A and B. However, none of these scales, of itself, measures involuntariness.

The Carleton University Responsiveness to Suggestion Scale (CURSS, pronounced, believe it or not, "curse") does not have this drawback. It is a standard scale that provides seven suggestibility items (tests). There are two motor items (arm levitation and arms moving apart), two motoric challenge items (arm rigidity and arm immobility), and three cognitive items (auditory hallucination, visual hallucination, and amnesia). Based upon these items, three types of suggestibility scores are generated for each subject. There is an objective score (CURSS:O) based upon the total number of items for which behavioral response occurred that met the specified criterion. The subjective score (CURSS:S) is based upon having the subject rate on a scale of from 0 to 3 how much he or she experienced the thing suggested by each item; then these ratings are summed across items. The objective-involuntariness score (CURSS:OI) is derived by assigning for behaviorally passed items one point for each such item for which the subject also rated involuntariness in the upper half (moderate or great involuntariness) of the 4-point involuntariness scale. (For details, including normative data and various psychometric properties, see Spanos, Radtke, Hodgins, Stam, & Bertrand, 1983.) CURSS:O scores have a bell-shaped distribution, but CURSS:OI scores are quite positively skewed (more or less like a backward J). Sometimes the CURSS is scored for voluntary cooperation (CURSS:VC), in which case a point is assigned for each behaviorally passed item on which the subject reports involuntariness in the lower half of the 4-point scale. CURSS:VC scores reflect the number of scores passed for which the subject felt the response was relatively voluntary; such scores are not suggestibility scores. (For details

see Spanos, Radtke, Hodgins, Bertrand, Stam, & Dubreuil, 1983; Spanos, Radtke, Hodgins, Bertrand, Stam, & Moretti, 1983.)

The CURSS is usually administered following a standard 5-minute hypnotic-induction ritual, but it can easily be administered as a suggestibility test (i.e., no induction prior to the suggestions) because the wording of its items does not imply anything about hypnosis or having been hypnotized. (Of course, some introduction is needed to explain to the subject something about what is happening.) Scores on all three CURSS dimensions (O, S, and OI) have reasonable temporal stability, correlate moderately with other standard scales (such as SHSS:C), and correlate with the same psychological predictors as other standard scales (e.g., with Tellegen's Absorption Scale, with measures of expectancy and attitude toward hypnosis, and with Field's "hypnotic experiences" inventory). The CURSS has often been administered as a group test with subjects scoring themselves on the objective dimension, as well as rating both subjective experience and involuntariness. (The exception here is that in objective scoring an experimenter scores the amnesia item, which requires reversibility of suggestion-induced "amnesia" for passing.)

With group administration, as in the report on psychometric properties of the test, CURSS:O yields, following varimax rotation, a two-factor structure (built largely around nonchallenge and challenge items), but a one-factor structure holds for CURSS:S and CURSS:OI. Spanos and colleagues accordingly suggest that there is a single, underlying subjective dimension in such measures. They further state that their results with the three types of CURSS scores (O, S, and OI) suggest that the bell-shaped distributions of hypnotic susceptibility found with scales such as the Stanford scales may result from a lumping of responses experienced as voluntary with those experienced as involuntary (Spanos, Radtke, Hodgins, Stam, & Bertrand, 1983). Weitzenhoffer (1980), one of the authors of the Stanford Scales, has also criticized those scales on the ground that they fail to consider subjective response to suggestions, such as involuntariness, and thus that they do not measure in any pure form the essence of the "classic suggestion effect."

However, Hilgard (1981), also an author of such scales, has argued to the contrary and has discussed relevant evidence, as has Kihlstrom (1985). Kihlstrom suggests that the Stanford scales somehow tap into the classic suggestion effect (which involves involuntariness), whereas, he tends to think, the CURSS might tend to elicit mere compliance from many subjects. He cites evidence that he feels support such charges, but I suggest consulting that evidence for oneself (and other

related evidence). Kihlstrom (1985) correctly notes that the CURSS tends to emphasize readiness to cooperate, which could add a stronger element of pure compliance than is found in some other scales. However, I personally know of no clear evidence that the CURSS is meaningfully different relative to compliance than the various Stanford scales. Some additional work is needed to adequately assess such a charge.

For investigators interested in the study of psi performance under hypnosis, the CURSS might, however, have some drawbacks as a hypnotic susceptibility test if its standard, five-minute induction is employed. Pardon the pun, but this sounds like a rather cursory induction, given the lengths of standard inductions in other tests.

I also have another reservation about the CURSS, namely, its inclusion of an item involving the suggestion of a hallucinated kitten in the subject's lap that he or she is to pet. Such an item might prove harmful to any subject who is both highly suggestible and phobic concerning cats. Please do not laugh! There are persons with irrational fears of cats, and there is considerable evidence in the literature to suggest that phobic individuals tend to have relatively high levels of hypnotic susceptibility or that there are commonalities between the phobic and trance-like experiences (Frankel, 1974; Frankel, 1976; Frankel & Orne, 1976; Gerschman, Burrows, Reade, & Foenander, 1979; John, Hollander, & Perry, 1983; Kelly, 1984; and additional work referenced in these sources).

Those wishing to consider other measures of hypnotic susceptibility than those discussed above might wish to read the recent *Annual Review of Psychology* chapter on hypnosis by Kihlstrom (1985) and study reports cited therein in his section on the assessment of hypnotizability, especially if they are contemplating the use of instruments such as the Creative Imagination Scale (CIS, Wilson & Barber, 1978) or the Hypnotic Induction Profile (Spiegel, 1977), but desire an instrument that will reliably accomplish the task achieved very well by the Stanford scales (especially SHSS:C). Not every measure that has made the pages of the hypnosis literature should be considered as of equal value for the study of general hypnotic susceptibility. The CIS, for example, does not show the psychometric properties of the HGSHS:A, and does not correlate well with it. It appears to access imaginal abilities specifically, rather than the more complex domain of hypnotic susceptibility (McConkey, Sheehan, & White, 1979).

My own opinion is that the Barber Suggestibility Scale (BSS, Barber, 1969), which has sometimes been used to advantage in parapsychological research (Honorton, 1972; Palmer & Lieberman, 1976; Stanford,

1971), has some distinct drawbacks. Perhaps the greatest is the strongly assertive or authoritarian tone of its suggestions. I worry that some subjects who are potentially responsive to suggestions or who are hypnotically susceptible might respond poorly to this scale because its assertive wording causes psychological reactance (resistance). Robert Pancza (1983) whose dissertation work was done under my mentorship, found in his work that the trait of absorption—which has often been found to predict hypnotizability—predicted suggestibility with a permissively worded suggestibility scale, but not to a significant degree with one that was worded very assertively or in authoritarian fashion. This suggests that if parapsychologists are interested in developing hypnotizability measurements that are mediated, at least in part, by imaginative involvement—as is the case with most hypnotic susceptibility scales—they might do well to stay away from the assertive BSS. This might be especially true when testing male subjects (at least with a male hypnotist or suggestor) because Pancza found that among male subjects the suggestibility-absorption correlation dropped to near zero when an assertive scale was used (albeit not the BSS). We do not know the explanation of this finding, but it was clear-cut. Possibly, many males find such assertive suggestions (with implications of extreme passivity and malleability) to be an affront to their traditional role as males.

A final drawback to the BSS is, in my judgment, an item that suggests that the subject's throat and jaw feel like they are clamped in a vise and that the throat is clamped so tightly the subject cannot speak! With extremely suggestible subjects there can be little doubt that the subject could experience considerable discomfort with such a suggestion. In one study I myself found that very effect with an extremely suggestible subject. After completion of that study I have never again used the BSS, at least in unmodified form.

In sum, it is likely that the most useful tool for preliminary *group* screening—and one that would link one's work with a large and growing body of psychological research—would be the HGSHS:A. Individual testing at the same, rudimentary level might best utilize the SHSS: A or B. More advanced screening (as in the search for genuinely outstanding subjects) or subject classification might best be accomplished through the SHSS:C, which is widely regarded as the state-of-the-art tool for anything above the level of rudimentary screening. Even the SHSS:C might profitably be supplemented by one or more measures of subjective response to the test items. For indications of responsiveness to a variety of high-level "cognitive" suggestions SPSHS:I or II should prove useful. The CURSS is certainly useful as a shorter test (7 items),



and it has the advantage of providing three types of suggestibility scores (O, S, and OI). However, it has some potential drawbacks mentioned above. If an investigator wishes to assess subjective response and involuntariness, as is done with the CURSS, there is no reason that post-hypnosis-session inquiries (modeled after those used in the CURSS) cannot be applied to the HGSHS and the various Stanford scales (see, e.g., Farthing, Brown, & Venturino, 1983).

Incidentally, there may be some reason to think that the probability of involuntariness (experienced or inferred by the subject) varies across types of suggestions. Farthing et al. (1983) found an unusually high proportion of subjects reporting that the posthypnotic suggestion was voluntary—a finding that concurs with the experience of Pancza (1983) in his dissertation, which involved work with another scale. Thus, in studying involuntariness-voluntariness, investigators might do well to examine any specific items of special interest to them, in addition to developing an overall index of this factor.

The parapsychological literature already contains indications that level of hypnotizability or suggestibility can be an important factor in subjects' ESP-task performance following a hypnotic-induction ritual (Honorton, 1972), and this topic needs further investigation, especially work using standard scales, and, ideally, one less authoritarian than the BSS. In the study of ESP in hypnotically induced dreams just cited, self-reported quality of hypnotically induced dreams was a successful predictor of hypnotic-dream ESP-task performance. It is worth noting that SPSHS:I and II each have an item concerned with hypnotic dreaming and, thus, might provide a preselection device for selection of promising subjects for hypnotically induced extrasensory dreams.

Parapsychologists' interest in finding good instruments for measuring hypnotic susceptibility might be heightened through awareness that Graham and Evans (1977) found, as they had expected, significant correlations between hypnotic susceptibility and subjects' ability to generate a random sequence. In the case of the HGSHS:A, the correlation was  $-.35$ ; in the case of the SHSS:C, it was  $-.46$ . (In terms of the data in question, the negative correlations mean better randomizing ability among high-susceptibles.) Let us be clear here that these investigators were looking at the ability to produce random sequences. Such findings might have relevance to Stanford's suggestion (1975) that hypnosis might accomplish some of its psi-favorability by freeing susceptible subjects, at least, from some of the rational, patterning constraints that normally affect ESP-test-taking behavior. Sargent (1978) found that hypnotic-condition subjects did *not* show a significant ten-

dency to balance calls across symbols in a forced-choice ESP task, whereas waking-control subjects did. Sargent also mentioned (but did not report statistics for) other evidence that hypnotic-condition subjects behaved with less rational constraints than did waking-control subjects. Interestingly, in Sargent's work subjects were not randomly assigned to the hypnosis and control conditions; there was room for a self-selection factor that might well have favored high-susceptibles being tested under hypnosis. Thus, having been subjected to an induction procedure was potentially confounded with unknown individual differences that might have included hypnotic susceptibility, which is now known to be related to the ability to produce relatively "random" sequences.

*Nonhypnotic Measures of Cognitive Skills Important to Hypnosis.* Probably the most commonly studied trait predictor of hypnotic susceptibility has been psychological absorption. Absorption, as measured by the Absorption Scale (Tellegen & Atkinson, 1974), represents the capacity and inclination of an individual passively and more or less exclusively to engage attention in some object or experience, either external or imagined. A high-absorption individual might become so enrapt in reading a novel that he or she would not hear someone call. For such a person, listening to music might become a totally absorbing inner experience or watching a waterfall might be wholly captivating. There is a long tradition of work related to this concept in hypnosis research. Another way of stating this is that many highly hypnotizable people are individuals with a tremendous capacity for imaginative involvement in stances outside the hypnotic context. (For relevant reviews see Lynn & Rhue, 1986, 1988.) There is a very respectable body of literature reporting a positive correlation between hypnotic responsiveness—behavioral and subjective—and Absorption Scale scores, which measure the trait of absorption. The essential idea here is that high-absorption persons have the capacity to make real to themselves the things suggested by the hypnotist and to maintain this sense of reality without distraction such that the experience of hypnosis is an intensely subjective one that involves involuntariness.

Why the trait of absorption should be associated with a sense of involuntariness is at the heart of a long-standing conceptual debate within hypnosis research concerning whether hypnotized subjects are or are not in an altered or alternative state of consciousness (or, similarly, are dissociated in responding to suggestions) or whether imagining the things suggested by the hypnotist strongly implies that a suggested movement is passive and is happening to one, rather than being done

by one. The former position is that of the state (or special-process) theorists (e.g., K. Bowers and E. Hilgard); the latter, that of the social psychological theorists (e.g., N. Spanos). None of these parties has actively disputed the correlation between hypnotic susceptibility and Absorption Scale scores. Indeed, both camps have contributed evidence of the correlation, but they vehemently disagree about how such findings should be interpreted (see, e.g., Spanos, 1986).

The absorption construct has received considerable attention within parapsychology (for a review see Stanford, 1987a; see, also, Stanford, Kass, & Cutler, 1988a, 1988b), but there has been until recently (see below) little evidence that this cognitive variable, which is important to really successful hypnosis, constitutes a successful predictor variable for ESP-task performance. However, this internal-states-related variable has proven very useful in a recent psychometric effort to pinpoint time-locked verbal markers of entry into and, function within an internal attention state during ganzfeld (Stanford, Kass, & Cutler, 1988a). Presently, I am exploring possible relationships between these objective, verbal markers (identified through use of Absorption Scale scores) and ganzfeld ESP-task performance, first using data available from the study just cited and, second, through a new study currently getting underway.

Investigators who use the Absorption Scale in parapsychological work should bear in mind that there is considerable and mounting evidence that (a) women score higher on the Absorption Scale than do men (e.g., de Groot, Gwynn, & Spanos, 1988; Spanos, Brett, Menary, & Cross, 1987; Yanchar & Johnson, 1981), and (b) the correlation of hypnotizability and absorption (or similar measures) is sometimes reported to be greater for women than for men (de Groot, Gwynn, & Spanos, 1988; J. R. Hilgard, 1979; Spanos, Brett, Menary, & Cross, 1987). Although the psychological explanation of such gender-related findings is presently unclear and deserves study, such findings clearly indicate that it is not advisable to examine Absorption or related measures as predictors without either separating the sexes or statistically taking into account such differences between them. Failure to do so could favor some very misleading conclusions. This circumstance has not, to date, received recognition in parapsychology (or in much of the psychological research on hypnosis and suggestibility). I have just (begun to reanalyze the data from our recent ganzfeld study (Stanford, Kass, & Cutler, 1988a, 1988b) in light of such considerations. Preliminary indications from that work are that gender may be an important factor and that failure to consider it obscured some interesting findings. It is conceivable that failure to consider gender is one reason efforts

to use Absorption Scale scores to predict ESP-task performance have been largely unsuccessful. (In a related vein, parapsychologists should perhaps also take into consideration that women tend to score higher on scales measuring belief in the paranormal; for discussion of some such evidence, see de Groot, Gwynn, & Spanos, 1988. Efforts to use such variables, as predictors of ESP-task success that do not consider the gender factor could conceivably produce misleading results.)

Despite a very respectable body of literature reporting a positive correlation between hypnotic responsiveness—behavioral and subjective—and Absorption Scale scores, a serious challenge has recently been lodged against the traditional interpretation of that correlation.

The claim has been made (Council, Kirsch, & Hafner, 1986) that, "Administering the Absorption Scale to hypnotic subjects may implicitly suggest that imaginative processes are important in hypnosis, which in turn could influence levels of expectancy for hypnotic responding" (p. 188). The Absorption scale has usually—but not always—been administered in a setting in which its link with the hypnosis study was transparent and, additionally, the scale has usually been given "unbuffered" (my term, not that of Council et al., 1986). That is, its items have not been embedded among items unrelated to the construct. The latter fact is perhaps even more troubling because all the items of the scale are keyed in the same affirmative fashion. A "yes" answer always contributes positively to the Absorption score. Under these circumstances, it is reasonable, following Council et al. (1986), to suppose that persons respond to the Absorption Scale, infer a connection between its content—intense inner focus of experience, including imaginal absorption—and hypnosis, and, then, infer something about their own likely level of response to hypnosis on the basis of such information. Then those expectations might influence their actual level of performance. Such are the claims of Council and colleagues. They (Council et al, 1986) report data, including expectancy measurements, that they claim support such an interpretation—at least when they are considered in light of earlier findings from other investigations that they review.

These methodological flaws are potentially serious ones and we can only be grateful to Council and colleagues for bringing these problems to the attention of the research community. Of course, such problems need not imply that any artifactual contribution to the Absorption-hypnotizability correlation is mediated by or only by expectancy. Demand characteristics and related compliance might play a role in the Absorption-hypnotizability correlation when the Absorption Scale is

given unbuffered in the hypnotic context. Another possible factor is that taking the Absorption Scale in circumstances in which its relationship to hypnotizability is transparent may favor higher hypnotizability scores among high-Absorption people because it provides cues as to personal inclinations and skills that can actually be used to advantage in the responding to the test suggestions that will subsequently be given. Let us term this the hypothesis of skill-relevant education. Demand characteristics and skill-relevant education could play roles over and beyond the possibility that such procedural errors have effects mediated by expectancy. Indeed, it is even possible that expectancy effects are themselves mediated by demand characteristics.

What I regard as the first clear-cut evidence against the claim that the Absorption-hypnotizability (or Absorption-suggestibility) correlation is spurious for any of the several reasons considered above came from our own laboratory at St. John's University in the dissertation research of Robert Pancza (1983), which I supervised. This evidence emerged prior to the publication of the Council et al. (1986) report. Pancza found the expected correlation of Absorption and suggestibility when (a) the items of the Absorption Scale were embedded among a majority of items measuring other types of traits (in accord with a recommendation from the author of the Absorption Scale, A. Tellenge), thus making the Absorption items less salient, and when (b) the Absorption Scale was administered in another experimental context by another experimenter, usually days or even weeks prior to the measurement of suggestibility, thus presumably eliminating any of the possible context-related artifacts.

The validity of the Absorption-hypnotizability correlation has been further supported by more recent work by Kihlstrom, Hoyt, Nadon, and Register (1987), work that used a different set of operations than those of Pancza (1983) and which therefore strengthens the conclusion that the debated correlation is valid, not artifactual. It now seems clear that the correlation is a valid one (although this does not mean that artifacts related to demand characteristics and/or expectancies would not occur under suitable circumstances). Parapsychologists should, therefore, not feel intimidated about using the Absorption Scale in their research, at least if they follow the general precautions outlined above in connection with Pancza's dissertation research (1983). Even if it should not prove possible to administer the Absorption Scale in a separate context from the dependent variable it is intended to predict, it is easy enough to administer it in a buffered form (i.e., with additional, non-absorption items to disguise the intent as much as possible). That is a nonoptimal solution, but it is better than nothing.

Psychometric work on the Absorption scale has continued, and there is now some evidence that for certain purposes its items might fruitfully be divided into those concerned with absorption in inner events and those concerned with absorption in external events (Balthazard & Woody, 1987). These investigators found that although there is no clean two-factor structure to the Absorption Scale and the items seem to form a continuum, the continuum can be thought of as having two ends, one constituted by absorption in inner events and the other by absorption in objective or outer events. In their work, absorption in outer events did not make a meaningful contribution to the prediction of hypnotizability beyond that contributed by absorption in inner events, yet absorption in inner events did make a meaningful contribution to prediction of such events beyond the contribution from absorption in outer events. This development, if it is reliable, might contribute considerably to refining the use of the Absorption Scale as a predictor in settings such as ganzfeld in which imagination is a very important factor. I would note, however, that Balthazard and Woody used a lengthier version of the Absorption Scale than have most investigators, myself included. I am currently seeking details on the above developments, including the expanded scale. Presently I have available only the abstract cited above.

In summary, the Absorption Scale is still of considerable interest to parapsychologists, despite several failures in efforts to use it as a predictor of ESP-task performance. Such failures could derive from irrelevance of absorption to ESP-task performance or they could derive from problems in the studies. In my recent review I discussed several possible reasons for failures, including unreliability of ESP-task scores and efforts to find the correlation in settings in which subjects are given a rather active operant-type set, rather than the passive one in which the trait of absorption is supposed to have relevance. In this commentary I have pointed to an additional possible reason for such failure, namely researchers' not having separated the data of males and females for analysis even when gender is known to affect both Absorption Scale scores and the likelihood of that scale correlating with hypnotizability. The predictive power of the scale might be enhanced by use of items referring to absorption in internal, rather than external events. Certainly, further work with the Absorption Scale (or selected items from it) is warranted because of its centrality as a trait relevant to internal attention states and because it has already proven useful as a trait measure that can help to identify time-locked verbal indicators of entry into and function within an internal attention state during ganzfeld (Stanford, Kass, & Cutler, 1988a). There are even preliminary indi-

cations in my own work mentioned above—which need confirmation—that the Absorption Scale does have predictive value for ganzfeld ESP-task performance, but that such findings might sometimes have been obscured by failures to recognize gender differences in both Absorption-Scale scores and in the capacity of such scores to predict external criteria (such as hypnotic susceptibility).

There are other measures of cognitive styles—probably quite highly related to absorption—that hypnosis researchers have sometimes examined as predictors. One of the most commonly used is the Inventory of Childhood Memories and Imaginings (ICMI), a test developed by Wilson and Barber (1981) and that has proven very useful (reviewed by Lynn & Rhue, 1988). The test is supposed to measure fantasy proneness. It has received some attention in parapsychological journals. For example, the ICMIC (children's form of the ICMI) correlates at a relatively low level (with undergraduates as subjects) with OBE reporting (Myers, Austrin, Grisso, & Nickeson, 1983) and, more generally, with the reporting of ostensible psi 'experiences' (Council, Greyson, Huff & Swett, 1986). However, such measures of fantasy proneness are neither conceptually nor empirically highly distinguishable from measures of absorption; correlations of greater than .70 have been reported (Lynn & Rhue, 1988). Council, Greyson, Huff, & Swett, 1986 reported one of .68.

*State Reports.* Tart (1980) also advocated studies of hypnotic depth using probes during the actual hypnotic session as the basis of making inferences about depth. Elsewhere, Tart (1972) had discussed in detail the use of such scales. As he recognizes (1972), the use of such scales requires certain theoretical and methodological assumptions. (Although he does not mention it, some of these are controversial in the field of hypnosis and others are doubtful on grounds of psychological scaling principles.) These assumptions include that there exists one or more dimension(s) of profundity of hypnosis along which subjects may change during a session (as when the induction procedure deepens hypnosis successfully). One must also assume, as Tart notes, that positions along this dimension have either experiential correlates or can be tapped through unconsciously mediated responses (in giving scale reports). Tart also notes that the depth dimension must have shared commonality across subjects in order for the scale reports to be useful across subjects.

In my view, there are two very important assumptions that Tart does not mention at all. Even if all the above assumptions were true—which I seriously doubt—we would have to meet two very important scaling assumptions, namely that (a) the scales are used in the same way by all

subjects and (b) by each subject in the same way across time. These assumptions would, at least, have to be met in order to do the types of group, process-oriented work that most of us desire to do in parapsychology. Additionally, there are other serious considerations to be discussed below that militate strongly against the use of such scales in research or, minimally, against making particular types of process-oriented conclusions on the basis of the use of such scales.

I will take as a case in point to illustrate the presence of several of these difficulties the Long Stanford Scale (LSS) (Tart, 1970, 1972), a scale that has received some use in the literature even very recently (e.g., Council, Kirsch, & Hafner, 1986 who used a modified version of the LSS). As used by Tart (1970), the LSS has subjects rate their depth of hypnosis by using a scale from 0 to 10. The lower anchor on the scale in this case referred to being in a normal state of consciousness, defined for the subject as being awake and alert. [Note the interesting and extremely controversial assumption here that being hypnotized means definitely not being awake and alert. Note that this is an odd "dimension," since it begins with awake alertness, which raises the question of whether the dimension should end in obliviousness (which hypnosis researchers would surely reject). I suggest that "awake" is a bad place to start for a scale of profundity of hypnosis since it is built around the myth, rejected by everyone, that hypnosis is sleep-like. Note also the assumption that the normal state of consciousness is awake and alert for everyone (or, if it is not, that they are walking around partially hypnotized?). What about individuals who suffer from excessive daytime sleepiness (EDS) for reasons such as narcolepsy? By conservative estimate, EDS afflicts somewhere between 1 and 4% of the general population, according to Anch, Browman, Mitler, & Walsh (1988) and increases considerably in likelihood with advancing age, due in part to respiratory problems that impair sleep. Interested readers can examine for themselves the full text of this scale (see, e.g., Tart, 1972, p. 256-257) to see how ill-defined are descriptors for the intermediate portions of the 11-point scale. The descriptor for "10" just indicates that subjects are deeply hypnotized and are, essentially, ready to do anything suggested to them. The hypnotized = compliance statement would seem virtually to insure that anyone giving the "10" rating (or, actually, any high rating) before any particular suggestion would try hard to succeed on that subsequent suggestion. It also has the very undesirable consequence that it biases the whole test toward finding a correlation, not only between the number of previously passed suggestions and self-rated depth, but also between an immediately antecedent rating and the likelihood of passing the subsequent behavioral test. Indeed, since



the entire scale is defined ahead of time for the subjects, it would seem to imply to subjects (among several other things) that the higher the rating you give of your depth, the more items you should pass. Tart (1972, p. 457) also indicates that all subjects were told that if they feel more deeply hypnotized, they should expect to experience more hypnotic phenomena (read "pass more test suggestions"). Such circumstances seem, truly, an arrangement almost guaranteed to produce (spurious) evidence of the construct validity of the depth scale—that is, by causing it to correlate artifactually with behavioral measures of hypnotic susceptibility.

Is it, then, very surprising that Tart (1970) should have found that across subjects the average state report that preceded passed items was greater than that for items not passed? If subjects report good depth just before a suggestion, they might appear to have been dissimulating in giving that report if they did not subsequently give concordant behavioral responses. Also, if they reported poor depth prior to a test item, responding to that item might itself appear to be faking. Such reactions on the part of the subject are definitely to be expected, given the way in which the depth scale instructions fundamentally link depth and behavioral response and given subjects' well known desire to be both self-consistent and anxious to manage impressions of themselves (not even to mention the possible pressures associated with the demand characteristics here). (The problem would be there even if the definition were not explicit, since it becomes implicit when inductions speak of "going deeper" and, after depth has been ostensibly achieved, test suggestions are introduced.) Of course, Tart also found that passing of items *prior to* depth measurement predicts depth reports in the way one would expect under either his theoretical orientation or a social-psychological (attributional) one.

The modified LSS scale used by Council, Kirsch, and Hafner (1986) reads as follows: "One means you're not hypnotized at all, 2 indicates a light trance, 3 means a medium trance, 4 is deep, and 5 is very deep" (p. 183).

When the LSS is thus modified, as used by Council et al. (1986), to a 5 point scale, there are potential problems of a somewhat different kind because of confusion about how the dimension to be rated (depth of hypnosis) is actually defined and a lack of anchor(s) for the scale. Here the lower end is potentially somewhat anchored (by the possible feeling "nothing unusual has happened to me as a result of the induction"), but the upper end is not. Here a lack of any clear guidelines is an open invitation both to subject differences in how the scale is used and within-subject changes during the session in how to use it. Problems

with using scales such as these are that they (a) provide the subject with no meaningful, clear definition of the dimension itself and (b) no way to anchor their responses relative to any dimension that they might somehow discover and, thus, no way to assign numbers consistently and systematically. This is an undesirable circumstance, psychometrically speaking.

It should be made clear, however, to the credit of the authors of this important, ground-breaking study (of which this particular scale is only one item of interest) that the instructions given to subjects prior to hypnotization did provide indications about what being hypnotized might be like. (Dr. Council has kindly provided at my request a copy of the instructions preceding the hypnotic induction.) Such information is usually given prior to an induction because it helps reduce possible fear and apprehension concerning the unknown experience. The provision of such information should make it easier and more meaningful for subjects to make judgments about whether they are hypnotized or about depth of hypnosis (defined in terms explicit and implicit in the instructions given them prior to hypnotization), and it might reduce inter-subject variability in how the scale is used. It cannot, of course, guarantee that the scale is measuring anything like true hypnotic depth (if that is a meaningful construct)—and I think that Council and his colleagues might agree with me here. Very important, the provision of such prior information cannot provide any clear or easy basis for subjects' making one-dimensional 5-point-scale judgments about their depth of hypnosis. At some level this must remain a complex, difficult and uncertain task, with possible spin-offs to be discussed below.

Let us consider these matters very generally now, without reference to any particular study. With such scales subjects are required to make judgments about an extraordinarily vague idea, hypnotic depth, about which psychologists themselves argue vigorously regarding its degree of conceptual appropriateness for what actually happens during hypnosis. It is this requirement of making ratings of depth of hypnosis in the face of exceedingly vague criteria that poses additional problems. When subjects must make judgments in the face of uncertainty, they are motivated to use all the information available to them.

I have already indicated the likelihood of their comparing their experiences with information given prior to the session in order to fit the match, somehow, into their judgments. What is potentially very important here is that we are really asking subjects to draw conclusions, make inferences or attributions, about the causes of their behavior and experiences in the situation. They are in a sense being asked to decide in what degree their behavior and experience are attributable to being

hypnotized. They presumably reflect upon these matters (consciously and/or unconsciously) and report a judgment. But it is very important to realize that this judgment is something that *comes into existence* because of a need to understand what is happening in order to respond to the query. It is a potentially very central attribution in this setting, and the outcome of this attributional analysis (be it done quickly and intuitively or more analytically and thoughtfully) will, probably, have a decided influence upon subsequent behavior and interpretations of the setting.

Let us clearly understand the potential functional and theoretical significance of this, if it occurs. Hypnotic investigators of the state-theory persuasion (i.e., who believe that hypnosis involves an altered or alternative state) opine that as subjects pass test suggestions this "deepens" the hypnosis. However, this claim is troubling and puzzling because of its vagueness. How or in what sense can state be changed by observing oneself pass a test suggestion? Perhaps state theorists can pose a clear answer to this question, but I have never seen one. My own inclination is to accept some variant of the social psychological view that passing a suggestion behaviorally *in the presence of a sense that the response occurred involuntarily* provides potential feedback to the subject that says, "I am hypnotized." (Social psychological theorists have explanations of how response in such settings can seem involuntary, e.g., Spanos, 1986, or Angelini & Stanford, 1987, the latter a view based upon cognitive-attentional factors.) This is because part of the socially agreed upon meaning of hypnosis concerns perceived involuntariness of response or perceived automatic occurrence of the things suggested by the hypnotist. Once the subject concludes that there is evidence that he or she is hypnotized, subsequent processing of the situation is different, less critical, and is focused upon accepting the things said by the hypnotist in a relatively literal way and a readiness to experience them as such.

If this is true of observing oneself respond to items on a hypnotic susceptibility scale, it might also be true of clear, direct attributions made in response to queries about one's state as things progress. (Here some such process seems, indeed, relatively certain because the investigator forces one to draw a conclusion about what is happening, or, minimally, to act as if one has. In merely responding to behavioral tests in hypnosis, such attribution may not be as actively favored, though it would often occur spontaneously.) In short, giving state reports might well represent, not a passive testing of something that already exists, namely depth, but, rather, a kind of intervention that can either facilitate or perhaps interfere with the total process. If state reports go

"well" (i.e., indicate increasing depth), the process tends to feed on itself, further facilitating the entire process. This is because state reports represent an active judgment and because a series of them indicating deep or deepening hypnosis may tend—like a series of passed suggestions—to convince the subject that he or she must really be hypnotized.

Interestingly, making such judgments tends to remove the subject's attention from the experimenter as a source of influence upon behavior (i.e., as a source of pressure for compliance) and refocuses it upon the subject (as, for example, a hypnotized subject experiencing the suggestions). The question, after all, is "How deep are you?" not, "To what degree were you influenced by a perception that I wanted you to respond a certain way?" Consequently, after making state reports indicating hypnosis as being successful, the subject may tend to forget about the hypnotist as a source of social pressure for behavioral compliance and may simply make the judgment that his or her state reports, like behavioral responses to suggestions, must have been influenced by inner observations.

A general consequence of the above is that giving a state report is reactive; it is virtually certain to influence what follows.

It is interesting that Tart (1970, 1972) consistently found a very substantial, but nonsignificant ( $p = .11$ ), tendency for the correlation between state-report and behavioral response to hypnotic suggestions to be higher for experimenter-requested "instant" (or "automatic") state reports than for experimenter-requested "deliberate" reports in which subjects are asked to make a conscious estimate of the best rating. A slight, but real, effect (perhaps significant with a more substantial sample) is what one would expect if subjects are forced by "instantaneous" instructions to make quick judgments about depth. Why? In such a case we would expect subjects to rely upon the most salient and accessible information as the basis of making their judgment since this would speed up response (as demanded by the instructions). It seems quite obvious that one's behavioral response to previous items is the information likely to be most salient and accessible in such a setting. Experiments outside the hypnosis area—for example in word association (Horton, Marlowe, & Crown, 1963)—have demonstrated that time pressure increases the use of accessible information.

In another sense, too, giving a state report is an active process by the subject that is likely to influence what subsequently happens. It represents a public commitment (i.e., one made openly to the hypnotist) that "This rating is how hypnotized I am." Even if the subject, due to subtle social pressures from the hypnotist, has given a state report better than what actually appeared to be the case (to the subject), he or she

is likely to come to believe that state report because of the need for reducing cognitive dissonance engendered by such reporting (Festinger, 1957).

In a different vein, if one gives a state report, there ensues a real sense of pressure to act in accord with that report. To do otherwise would be to suggest dissimulation. In other words, state reports force the subject into a situation that requires impression management or self-presentational strategies. (For reviews and conceptualization of impression management or self-presentational strategies, see Baumeister, 1982; Schlenker, 1980; Tedeschi, 1981.) Self-presentational actions—even dishonest ones—might in such a situation lead to self-persuasion through more than one channel.

In summary, the use of state reports in internal states research is fraught with difficulties. (And we have not even discussed here the potentially ready influence upon such reports of demand characteristics.) It suffers from very great psychometric problems including, but not limited to, the differential use of such scales across subjects and within a given session. The state-report approach is definitely reactive in character and thus has serious shortcomings relative to internal validity.

Possibly one response to this from some persons knowledgeable about the parapsychological literature would be that a considerable number of studies have found state reports, including state-report shifts, to be predictors of ESP-task performance. (See Palmer, 1978 for a review.) Let us assume, as I am inclined to, that this is a true, valid pattern of findings. Its interpretation is what is considerably problematic. At face value it seems to suggest either that a more altered state or a greater change in alteration of state as a consequence of session procedures favors ESP-task scores that differ from mean chance expectation (albeit, according to Palmer's review, not necessarily or always in the psi-hitting direction).

The discussion above about the problematic character of state reports, although developed in the context of hypnosis work, applies equally well to ESP testing in a number of settings that favor altered or internal attention states. It suggests alternative, and perhaps equally interesting, scenarios as interpretations of the covariation of state reports (or within-session shifts in state report) and ESP-task performance. Various scenarios could be developed, but I will discuss only one here as an illustration of a plausible alternative. If subjects are influenced by making state reports (and reflecting upon the information that is used in making them) in the ways suggested above, it is clear that a subject giving a state report indicating that consciousness has been

properly altered would, in settings such as typical altered-states-ESP studies, also come to believe that he or she is in a suitable setting for extrasensory things to happen. (Why, after all, the elaborate manipulation—ganzfeld or hypnosis—and why the state reports? Experimenter expectancies in such a setting are transparent.) If the experimenter expects things to start happening and he or she is an expert—as surely he or she is—then the subject will come to expect that they will happen. This automatically results in the kind of freedom from egocentric striving, combined with expectant, passively interested, intrigued watching that may provide an excellent circumstance for ESP-task success. The subject can, figuratively and even literally, lean back, relax, and watch the psi start to roll in. Scientifically interpreting this scenario, the use of state reports has served as a “clever” manipulation, one that forces the subject to develop his or her own conclusions about state, and these conclusions in turn influence psi-favorable task attitudes and expectancies.

In short, when the ESP-task is set in an altered-states-favoring environment and state reports are elicited, these are not merely measurements. They are active manipulations or interventions that can influence the subject's performance in a way that would not have occurred in their absence. For ESP work, state reports could well be intrinsically reactive methodology, an approach that has sufficient methodological ambiguity to seriously temper any conclusions drawn from such methods—at least in the absence of ancillary work intended to resolve ambiguities introduced by them.

Taking a step further back and looking at this a bit more philosophically, it is easy for the investigator to believe that the subject can be passively measured in all kinds of ways relative to altered states and that through such methods one is simply discovering “the truth.” It is easy even to believe that there are almost no limits to what one can explore in these wonderful, direct, introspective ways. One can find out about all kinds of wonderful and esoteric things like shifts in consciousness as they occur throughout the session. A kind of scientific imperialism can develop in which one fatuously believes that more or less the whole world of the mind is lying there, passively waiting to be measured. This dangerous delusion that one can very directly and meaningfully measure the mind without compromising its integrity gains a heady sway. Observation without interference is a laudable goal of science, but sometimes it is more apparent than real. The fact is that eliciting state reports—as many measurements in altered states settings—has an active character that inevitably affects subsequent events. Accurately interpreting research patterns—however reliable

or replicable—obtained with these reactive methodologies requires great caution and, probably, supplementary work.

The use of potentially reactive measures in current hypnosis research does not end with elicitation of depth reports. It extends also to the very important study (see below) of the role of expectancies in response in the hypnotic setting. The Council, Kirsch, and Hafner (1986) study mentioned earlier is an example of a potentially reactive study with regard to subject statements about expectancies, as well as with regard to depth reports. In it, subjects were asked not only to rate their post-induction depth of hypnosis (as discussed above), but to predict prior to induction a number of allegedly unsuggested hypnotic effects and their responsiveness to particular suggestions that were very similar to those used on the dependent variable. They had again to predict responsiveness to specific suggestions following the induction but prior to administration of actual suggestions. The use of such methods here, as in a number of hypnosis studies by other investigators, seems to imply that we can measure pretty much what we want related to subjects' internal processes, but still not change anything. That seems doubtful. This particular study is far from unique among hypnosis studies in using methodologies that might be considerably reactive. (And, despite these potential problems, this paper makes exceptionally important contributions because of its examining possible consequences of methodological problems in earlier studies of absorption and susceptibility.)

In the event that the above remarks have been overinterpreted or misunderstood, let me voice a caveat. Nothing said earlier should be construed as indicating that subjects do not or cannot have internal access to cues that are somehow related to internal state or depth—assuming, for the moment, that those are useful constructs. It is, rather, to say that even if this is true, those cues are likely to be diverse and that such information is likely to be complex, confusing, and difficult to interpret on a simple, unidimensional scale. For this and other reasons, the use of depth scales poses many problems discussed above. The use of such instruments, if it is to be done at all, must proceed with the greatest caution and realism.

Nor should these remarks be seen as an indictment of introspectionism in all its forms. For example, I will discuss below what I regard as better examples of such methods. In general, introspectionism works best when what subjects are asked to report are, phenomenologically speaking, straightforward experiences that are meaningful to people and that appear as part of the common language. Subjects, for example, have realistic, meaningful insights about how physically relaxed they

are (Braud & Braud, 1974), as evidenced by correlations between "state reports" on relaxation and recordings of actual muscular tension. Asking for reports on hypnotic depth is definitely not an example of introspectionism in its classical and respectable form. It requires complex integration of and judgments concerning information, judgments that can only be based upon vague and ill-defined criteria. As stated earlier, it really, in essence, forces the subject to make interpretations or attributions about the cause of his or her behavior.

### *Measures Based Upon Post-Session Retrospection*

*Inventory of Hypnotic Depth.* This (Field, 1965) consists of 38 true-false items about allegedly unsuggested<sup>5</sup> cognitive and perceptual distortions or changes experienced during hypnosis. It is given at the end of the hypnosis session. The sum of items endorsed is supposed to indicate the degree of alteration of consciousness experienced during hypnosis. This instrument has found considerable use in the hypnosis literature. It has the special value that it is unobtrusive relative to the actual hypnosis session and is not reactive in the sense that in-session elicitation of depth reports is. (One could, however, ask questions about the psychometric wisdom of combining, by addition, the number of endorsements, as though this constituted a direct measure of a single dimension, depth.) Although this instrument does ask for introspections by subjects, it asks about particular possible kinds of experiences, not a judgment or attribution about the causes of one's behavior. Its content is quite face obvious and is potentially subject to many demand characteristics. Perhaps these can be reduced or obviated by appeals for forthright reporting. There is a sense in which this is not very different from the series of questions parapsychologists often ask subjects at the end of a ganzfeld session. Perhaps this instrument also warrants study for ways in which some of its ideas might be generalized to ganzfeld work, because it is obviously intended for hypnosis research. Parapsychologists might find it useful in hypnosis-psi studies.

This inventory has the advantages that it has been used in numerous published individual investigations, that its correlates are well known, that it is easy to use, and that it is unobtrusive and can probably be used in a nonreactive way, given that it occurs at the end of the session. However, one cannot be sure that answers to all its items represent

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<sup>5</sup> I say "allegedly unsuggested" because it seems to many hypnosis theorists with a social-psychological orientation that some of these effects are at least implicit in the suggestions typically included in a hypnotic induction or in test suggestions administered.



actual experiences during the session, for some may represent retrospective interpretations of those experiences. I feel, however, that it has potential use in parapsychological hypnosis studies and that it should receive some attention. I am not aware of any published review covering all the work with this instrument.

*Phenomenology of Consciousness Questionnaire.* Ronald J. Pekala and various co-investigators have developed an instrument that is intended to measure several alleged dimensions of subjective experience associated with altered-states induction procedures (like hypnosis or meditation). The questionnaire is intended to measure particular elements of such experience in a given setting. In a recent paper (Pekala, Wenger, & Levine, 1985) it is stated concerning the Phenomenology of Consciousness Questionnaire (PCQ) that: "It is a 60-item inventory<sup>6</sup> with each item consisting of two statements separated by a 7-point Likert scale. Thirty-seven of the PCQ items adequately assess nine dimensions of consciousness. These dimensions (and associated subdimensions) include altered experience (body image, time sense, perception, meaning), attention (direction, absorption), awareness (self-awareness, state of awareness), imagery (vividness, amount), volitional control, internal dialogue, positive affect, negative affect, and memory" (p. 127). Four items, as an average, compose each of these nine dimensions.

This instrument sounds, at least on the surface, as if it might be useful to parapsychologists using techniques like hypnosis or ganzfeld. While I have suggested elsewhere (Stanford, 1987a) that this instrument might have potential use for parapsychologists, I am not very favorably impressed with the research report cited above and with some other information that I have examined on this instrument. I would like to know much more about any evidence concerning the reliability and validity (including discriminant validity) of the supposed dimensions and subdimensions. Correlations used as the basis of conclusions are often startlingly small, but significance is achieved through large samples. Furthermore, Pekala and colleagues seem inclined to use as the target of subject retrospections (using the PCQ), very short periods (e.g., four minutes) of, for example, persons sitting with eyes opened and with eyes closed. The study of this instrument could, it would seem to me, benefit by using it with extended (greater than four-minute!) meditation periods with experienced meditators and with situations such as ganzfeld (along with trait predictor-variables like absorption,

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<sup>6</sup> I have in my possession contradictory information about the number of items in the PCQ. It appears to be in a state of flux due to revisions being made on it.

such as has already been done). This scale could, in my judgment, be more properly viewed as a valid and useful instrument if it showed meaningful results in such more realistic settings for extended periods. It has been used following hypnotic induction, apparently, with some success. Study of this instrument with factor analysis, rather than *a priori* judgments, would also, in my judgment, be useful, if that has not already been done—some of the research on this instrument has not been examined by me.

It is difficult to assess the promise in this overall, ambitious program because the pattern of significant results emerges from among many possible significant outcomes. Some findings that one would expect do not appear, whereas others that appear as significant are a bit surprising. For these and several other reasons that cannot be discussed here, I consider that this program is very interesting, but that the case has yet to be made for this being an instrument that would have potential payoff for investigating problems of interest to parapsychologists.

Certainly, this effort at mapping consciousness (as Pekala tends to call it) is fraught with many methodological problems and conundrums and has as a major difficulty the problem of very serious demand characteristics. I do not personally feel that questions related to demand characteristics have been adequately addressed in the research reports on this instrument with which I am familiar, despite some suggestions to the contrary.

This program does not have some of the problems associated with depth reporting in an ongoing session, but it surely has problems of its own. Perhaps with further testing and refinement Pekala and colleagues will have an instrument that will prove of value to parapsychological investigators. Or perhaps parapsychologists might wish to use the instrument now and attempt to aid in its investigation. If so, I urge great caution with regard to the problem of demand characteristics. Pekala's general approach, has, however, the very attractive feature that it seems to promise a considerably more rich analysis of the topography of internal states than has heretofore been offered. It is built, in large degree, around Tart's conceptual analysis of altered states (e.g., Tart, 1975), which is, to my way of thinking, a very thoughtful and potentially productive analysis that helps make altered states a manageable area for research and conceptualization. Another asset of the PCQ is that it is not limited in its application to a single altered-states-conducive setting.

It would be of interest to try this instrument with subjects at the end of a ganzfeld-ESP session in order to learn whether any of the dimensions predicts ESP-task performance. (Discovering the proper inter-

pretation of any such finding would, naturally, require additional research.)

Potential researchers with the PCQ should know that it is not, apparently, available for use gratis as a research tool. Information on the cost of the instrument, instruction sheets, various other materials and the user's manual can be had through writing to Pekala, whose address appears on his publications.

*A Special Methodology Associated With Hypnosis Research: The Real-Simulator Design.* This design was first discussed in the parapsychological literature, to the best of my knowledge, by Honorton and Krippner (1969). It is the creation of Martin T. Orne (1959, 1972). Without getting into details, it represents an effort to discover what are the consequences of any demand characteristics present in one's design. It involves careful identification, *a priori*, of two groups of subjects. One is highly susceptible to hypnosis and the other definitely is not, as determined by at least two standard tests. The former become the "reals" in the design, and the latter, the "simulators." In the real-simulator design there are two experimenters. The one who is the hypnotist and who takes the dependent measure is blind as to which subjects are "reals" and which are "simulators." For the simulators the nonblind experimenter provides instructions to the effect that they are to fake being hypnotized in the session with the hypnotist-experimenter, and they are told that although the latter will know that there are some simulators, he or she will not know who the simulators are. Simulators are further told that if the hypnotist-experimenter detects their simulation, the experiment will immediately be terminated. Simulators know that there are also reals (susceptible persons) who will be in the study. Reals do not have simulation instructions and do not even know of the existence of simulators in the study. Nor are they told that if the hypnotist-experimenter should ever think they are faking the experiment will be stopped.

Trance or state theorists have seemingly loved this design because a sizeable number of studies have shown differences in behavior of real and simulating subjects when they are given the same suggestions and are seemingly acting under the same demand characteristics. Such differences have often been taken in the literature as evidence that something nonartificial and real is happening with the real group, something that indicates a special characteristic associated with being "hypnotized" that allegedly does not occur due to the social psychological variables so much invoked by social constructionists in trying to explain the hypnotic domain. Readers should consult Orne's 1972 paper to learn his own views on the requirements of the design and the legitimate

conclusions that can be drawn from it (and to learn what he thinks are some illegitimate applications to which it might be put). He claims that the method is basically for the purpose of assessing what will be done by nonhypnotized subjects when they are motivated to do so by the experimental setting. He sees its uses as including evaluation of claims that hypnotized persons can do things they could not do if not hypnotized or will do things that they would not do if un hypnotized. He sees it as also useful when there is the possibility that subjects will see through experimental deception.

It is my personal judgment that the hypnosis literature is replete with examples of persons overinterpreting real-simulator differences found in studies. The method has its own special problems. These are discussed in some detail by Spanos (1986). The real-simulator design, as Spanos (1987) notes, neither controls for demand characteristics nor insures that they are the same in so-called real and simulating subjects. The latter would seem to be a requirement of the design if it were suitable for the objectives often alleged. In what follows, I make no claim that my analysis necessarily follows that of Spanos.

Investigators need to understand the many kinds of limitations in the real-simulator design. In the first place, it is confounded heavily with subject characteristics (Barber, 1969). (Orne knows that, but many investigators who write reports on their findings with this design seem to ignore it.) There can be no doubt that real and simulating subjects possess different cognitive inclinations and/or skills, precisely because several such differences have been identified between low- and high-susceptible subjects.

A study has recently appeared that illustrates this potential confounding and how investigators and readers sometimes ignore its potentially problematic implications (Nash, Johnson, & Tipton, 1979). These investigators were studying hypnotic age regression and had as dependent variables three measures of how regressed subjects, under hypnosis (reals) or simulating (low susceptibles), related to a transitional object. Significant differences were found on all three dependent variables. This, in the opinion of the investigators, showed an effect due to hypnosis.

Such a conclusion is, however, unjustified. Differences in cognitive skills and inclinations in high-as contrasted with low-susceptible subjects might account for the differences observed, as could other possible differences such as childhood experiences, that might affect hypnotizability. Would this pattern of results, for example, have been favored if the reals simply had much more vivid imaginations that allowed them to experience more clearly the situation suggested by the hypnotist?

Or what about the possibility that subjects who remember having had (actually had?) transitional objects tend to score better on susceptibility tests? It is to Nash's credit as a thoughtful and honest investigator and reporter that in a second paper on this general topic (Nash, Lynn, Stanley, Frauman, & Rhue, 1985) he does discuss (only) the latter possibility and considers evidence from other work that is relevant to it, but he did not seem to recognize this possibility at the time of writing the first report. Neither, apparently, did Kihlstrom (1985) in reviewing age regression work, for he indicates that this is the single study providing strong evidence of a reinstatement effect during hypnotic age regression. It is certain that the real-simulator design is inadequate here to sustain the kind of conclusion that many commentators would have liked to have had.

It is good to be able to report that Nash and colleagues in the second study cited above also point out that real-simulator differences are quite problematic of interpretation and that ancillary designs are needed for definitive conclusions. Regretfully, real-simulator designs have sometimes been misused and misinterpreted in the literature and, too often, with no such sage caveats forthcoming from their authors. As noted by Nash and colleagues in the later study (Nash et al. 1985), the real-simulator differences they observed might simply be due to engaging in simulation (as contrasted with passively being a hypnotic subject), rather than to the effects of hypnosis *per se*. It is rare to see such appropriate reserve in interpreting a set of new and exciting findings. It is also too infrequently found in the real-simulator work generally.

The latter remarks—concerning the task or processing demands of being a simulator—also bear comment. This is one of the major problems with the real-simulator design. The processing load for the simulator is heavy. There is a need to understand the suggestions given, to anticipate just what would be proper hypnotic response, and to enact such response. This is all done under the psychological pressure that if one is not good enough at one's performance, the show will abruptly be stopped. One will be caught in the act of faking and the show will not go on. The privileged reals, on the other hand, are not even told that if their performance is less than up to snuff, then the whole act will be canned! One wonders what might happen if they were told, "If the hypnotist should for any reason conclude that you are faking this, rather than really being hypnotized, the experiment will be terminated." Of course, it would be argued by some that this would surely interfere with hypnotic responding. A social-psychological theorist would point out that the strong public self-consciousness thus engendered might interfere with performance of any role, including that of

the simulator, who is actually exposed to such a threat. (Interestingly, and not surprisingly in light of the above discussion, simulators often overplay their role.)

Sarbin and Coe (1972), two leading theorists of hypnosis as role playing, note that since reals and simulators get decidedly different instructions (with different background information relative to those), they could be expected to play different roles. Much of the alleged evidence for a special role of hypnotization might thus be explained. In a closely related vein, Wagstaff (1981, pp. 107-109) does a masterful job of explaining how differing role construals mediating the compliance of reals and simulators provide a cogent alternative explanation of the results of a classical study of the alleged compulsion induced by posthypnotic suggestion (Orne, Sheehan, & Evans, 1968). Readers might wish to consult the details of this account for the sheer intellectual stimulation of seeing how a little imagination can make some fairly mysterious looking outcomes seem relatively prosaic.

There is also the problem that simulators know that they cannot "genuinely" perform on the hypnotic tasks, so they are already at a moral disadvantage. (On the other hand, reals are even protected from the knowledge that simulators are involved—on grounds that their feelings might be hurt by the thought that they are not trusted and that this could result in impairment of their hypnotization. Simulators, as noted earlier, know that reals are also involved.)

Martin Orne is well aware of the possibility that such factors might play a role in influencing real-simulator results, but investigators using the design have been less cautious in too many instances. Furthermore, any caveats put forth in discussion sections are likely to be ignored by many readers and, very often, even by those preparing literature surveys or reviews. Orne (1972) discusses possible alternatives to real-simulator designs and other approaches to controlling for the demand characteristics problem.

It is fair to say that, in sum, the real-simulator design has very great limitations, has been overused and may well have outlived its usefulness except for some very specialized types of problems. Spanos is surely right that it does not handle the demand characteristics problem suitably (Spanos, 1986). There is a need for additional work on finding alternative, less problematic designs. Parapsychologists might lend a helping hand here and thereby show that they can make important contributions to nonpsi areas of research. It is possible, as I will suggest below, that real advancement in determining the effects of demand characteristics could depend upon conceptual developments.

With such problems for the real-simulator design, I find it surprising

that it has had and continues to have such popularity. It seems to me potentially wasteful of investigators' time in many instances. Surely we can find ways of motivating our subjects without telling them that they cannot really do something but will have to fake it! T. X. Barber's original task-motivational instructions (e.g., Barber, 1969), which were intended to provide a control for the motivation imparted to hypnosis-condition subjects by the instructions and psychological setting, have been properly criticized (e.g., Bowers, 1967) because of their misleading subjects with regard to normative data on performance by past subjects (saying all of them could do the things suggested) and because they introduce some rather extreme strong-arm tactics to get subjects ready to go along with the suggestions (implying that one might be sub-normal if one did not accomplish the things suggested and that a lack of response would waste peoples' time). Various studies have demonstrated that persons respond to such strong-arm tactics by out-and-out compliance. However, we have in our laboratory (in nonpsi work) been using a task-motivational approach that does not attempt social arm-twisting or induce possible guilt about noncompliance. I would suggest that new approaches to motivation-relevant controls are needed in hypnosis and suggestibility research.

It is important in this context to note that parapsychologists may not face all of the same problems vis-à-vis demand characteristics as do investigators who use dependent variables (behavioral ones) that are under volitional control. In a psi experiment, even if the subject can guess the investigator's hypothesis thanks to demand characteristics of the study, this does not mean that he or she can or will therefore deliberately bias the results of the study. Although success at such deliberate biasing of psi outcomes is not inconceivable, it seems doubtful that most subjects would even try to shape their psi results to meet experimenter expectations. This is because most subjects almost certainly have no belief that they can deliberately manipulate their psi-task performance in such a way or would believe that they know how to go about it. They are busy enough just trying to have some extra-chance success.

If demand characteristics play a role in psi experiments, it is, therefore, likely that they do so, not through deliberate attempts at compliance, but through the mediation of situation-induced expectations. The cues that tip off subjects to what kind of performance the experimenter expects in the setting at hand can also lead them to expect that such performance will actually be forthcoming. This can happen if subjects accept such expectations as "expert" and correct opinion

and, as a consequence, have their psi-task performance shaped (through whatever means) by those cue-based expectations.

This is not an improbable scenario. The problem of what to think about one's ability to perform in the ESP-task setting is an ill-defined one for the subject, who is usually unfamiliar with such situations and what is likely to happen therein. Such ill-defined situations are where social comparison processes are likely to come vigorously into play (Festinger, 1950, 1954), precisely because we have a need to evaluate our own opinions and abilities. In such a case, said Festinger, persons evaluate their own opinions and abilities by comparison with the opinions and abilities of others, to the degree that more objective means of doing so are not available. In the present case, a subject presumably asks himself or herself, in effect, "What can I think about the likelihood of my doing well in this task? I have no past record upon which to base expectations. What information is present here that could tell me what to expect, how well I will likely perform? Oh, yes, the experimenter obviously thinks people will do well here. Otherwise, why would he go to the trouble of hypnotizing everyone? I suppose I will do well, especially since the experimenter obviously has some good reasons to feel that way." If the subject thinks this way, it might influence his or her psi-task performance.

If the above reasoning is valid, the parapsychologist's problem vis-à-vis demand characteristics is one of somehow controlling for any expectancy effects generated by them. This suggests that investigators need to be sensitive to the expectancy-related consequences of their manipulations. Such consequences are confounds when we intend to manipulate something other than expectancy. This circumstance may also suggest the need for manipulation checks relative to expectancy. William Braud has been one of the leaders in paying attention to this possible problem through the use of manipulation checks. Many of us have lagged behind, but here is a good reason for "throwing in" an expectancy-relevant question or two, even if we are not interested in the sheep-goat effect per se.

In light of the centrality of cue-induced expectations in parapsychological studies, it is of interest that there are growing indications that cue-induced expectancies are of importance, also, in altered-states work, especially in hypnosis studies and, probably, in psychological studies in general (e.g., Council et al., 1986; Kirsch, 1985; and other studies cited in those publications). In particular, the available evidence (Council et al., 1986)—though there are methodological problems discussed elsewhere in this paper—suggests that the hypnotic induction itself might serve as a major manipulator of expectancies as subjects



observe what happens during the induction and make attributions. These expectancies can, in turn, strongly influence behavior. The claim that expectancies are major mediating variables in response in the hypnosis setting has long been claimed by the social-psychological theorists of hypnosis (e.g., Barber, 1969; Barber, Spanos, & Chaves, 1974). What is of special interest here is that it would now appear that expectancies may have become the focus of psychologists' efforts to understand various effects in the hypnotic setting and, in particular, that the major effects of experimental cues that tip off subjects about experimenter expectancies (i.e., demand characteristics) may be mediated through the expectancy factor. (The discussion of expectancy effects in the literature is, however, considerably broader than the demand characteristics area.) This, by the way, shows even more clearly that the real-simulator design may be misconceived in its exclusive focus upon effects of demand characteristics that are not really mediated by expectancies (but by self-conscious compliance).

Here, then, is a major new development in the demand characteristics area. Many—though that is not to say all—effects related to demand characteristics may be expectancy-mediated. In short, experimenter expectancies become transferred to subjects, and thus subsequently affect both behaviors and subjects' interpretations of the meaning of their behaviors. This is a highly attributional analysis, but the theoretical framework of such thinking is far from complete. In my judgment, it does not at present adequately address the task before it. Far more conceptual and empirical work needs to be done to explain in some detail how expectancies can affect overt responding, in addition to explaining in detail how they affect subjects' interpretations of their behavior in the hypnotic setting. Angelini and Stanford (1987) have developed and tested ideas to explain how the experience of involuntariness arises in such a context, but it is not predicated in a direct way upon expectations, but, rather, upon attentional factors and subsequent attributions. Our concept can easily explain how expectancies affect attributions of involuntariness because it is reasonable to assume that expectancies affect the locus of subjects' attention during administration of suggestions. With some additional, fairly simple, assumptions, our conceptualization might also explain how expectancies can facilitate behaviors. However, it seems to me that the leading expectancy theorists have a potentially difficult unaddressed problem before them that they have not addressed: Through what means do expectancies affect overt behavior? Some very explicit conceptualization is needed here. Here is another area in which parapsychologists might make contributions

that could have direct relevance both to general psychological concerns and to parapsychology.

Thus, in hypnosis work much of the discussion of hypothesis-relevant cuing (demand characteristics) is presently developing around the concept of expectancies. Aside from the theoretical bluntness that presently exists in the expectancies area, there are also serious methodological difficulties. These center, as I have indicated, around the use of measures of subject predictions of suggested and not-directly-suggested effects (as well as the solicitation of hypnotic depth self-reports) that are reactive by their nature. Despite this fact, there seems little recognition in the field that having subjects make predictions about subsequent behavior may be reactive in the sense that they might never have made such predictions had they not been asked to do so and that the predictions in turn influence behavior. (Similar remarks about reactivity apply to the often-solicited depth reports.) What is needed now is a broader recognition that the operations used to study such effects are, to use a medical term metaphorically, invasive. They potentially bring into play in the hypnosis research area the problem of self-fulfilling prophecy, even while they may measure much more than the preexisting expectations of subjects. (Of course, none of this is to say that the present methods, even if they are reactive, have no value at all for their intended purposes. What is more, they may actually be examining the role of self-predictions and expectations, but in a way that is not immediately relevant to the original, intended question.) Here, again, is one of those very difficult problems of methodology that should challenge psychologists and parapsychologists alike to rise to the occasion.

Much has been said about possible methodologies that might be borrowed from psychology, especially from the hypnosis area. However, in pursuing the literature in hypnosis, I have developed a clear impression that methodologically, as well as theoretically, that field, like parapsychology, has a long way to go. The perpetual theoretical battles among special-process or state theorists and social psychological or social-constructionist theorists have also contributed to many heated discussions about methodology. Not surprisingly, researchers sometimes seem to prefer methods that support their theoretical predilections! Even the hypnotic susceptibility scales selected sometimes seem to vary according to theoretical camp of origin.

Hypnosis researchers at times still do studies without adequate controls for demand characteristics, even when they claim that they have instituted such controls—as was noted above under the real-simulator design. Sometimes, too, they have introduced potential confounds in

using a design intended to provide such control. Very important, researchers have sometimes used highly reactive methodology. These trends continue to the present.

As concerns the continued and often quite naive use of the real-simulator design, there seems to be an attitude of methodological imperialism that says, "If a method has been around long enough, it has claimed the territory and is not to be dislodged, whatever the cost!" Of course, we see this in all fields and parapsychology has been no exception.

There has been some real methodological progress in the hypnosis field. A hallmark here has been the recognition of the importance of getting reports about the subjective aspects of response to suggestions after the entire session has ended. Probably both special-process and social-psychological theorists will agree about this (if about nothing else). Special-process theorists need such reports in order to try to ascertain whether the defining characteristics of hypnosis are fully present, whereas social-psychological theorists need such reports in order to learn what features of the total situation influence the kinds of attributions that lead to reports of, for example, involuntariness. Whether or not the subject experienced inwardly the things suggested by the hypnotist and whether suggested behavior was felt or inferred to be involuntary are topics that have come to the fore in the last two decades of hypnosis research. The development of methods for assessing such things has led to important empirical advances, even if it has not erased theoretical conflicts.

The most important reason for relatively slow methodological advance in hypnosis and related psychological fields may actually be the absence of any better suggestions about how to do things. There is no doubt that altered states research (or internal states research, to use a somewhat different concept) is intrinsically very difficult. It should be clear that in the case of the hypnosis-suggestibility area, the parapsychologist might be able not only to borrow tools and concepts, but to contribute to the refinement of the methodology and to the empirical and conceptual development of that domain.

### *Pitfalls and Possible Solutions*

*The Delusion of Operation Omnipotence.* The belief that if one applies a fixed set of "altered-states-favoring" operations to one's subjects, they will develop such a state, is the delusion of operation omnipotence. This belief can be implicit in one's actions, as when one acts as though such operations must be effective for all subjects. Much of

what was said earlier about the importance of finding test instruments to identify individual capacity and/or inclination for internal attention states or, perhaps, altered states of consciousness was predicated upon the assumption that merely putting an individual through a hypnotic-induction ritual or exposing him or her to the ganzfeld does little if anything to insure that this person is consequently in either an internal attention state or an altered state of consciousness. If we through prior testing have evidence that the individual is capable of successful response to an hypnotic induction (through use of a standard hypnotic susceptibility scale) or is inclined toward experiencing inward attention states (through administration of the Absorption Scale), it is reasonable to assume that if other considerations are right, the person will respond positively to the set of operations to which he or she is exposed and will thereby enter an "internal attention state" or perhaps an altered state of consciousness." However, there is nothing to guarantee that this will happen, even if it is relatively likely (as we know from prior psychological testing).

If, as in the case of many psi studies, the experimental hypothesis more or less assumes that the operations do affect the individual in the desired way, we should either (a) use measures of whether or not the subject actually does respond as desired to our operations and/or (b) use additional measures to further increase the accuracy of our prediction that the individual subject will respond well to our set of operations. Let us consider the latter first.

*Better prediction.* Investigators in the hypnosis area have over the years amassed considerable evidence that motivation for hypnosis (e.g., interest in and freedom from potentially frightening ideas about it) and a trusting relationship with the hypnotist are important. (For a review of some related evidence see Barber, Spanos, & Chaves, 1974.) These are precisely the elements that are never measured in many studies involving hypnosis. It is reasonable to assume that even persons interested in experiencing hypnosis—as are most persons who volunteer to participate in ESP-hypnosis studies—might not be ready to let themselves go and respond as a "deeply hypnotized subject" if there is something about the hypnotist-experimenter that evokes a negative or uncertain reaction of any kind. Much the same can be said with regard to the experimenter in charge of ganzfeld.

These special situations in which the subject feels very passive and even dependent upon the hypnotist or experimenter may be likely to stir up anxieties or uneasiness, even of a nonverbalized kind, unless there is a feeling of trust and a degree of liking for the experimenter. This might always be true in some degree in experiments, but it may

be especially likely in those in which the subject is uncertain about what he or she should really be doing (as in trying to use ESP) and so feels a special dependency upon the experimenter. There is, however, every reason to think this would be especially true when an uncertain task like taking an ESP test is combined with a somewhat mysterious and potentially conflict-producing situation such as hypnosis or ganzfeld. We probably need to know much more about the interpersonal elements in such studies than we know at present.

Ideally, reactions to the experimenter (or hypnotist) should be carefully assessed. This might often best be accomplished by some other person than the hypnotist or primary experimenter. (Of course, it must be assessed before the subject has any feedback about ESP-task performance.) Additionally, motivation for or interest in experiencing the particular procedure used should probably routinely be assessed. Such assessment must, of course, follow efforts to insure that the subject understands what the experience will probably be like. Since such assessment of liking-disliking for the task can be influenced by social demands, it might be best to measure subjects' preferences (or even select subjects) by having them rank-order their preference for available studies after having been somewhat familiarized with them. Such measures could help to enhance prediction of who will and who will not respond favorably to a special situation such as hypnosis or ganzfeld.

*Measuring the Desired State of the Subject.* Probably one of the most useful ways to measure the degree to which the subject actually experienced hypnosis in the traditional sense of that term is to use post-session retrospection relative to classical elements of the hypnotic experience, such as involuntariness, and subjective realness of the thing suggested. We have earlier discussed how such things can be measured. Such measurements may be especially important for learning whether the subject responded favorably to suggestions of the type(s) deemed to be most important to the psi task and whether the experience of involuntariness really occurred (since the latter may help the subject to break away from egocentric approaches to the ESP task). The use of Field's (1965) Inventory of Hypnotic Depth might be a useful general tool here, as was noted above.

In the case of operations, such as ganzfeld or meditation tasks, in which the experimenter is less active than with hypnosis, knowing whether the subject entered an internal attention state or an altered state (if one believes that either construct is useful) poses greater difficulties. Earlier I mentioned the possibility that Pekala's Phenomenology of Consciousness Questionnaire should perhaps be explored, but I have considerable reservations about the present state of the

evidence as to the validity of this instrument. Still, its use in an exploratory way would be warranted. I believe that in the long run parapsychologists (and psychologists) might make their most meaningful contributions to solving such problems by using a method that is both nonreactive and much more free from demand characteristics than any form of contemporaneous or retrospective inquiry. I am referring to the use of transcripts of session utterances to develop measures of the psychological condition of the subject, including internal attention states. This approach is in its early infancy, but analysis of session transcripts represents a growing methodological tool in psychology generally. It is surely a tool that should hold promise here if we are willing and ready to devote the required time and effort to its development.

Stanford, Kass, and Cutler (1988a) found significant, replicated evidence that a certain temporal trend in the rate at which subjects speak in the ganzfeld session may be indicative of entry into and function within an internal attention state, as evidenced by its consistent correlation with Tellegen's Absorption Scale, the latter being probably the best indicator of the trait that favors the development of such attention states. This development should greatly encourage the effort to find verbal indicators of entry into and function with an internal attention state. There are no good reasons why this approach cannot be developed much further than it has been at present. If parapsychologists become involved in this effort, they might simultaneously enhance the investigator's ability to predict psi-task performance and contribute meaningfully to the emergent psychology of internal attention states.

The problem here is a tricky one. We are trying to develop methods of measuring entry into and function within special states, but we do not already have any proven, reliable measures of those things against which new measures can be compared as criteria. I nevertheless believe that progress is possible, but this is not the place to discuss precisely the approach that my student colleagues and I hope to use in our own laboratory. Suffice it to say that we hope to make use of convergent operations in trying to pinpoint the markers that will be useful indicators of the states we are seeking. I would suggest that investigators in this area not be blind to the possibility that some such markers could be somewhat idiosyncratic to the individual subject. Therefore, longer-term work with individual subjects might prove useful, as well as work with groups of subjects, the latter being what we have done to date.

*Failures to Test the Assumption of Functional Equivalence.* Recently, thanks in part to financial support from the Parapsychology Foundation, Inc., I have been assembling and critically examining the entire pub-

lished literature on scientific studies of out-of-body experiences (OBEs). The effort to digest that material is still underway. An OBE must represent an altered state of consciousness, almost by definition. Clearly, an individual in such a state is processing the usual information about the world in an uncharacteristic way and is presumably overlaying it with imaginal information. Given this obvious commonality among OBEs, it is perhaps not surprising that almost all of the empirical work on the psychology of that experience has operated off the assumption that all OBEs are functionally equivalent, regardless of the state of mind during which they were reported to have developed and regardless of the external circumstances that seemed to have supported or initiated the OBE.

This assumption may often be untenable on empirical grounds—i.e., it may simply sometimes be counterfactual—and it can, in any event, be questioned on conceptual grounds. Much of the research on psychological correlates of the OBE is aimed at discovering what developmental, personality, or cognitive factors (in interaction with life situations) either allow or predispose an individual to experience the OBE. It seems reasonable to assume, as a starting point that is itself subjected to empirical examination, that OBEs developing out of differing states of mind will in many instances not correlate similarly with psychological variables. This is because, for example, the cognitive skills and personal inclinations that allow a person to experience an OBE while ostensibly wide awake with eyes open could be radically different than those that allow such an experience while the individual is dreaming or, perhaps, even while the individual is falling off to sleep. Reality testing is very different in the latter situations and, especially, during dreaming.

The potentially dangerous assumption of functional equivalence usually leads to *a priori* pooling of data for a correlational analysis regardless of the state of consciousness in which an OBE has been reported to have occurred. The implicit assumption behind such pooling must be deliberately and selfconsciously examined. Otherwise, we may find ourselves entertaining some very erroneous conclusions. The pooling of data across such varieties of experiences can only be justified by *a priori* demonstration that classes of OBEs thus pooled are for these specific purposes functionally equivalent. For example, it must be demonstrated, if one is examining whether a history of severe childhood punishment is associated with subsequent OBE reporting, that the classes of OBEs thus pooled to observe their correlation do not have differing degrees (or directions) of correlation with the variable of interest (childhood punishment). If they do differ in this regard by some

liberal statistical criterion, pooling them could lead to extremely misleading conclusions.

This is a point that seems to have been lost in almost all of the scientific reports to date that examine psychological correlates of spontaneous OBEs. The point is of more than academic importance because in a recent study I found that developmental correlates of the OBE differ according to the state of consciousness in which individuals typically report OBEs as having occurred (Stanford, 1987b). We are in our laboratory currently examining the outcomes of a more recent such study in which state of consciousness appears to be a critical moderator of certain of the correlations between OBE reporting and psychological variables.

While the previous discussion has concerned the psychological study of the OBE, there are reasons to suppose that the same concerns about state of mind have relevance to learning whether ESP-task performance is superior among persons who have experienced the OBE. The same concerns should also be generalized to efforts to predict, say, waking ESP-task performance on the basis of questions about ostensible spontaneous psi experiences. Should persons who report all or almost all of their spontaneous "psi" experiences as emerging from dreaming be assumed to have the same likelihood of performing in a waking ESP task as those who report such experiences as emerging entirely or largely from a waking situation? I do not believe that sufficient attention has been given to state of consciousness (and perhaps setting) factors in work that seeks to predict ESP-task performance in the laboratory. Whether psi manifestations "prefer" one state of mind in a given individual is an empirical question. Whether there is a general psi proclivity that transcends state of mind is also an empirical question. The central point here is that we must be aware of the assumptions we make and must subject them to empirical examination.

### *Two of Parapsychology's Biggest Problems in Internal States of Research*

*Improper Use of Same-Subject Designs.* Elsewhere (Stanford, 1987a), I have discussed at length how the rather persistent use of same-subjects designs in hypnosis-ESP work has created several kinds of ambiguity for interpreting the findings contrasting a hypnosis and a control condition. Readers are referred to that discussion for details. Continued use of such designs can only retard progress in this area unless they are used for the explicit purpose of illuminating the undesired conse-



quences of such designs in this kind of work (or for some of the few other questions for which such designs would be appropriate).

*Subject Selection Problems (Non-Random Assignment).* Ironically, in the five instances in which parapsychologists studying hypnosis have been able to avoid the pitfall of same-subjects designs, they have stepped into another, one at least equally dangerous, but in very different ways: the problem of violating the requirement that subjects be randomly assigned to conditions (e.g., to hypnosis and control conditions) (Stanford, 1987a). Random assignment is a requirement both for statistical evaluation of such work and for interpreting the meaning of any observed difference in performance across conditions. Efforts to meet this requirement involve a number of complications related to recruitment, informed consent, and keeping the psychology constant for experimental and control conditions. There is not the opportunity to discuss these issues here.

#### *The ESP Measure in Internal States Work*

Other of the speakers will hopefully address the problem of developing more sensitive measures of ESP-task performance in the free-response setting, the setting typically used in internal states research. Here I would note that all manner of refinements in the areas discussed earlier about predicting and measuring internal states will yield little net gain for parapsychology unless our measures of ESP-task performance are reasonably sensitive and reliable. That is a difficult and complex assignment and one that requires considerable further work. The free-response ESP-task success of a particular subject will, if the subject is his or her own judge (of picture-utterance similarities), depend upon, minimally, the following factors: (a) encoding of psi-mediated material into conscious cognitions and/or perception-like units; (b) attention to the psi-mediated material as contrasted with the various sources of noise in the mentation; (c) reporting of the psi-mediated information (in some balanced relationship to irrelevant information); and (d) the complex and difficult task of judging picture-utterance correspondences.

The latter may be a particular source of error variance (or systematic bias, depending upon what problem one is studying) when the subject is judging. This is why I have opted, recently, for judging by more than one experienced outside judge when doing process-oriented ganzfeld-ESP research. In order to help insure maximal access to subjects' thoughts during the session, the external judges have also had access to supplementary comments by subjects, comments made after

their hearing read back to them notes on their spontaneous utterances during the session, but while they are still in ganzfeld (except that noise is no longer introduced). This approach is worthy of further exploration and comparison with results using subject judgments.

It is possible that some of the refinements in judging and/or score development being discussed by various researchers will help further reduce the problems of error of measurement in free-response ESP-task performance, even when subjects are their own judges. I would also note that choice of pictures (or other stimuli) for targets and foils (or decoys) is a critically important matter to which much attention needs to be devoted. Probably others will address this matter to some degree.

#### *Re-examining the Alleged Resistances of Subjects Who Seem Ready to Let Us Down*

There has been considerable discussion in the parapsychological literature about alleged deep-lying fears of psi phenomena, especially of strong psi phenomena. (For reviews, evidence, and related discussion see Tart, 1984, and Tart & Labore, 1986.) My purpose here is not to dispute that fears of such events sometimes exist and can affect the performance of experimenters and subjects. I have no quarrel with that. I do wish, however, to suggest that some behaviors on the part of subjects that have sometimes been construed as indicating fears and related resistances to demonstrating psi phenomena might have another interpretation, one now widely known among social, personality, and clinical psychologists, but which has never, to my knowledge, been discussed in parapsychological writings.

Let us now look more closely at the subject behaviors sometimes interpreted as being due to fear and/or resistance to producing psi phenomena. Subjects who have been succeeding at psi tasks in the laboratory sometimes seem strongly inclined to do things that would seem to lower expectations (their own and those of the experimenter) for subsequent success on the psi task. Some parapsychologists have privately indicated to me that they are sure in such instances that subjects, having discovered their powerful psi abilities, are becoming ridden with fears about those events. I would suggest that there is an alternate explanation that should be considered for such a turn of events.

The proposed alternate explanation is known as "self-handicapping." Basically, self-handicapping occurs when a person either creates or allows a situation to develop that will allow a handy excuse for failure at some task. In an early paper, Berglas and Jones (1978) considered self-

handicapping to be "any action or choice of performance setting that enhances the opportunity to externalize (or excuse) failure and to internalize (reasonably accept responsibility for) success" (p. 406). This means that if failure occurs, it can be blamed on something other than inability or incompetence, but that if success occurs, real ability is strongly inferred. The implicit ideas here go back even further to early attribution theory (Kelley, 1973). H. H. Kelley proposed that if more than one cause for a given event is present and the event occurs, the presence of one or more alternative plausible causes means that in interpreting the event, a given single possible cause for the event will tend to be discounted or given less emphasis. This is known as the *discounting principle*. One deduction from this principle is that if both an obstacle to performance and a lack of ability could explain a person's failure on a task, failure in the presence of an obstacle to success is less likely to be attributed to lack of ability. On the other hand, if something happens and there are present both facilitating and inhibiting causes, the role of the facilitating cause will be emphasized in attempts to explain it. This is termed the *augmentation principle*. Thus, if a person is trying to do something (facilitative cause) and the task is very difficult (inhibitory cause), success will mean that the facilitative cause will be given emphasis in explanation—the person will be seen as having great ability. If one can arrange a strong inhibitory cause, then success means that one can think very well of oneself. If failure occurs under the same circumstance, one need not think badly of oneself; one can continue to believe in one's competence. (The same analysis applies to others' making judgments about oneself under such circumstances.)

It is, therefore, of considerable interest that several parapsychological investigators with whom I have talked have reported what appears to be self-handicapping behavior by subjects after those subjects have already established some reputation at the task. (This is why some investigators have suggested that this is all "psi resistance" due to strong fears stirred up by the feeling that the subject might have strong psi.) The subject, in terms of the self-handicapping construct, had begun to build a sense of private self-esteem (not to mention, public image) around being a "good psi subject." The continued experimental work posed a threat to that private self-esteem (and to that public image), but the use of self-handicapping would help reduce the threat.

It is of considerable interest here that the psychologists who study self esteem have discovered that the research paradigm that most reliably elicits it is one of prior *noncontingent* success, in other words, success not clearly dependent upon the magnitude or quality of the subject's own efforts. Anyone familiar with psi research will know that

this is exactly the distressing circumstance of the vast majority of subjects who succeed at our laboratory tasks! They are having success and would like to continue it, but they just do not know what to do to help insure success because they are unsure of how they accomplished their success in the first place.

Researchers have found that with feedback about noncontingent success—essentially, what one has in psi-task performance, from the standpoint of most subjects' perceptions—subjects will, if given the opportunity, work under a difficult circumstance (such as performing under the influence of an inhibiting drug, Berglas & Jones, 1978, or under some distracting condition) or may elect to participate in a task that has no diagnostic ability (relative to the area in which success has been established) (Sachs, 1982). Interestingly, subjects who are faced with such prospects of failure will sometimes elect to take drugs that would deter success, such as alcohol, prior to performance (e.g., Kolditz & Arkin, 1982; Tucker, Vuchinich, & Sobell, 1981, as cited in a review in Leary & Miller, 1986, Ch. 4, "Self Processes and Behavioral Problems"). All this might remind parapsychologists of some things that have happened with certain of our special subjects.

Roy F. Baumeister and Steven J. Scher (1988) in a review of self-handicapping and other topics related to self-defeating behavior regard self-handicapping as "tradeoff" that sacrifices one's chances for success in exchange for attribution-related benefits (protection from implications of failure, but special credit for success). In support of this tradeoff concept, they cite work by Greenberg, Pyszczynski, and Paisley (1984) that showed self-handicapping when the stakes were low, but not when a large amount of money was potentially available. In the latter case, persons dispensed with self-handicapping and apparently tried their best. This suggests a cost-benefits analysis relative to the choice of self-handicapping or not (for at least certain subjects).

Every teacher knows that if a student has doubts about his or her ability to deal with the material and thus has doubts about success, such a student will often exert minimal or no effort to succeed. Here, self-esteem could be protected and apparently is, even at the cost of a poor grade. This is surely a form of self-defeating behavior, viewed from the perspective of the outsider. (By the way, no teacher would be ready to allege fear of success in such a situation!) Some studies have shown a reduction of effort to be a way of avoiding the negative implications of failure (e.g., Harris & Snyder, 1986; Tice & Baumeister, 1984, cited in Harris & Snyder, 1986); however, the Harris & Snyder findings held only for male subjects who were uncertain of their answers to questions related to self-esteem).

There are, however, hints in the literature about how one can get persons who are frustrated by previous failure on a task to engage their efforts and work very hard. This is by giving a task that is described as very difficult, for it carries its own excuse for failure that protects private self-esteem (and public self-image) (e.g., A. Frankel & Snyder, 1978). Such a task can be preferred over one of moderate difficulty, according to the research cited.

Other self-handicapping strategies include insufficient practice or preparation prior to an important evaluation of some kind, something that apparently happens mainly among individuals who are inclined to use self-handicapping as a protective strategy (Rhodewalt, Saltzman, & Wittmer, 1984). Some self-handicapping researchers have developed and used inventories to pinpoint individuals particularly prone to such strategies, as in the study just cited, which concerned the preparation of athletes for contests. Interestingly, individuals inclined to use such strategies appear to be more likely to use them prior to important contests. Parapsychologists might benefit by the use of one or more such inventories, especially when they are undertaking longterm work with particular or "special" subjects. This could alert one to which individuals are likely to use self-handicapping strategies in the face of the uncertainty that always attaches to parapsychological success (just as it attaches to success in athletic contests, for which ultimate success depends upon one's own abilities, one's efforts, *and* those of the rivals, not to mention "chance" factors).

Baumeister and Scher (1988) in reviewing self-handicapping note two general forms it can take: the creation of obstacles to one's own success that can serve as an excuse in the event of failure; and citation of external excuses that can be conceived to have interfered with success. The first is obviously in some sense self-defeating in that it at least increases the likelihood of failure. It can take many forms, some of which I have mentioned above. They include taking alcohol or drugs, failure to practice adequately and deliberate low effort. Very insidiously, this can include the choice of a very difficult goal, whereby one cannot be blamed for failure (Greenberg, 1985, cited in Baumeister & Scher, 1988). The second form mentioned in the review just cited can take the form of reported test anxiety or poor mood. As Baumeister and Scher note, whether such excuses are truly self-destructive in character will depend upon whether they are merely cited retrospectively or are somehow actively fostered in anticipation of possible failure. In practice, it is difficult to know whether such excuses refer to realities or fictions. However, there is evidence to suggest that persons can make such things as test anxiety, bad moods, and troubling bodies quite real to themselves

on habitual bases, using them handily when they would help to preserve self-esteem (or allow impression management). (For a most interesting, even exciting, review of such research, see Leary & Miller, 1986, Ch. 4, pp. 56-58).

Whether self-handicapping really subserves private self-esteem or public self-image (impression management) is sometimes debated in the social psychological literature. Kolditz and Arkin (1982) addressed this issue by manipulating whether others did or did not know about the self-handicapping circumstance. (If there is no role of public self-image in all this, such a manipulation should make no difference; everything would be predicated upon what one can believe about oneself.) Their study supported the impression-management interpretation. However, there is currently no reason to suppose that self-handicapping is only of an impression-management kind. It would in many instances simultaneously serve both impression-management and self-esteem-protective functions.

The issue of self-esteem versus impression management is, in some respects, a somewhat artificial one. There can be little doubt that self-esteem is meaningfully and regularly affected by our observations of the impressions we make on others and by how they react to us and our efforts. Even the very concept of self has in the view of some commentators been fundamentally linked to how we see others react to ourselves; the self sense can only develop in interaction with others, according to symbolic interactionists such as George Herbert Mead (as discussed in Baldwin, 1986). The question does take on some pragmatic interest for parapsychological researchers, however, because if self-esteem is the primary instigator of self-handicapping, it is possible that subjects will self-handicap and we will never learn about it. If impression-management is involved, on the other hand, even if the selfhandicapping occurs in private, it would seem likely that the experimenter would somehow be let know about it. Otherwise, the ploy would not work. If investigators are clever and are insightful about junctures at which selfhandicapping is likely to occur, they might be able to control the type that occurs and thereby manage the situation a bit. Some systematic study of the consequences of deliberately providing self-handicapping opportunities is needed. I would also suggest the importance of learning who is most inclined to use such a strategy and who is not. One way of identifying persons inclined toward self-handicapping is to give the subject the opportunity to select a self-handicapping circumstance in a situation that could be expected to trigger self-handicapping in persons thus inclined.

The implications for actual task success of having a self-handicap are

less clear, at least on the basis of my personal knowledge of the current literature. Probably the answer is quite complex, but this should not deter parapsychologists from looking for an answer. Actually, that answer would probably depend upon a combination of circumstances and, as such, might vary from situation to situation.

What is clear from the above analysis is that there is every reason to expect to find self-handicapping in parapsychological settings, especially at critical junctures in a subject's work, because of the experienced non-contingency of success. As noted by Baumeister & Scher (1988), "The central cause of self-handicapping appears to be some form of induced insecurity about future performances, especially when coupled with high external expectations for success" (p. 8). The latter consideration combined with the experiential lack of a sense of outcomes being contingent upon what one does must surely make self-handicapping a major concern of many psi-test subjects and, therefore, of psi researchers. Researchers need to bear these things in mind and not automatically construe as "resistance" to success the potentially harmful things subjects do to themselves, claim about themselves, or select as test circumstances. Resistance to the perception of personal failure may be more likely than resistance to success, given the experiential non-contingency of the whole situation!

What has this to do with research on internal attention states and parapsychology? First, self-handicapping in psi research is a broader issue and, as such, is worthy, in its own right, of mention in a conference devoted to methodology. It provides a potentially helpful new way of interpreting certain behaviors frequently noticed in subjects. It could lead to more understanding ways of responding to such situations. When the subject really fears potential harm to his or her image in the eyes of the experimenter, but that experimenter feels sure the problem is resistance to psi, this represents a form of social insensitivity to the consequences of what we are doing to subjects. (Of course, this does not mean that fear of psi and resistance to it may not sometimes emerge as real factors. My personal guess is, however, that self-handicapping related to perceived noncontingency is more often the culprit.)

Possibly self-handicapping is most likely to appear in internal states research when there is extended experimentation with a given subject or when the subject already has a reputation to defend in the laboratory. For first-timers in hypnosis-ESP work or ganzfeld the magic of being exposed to an elaborate setting or technique that is intended to "cause it to happen" might lessen the sense of ego-involvement that can favor self-handicapping. The subjects' interpretation of the situation may be such that they tend to view themselves as very passive and waiting for

the magic to make psi happen. If it does not happen, maybe the effort just somehow did not "take." On the other hand, continued, regular success in such a setting creates a reputation—the ability to function as a psi specialist in such a setting—that must somehow be defended (even if one is unsure how one "makes it happen," but has only some half-baked ideas that never get solid confirmation). The same thing might be true with a self-proclaimed psychic in these settings. Here some opportunity for self-handicapping might be welcomed and might help minimize the egocentric involvement that might deter success. Sometimes it might come in the form of a statement from the experimenter that, "Of course, I recognize that you have never worked under precisely these circumstances before."

Chronic self-handicappers might well be recognized by their readiness to come to the session with a built-in excuse for failure, such as "I sure didn't get much sleep last night, but I thought I would come up and try anyway." Perhaps I am wrong, but my intuition says that one will find quite a few self-proclaimed psychics ready to use self-handicapping. Part of this might derive from their intrinsic uncertainty about how they do what they do and the difficulty of their meaningfully preparing for a session—it is not quite like practicing a difficult piece on the piano before a concert. There may be other factors here, too.

One of the nice things about techniques such as ganzfeld and hypnosis is that they, as special settings, do not directly raise expectations about the person's ability to do "psychic things" in other settings. Even considerable success in such a setting need not increase the subject's feelings that the experimenter will automatically expect success in another setting. Thus, the subject does not have to worry about being "put on the spot" in that regard. If the subject does try a different task (e.g., a non-ganzfeld one), he or she comes to it without any prior commitment to success—it is *so* different as compared with ganzfeld—and he or she need not be embarrassed about failure.

Another way in which altered-states-related self-handicapping can occur is use by subjects of drugs such as alcohol or marijuana that, according to folklore, can *in just the right amounts* facilitate psi performance by "taking off the edge" and getting a person to relax, be unanalytical, and spontaneous. However, with *just a bit too much*, the same drugs, according to the same folklore, can deter psi performance. So, what better self-handicapping device in the face of a potentially threatening psi task than to tell the experimenter—whether or not it is truthful—that one has been "priming" for the study and hopes one has gone just far enough but not too far? With success, one might look clever and resourceful, but failure would mean, not that one's powers



had failed, but that one had accidentally gone "over the line" in a special preparation one had tried to make. "Sorry!" Of course, this can also protect one's self-image as a successful psychic.

The concept of self-handicapping deserves the serious consideration of the working parapsychologist, who should probably make every effort to examine the relevant psychological literature. It might provide a much-needed new way of looking at some old problems in psi research, problems that are almost unavoidable when a subject has created some degree of reputation in the parapsychology laboratory or initially comes to the laboratory with a reputation to defend. The opportunity for the parapsychologist comes in understanding how this problem might develop and manifest itself in a particular study or, perhaps, in the individual subject.

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### DISCUSSION

EDGE: I think you provided us with good recommendations that parapsychologists, whenever possible, ought to stay away from the CURSS, from the Barber Suggestibility Scale.

STANFORD: I did not comment really negatively on the CURSS; actually the CURSS is all right, except perhaps for the very brief induction.

EDGE: . . . and from naked absorption. Now, that is what we have to look out for.

HONORTON: I am not sure really what all this has to do with the kind of internal attention states research that predominates in parapsychology today. And I am wondering whether you would say a little bit about the various versions of the hypnotic susceptibility scales. What does this have to do with ganzfeld research?

STANFORD: Hypnosis research?

HONORTON: Are you suggesting basically that we should be focusing much more on hypnosis per se or using models and methods from hypnosis? What really is the message here?

STANFORD: I will not discuss the paper that I completed and then hid somewhere. You can have a copy of that, too, if you want. In this particular paper I am focusing on the idea that we need to know something about the skills of the people who come into an internal states experience setting. Do they have the skills that would allow them to be the kind of people in that setting that we wish them to be when we invite them in? Hypnotic susceptibility scales can help us to know for sure, especially a prudent choice of scale. If we want to use suggestions of a particular type, such as hypnotically induced dreaming, there are ways of premeasuring that capacity using some of the standard scales. We need to establish that subjects actually experience involuntariness before we can test the role of involuntariness. There is a real chance

that the experience of involuntariness is very central to getting something to happen under hypnosis vis-à-vis psi because of that kind of extra ego feeling that something is happening to the individual that is interesting and exciting.

HONORTON: What constitutes an adequate control condition for an internal attention states experiment?

STANFORD: What constitutes an adequate control for an internal attention states experiment? What constitutes an adequate control depends on what hypothesis you are testing. If you have a hypothesis about why, say, ganzfeld favors ESP task performance then you have obviously got to have a control group that tries to hold everything constant except that particular factor. I am not going to say that is always necessary, but that is what you have to got to do. But the notion that there is an abstract kind of magical control group that you need for ganzfeld experiments is, in my opinion, simply wrong. It depends on the type of conclusion you want to make from ganzfeld experiments. But if you are just asking if this condition is favorable to the occurrence of psi, you just find out if psi occurs there or not. But that does not tell you anything about why it occurs.

SCHOUTEN: Well, I agree with Rex in the sense that my feeling is that of course you are never certain about what the control is. But in the case of control of the ganzfeld specifically, I always had the naive feeling that the procedure itself, isolating the perceptual input, is supposed to create the altered state which was supposed to be psi enhanceive. So I figured in this case that logically the first control would be to compare two situations in which either the subject is isolated in a specific ganzfeld way by having the ping-pong balls on versus having exactly the same situation without the ping-pong balls on. Now that is not a perfect control, I immediately agree to that, but let us say at least it would tell you something about the assumptions mentioned above if the experiment had given a clear answer which I would have loved to see. You can expand on that, but the point is I think that is the first sort of control I would look at.

STANFORD: That is probably because you wonder if a uniform visual field is an important factor. If that is one's hypothesis, it seems to me that is the route one would go, if you think that uniform visual stimulation is an important factor.

SCHOUTEN: We are dealing here with the state itself. That is supposed to be the real factor. Now ganzfeld is supposed to enhance that state, to bring subjects into the state. If you have an independent way to measure the state then I would say that that is the way to go with it, but I doubt whether you have.

STANFORD: That is one of the things psychology lacks for a given setting, be it hypnosis or in our case perhaps most important the ganzfeld right now. We do not have an assured way of knowing whether a person is in an internal attention state when he is in ganzfeld. We can predict whether they might be by using things like the absorption scale, inventory of childhood imaginings and memories, fantasy proneness, other personality scales and maybe hypnotic susceptibility all bundled up together. Then we would get people who are high on all of those and almost guarantee that you would find somebody in the ganzfeld who would experience an internal attention state. But maybe they do not like the person who is running the ganzfeld, they do not want to open themselves up or something like that. We have been working with trying to develop objective measures of entry into an internal attention state. I do not have time to get into this here, but in the Stanford, Kass and Cutler paper presented at the 1987 PA Convention you will find our work on this. I think we are making headway and we have some interesting leads that we want to follow-up, too, and we are going to continue work on it. It is not an easy question. I discuss this in my paper to some degree.

HONORTON: I would argue, Rex, that with respect to the ganzfeld in particular, the ganzfeld is by definition an environment in which the individual is in an internal attention state because he is either asleep or he is in an internal attention state. There is no other possibility in the ganzfeld.

STANFORD: Well, I do not think that that is necessarily true. I have had subjects sit in there and talk about the setting, the room or the chair or their personal comfort or this or that. When we speak of an internal attention state we mean a certain type of passive attention.

HONORTON: Well, my definition of it is in my Wolman chapter which is what I have been using all these years. Simply, an internal attention state is a condition in which the individual is awake and is alert, but is focusing on and responding to internally as opposed to externally generated stimuli.

STANFORD: My response to that is you can lead a horse to water, but you cannot make him drink.

SCHOUTEN: I think so, too. The little experience I have had in this area is that it is perfectly possible that a subject who has the ping-pong balls on starts fantasizing and triggers all sort of fantasizing processes which you can do with or without the ping-pong balls. The ganzfeld in itself is not a guarantee that subjects enter an altered state. Just defining an altered state by the method one applies doesn't really help us.



STANFORD: Some of our people on occasion sleep quite a bit.

MAY: On this point I have to say I disagree with Chuck. To assume that someone is focusing his attention on internal processes is an enormous assumption. I am expanding on Rex's comment to you because, personally, when I get in there I am looking at the damn ping-pong balls. I am not paying the slightest bit of attention to what is going on in my mind. For me personally the procedure is very distracting. Now is that an internal attention state?

STANFORD: If you know in advance about the blank-out effect that occurs where you cannot tell whether your eyes are open or closed, you can be sitting waiting for that to happen. It is kind of cute when it happens but it is actually a physiological event.

MAY: But is that internal attention? You see that might fit his definition of internal attention.

STANFORD: I do not think so, not in the sense that Chuck really wants to get at things that are being generated in the head that might match up with targets.

HONORTON: I think it is important to understand in terms of the origin of the ganzfeld that the idea was not that the ganzfeld produces some kind of weird state. The idea was that the ganzfeld would provide a way of approximating the kinds of "altered states" that have traditionally been associated with psi, particularly dreaming.

STANFORD: Well, I understand that, but one thing I have an unshakable conviction about is that the ganzfeld does favor psi. We get people in the ganzfeld in our lab who experience an internal attention state there that fortunately they do not do in my class lectures. We have people who are zonked, plastered to the chair, so to speak and we have people in the ganzfeld who are very much, by almost any definition you use, in their ordinary frame of mind when they are talking in there. And this is why we need ancillary devices to try to pinpoint what is happening internally to help to predict it. This is not to detract from the importance of the ganzfeld as an instrument in parapsychology. I think it is extremely important precisely because it does set up a circumstance that very few of us have ever experienced where we just do not have distractions.

HONORTON: That is the point.

STANFORD: And that is very, very important.

BRAUD: There is a suggestibility scale by Sheryl Wilson and Ted Barber called the Creative Imagination Scale. It is a very non-authoritarian one. I would like to know your thoughts about that scale.

STANFORD: Yes, the Creative Imagination Scale is great provided you are interested in what they are interested in, which is basically

fantasy proneness as such. It might be really useful for something like the ganzfeld where you are interested in fantasy proneness as a moderating variable of the effects that you get in ganzfeld. But if you are really interested in hypnotic susceptibility, it is probable that you should use one of the standard scales and, I would suggest, supplemented by inquiries about subjective experience. The reason is that it has been shown in the literature that the CIS does not have the same psychometric properties as the major hypnotic susceptibility scales. There are a number of studies that show that now. There does seem to be a pure imaginal factor in the hypnosis domain. For instance, if you score the CURSS for subjective or objective involuntariness it gives you a one-factor scale. Nick Spanos is the original author of the CURSS; he believes it is related to fantasy proneness. But if you look at the behavioral response, it is something else. Now behavioral response may be important in addition to subjective response. There are people who do not produce behavioral responses in the CIS, as you know, but the behavioral response may be important because it feeds into the attributional process. It gives feedback to the subject that something outside his ego is happening. This may really be important for the consequences of our hypnosis experiments in particular. So for that reason I would suggest using one of the standard scales, ideally the Rolls-Royce of the scales the ISHSS:C and supplementing it with some subjective reports afterwards. By the way, I have got to mention something that is related to what you said. If we want to find out what people experience under hypnosis, I suggest we not do it by interrupting the process with a bunch of state reports. That is another problem with state reports. What we need to do is to use tools like Field's (1965) Inventory of Hypnotic Depth that ask questions related to standard classical effects that people experience under hypnosis. This has been used many times in the literature, a lot is known about it. It could be extremely useful to us as an after-the-fact retrospection about whether a person was experiencing classical hypnosis or not.

PALMER: This paper is very timely for me because I have been considering using hypnotic susceptibility scales as a screening device for my subliminal perception research. You are not the first one who has recommended the Stanford C in that connection.

STANFORD: Tart told us that years ago.

PALMER: I have not gotten into this in much detail yet, but my understanding is that many, if not most of, the Stanford C items are rather hard to pass because it is an advanced scale. When you are using it as a general screening device you may find your few virtuosos, but also you are going to give a lot of other people failure experiences that

could conceivably influence their mental state. This could be a problem if you want to test them in other kinds of experiments. One possible solution is to give the Stanford A and B first.

STANFORD: Some people do that, but there are a lot of studies where they do not. You could do that because it does not have those items and you can use the other for later screening purposes. You can use a stage process, but often we do not have time for that; also it is expensive.

PALMER: Well, that is the problem, so we are probably going to end up using just the Stanford C. I would like your thoughts about whether the concern I have just expressed is something I ought to worry about.

STANFORD: I think it would depend upon what you actually wanted to do with the results of it. We know that hypnotic susceptibility is pretty stable over a period of time. People are as susceptible as they are. They are going to find out sooner or later in your study. If you fully measure ahead of time how susceptible they are, there is the chance for negative feedback. You could conceivably modify any scale so that you can null out the effects of negative suggestions. But let me point out that all these scales are arranged in theory as a kind of Guttman scale where the items get progressively more difficult in a step-wise order as to how many people pass. People are not affected by failure up to a certain point on the scale and that is a useful thing to keep in mind.

MORRIS: I would like to hear more about self-handicapping. Can you tell us more about some of the literature that has been built up around it in terms of how analogous it may really be to our research and whether there are variables such as proneness to self-handicapping or whether it is task dependent or varies with social context. Is there enough there so that we can begin making some predictions?

STANFORD: There is quite a literature. In some aspects of the literature there are definitely some contradictory unresolved results. I have cited some surveys of it in my paper that you can look at and there is a number of others that I did not cite. I am not up on all the literature on this. But let me say first off that a bottom-line boundary condition for the effect it seems is usually that a person should have some sense of success at something and have an opportunity to do it again, or something similar, and not have the slightest idea of how it operates. The effect has been studied in many different contexts, and one thing that is clear is that it definitely functions as an impression-management strategy. In other words, for instance, we know that people self-handicap in order to make a good impression on other people. That is one thing that we know. It has some interesting implications. It implies for instance that if they self-handicap, they are probably

going to report to you ahead of time about the self-handicapping situation that they have instituted. If you do not allow them an opportunity for that, you are depriving them of the opportunity to self-handicap. That is one kind of implication. But there is also some evidence to suggest that they do it to manage their own kinds of self-esteem. All right, let me sneak in something here.

People have been asking how this applies to parapsychology. Let me tell you one angle where I think it really does apply. Some of us have encountered this in our laboratories, I think. Now parapsychologists, of course, never encourage subjects to go out and alter their state of consciousness for an experiment in ways that are illegal, illicit etc. But sometimes we have seen people who go out and do it anyway. They take some kind of drug beforehand, including one that according to legend, can in just the right amounts facilitate psi performance. But the legend also says that in the wrong amounts, just a bit too much of it deters psi performance. So that if you come into an experiment and you have soused yourself with something ahead of time, this is a super self-handicapping strategy. I really think that we parapsychologists need to get into that literature and try to tease it apart for ourselves and start considering this. I would be extremely surprised if many of the things that we have not interpreted as resistance are not attempts to protect one's own impression in the eyes of the experimenter and those around him or one's own self-esteem. If we do not recognize that in our work we are being very insensitive to our subjects socially. There is a little bit of self-flattery that comes in, in a way, that it excites us to say, "Wow, they are scared to death of the powerful effect that we have been getting." That makes us feel we are onto something really hot, so to speak. And I think that it may be a kind of a back-of-the-mind motivation that makes us accept, almost without question, that just about everything subjects do that is untoward is out of fear. I am not suggesting that fear is not a factor. I believe that it is, but we need to be careful. Now one of the most interesting questions concerns what you do about all this in your experiments. Do you allow subjects the opportunity to self-handicap or do you not? Well, it depends upon what you want to find out in you study for one thing. But also you know already from the literature the point at which it is likely to start to happen. It is as soon as the experimenter and all the information feedback the person is getting says you are a reliable performer, you have a reputation, then it starts to happen. We have some old folklore in this field related to this. When I was working at the University of Virginia Medical School I had people who would

come in and start telling me about a seemingly genuine psychic experience before the experiment. I made it a rule to ask them if they would please tell me that afterwards. I am extremely interested, but we need to go into the procedure right now and please be sure to tell me afterwards. They wind up going out on a limb making a reputation to defend if they say it ahead of time. Now, I think we need some studies to look at this kind of thing in our field because we know that this interfaces with parapsychology in terms of ego-centric effort. Subjects do not know how to make it happen. They do not have any insight into the process because this stuff that we are dealing with is ego alien and then you get in there and you have to make it happen. What do you do? You may try all kinds of crazy things that mess up the process. I am just trying to open up the discussion to this kind of thing. I do not have a lot of answers for you, Bob, at this stage. Maybe at another conference I will, but I am going to be digging a bit more into the literature myself. I thought it was really important to get this out since I just thought about its application to parapsychology within the last year and half. Let us see where it leads.

SCHOUTEN: I guess your paper is "must reading" for everybody who wants to go in that direction. What surprises me though is that I had expected that to measure hypnotic susceptibility experimenters would also use psychophysiological methods. I can well imagine that you know the extent to which a person is able to influence his own autonomic processes and that might be used as indicative of susceptibility. I have always been skeptical about hypnosis. What convinced me that something real was there was a few instances where people were hooked up to an EEG and I saw very dramatic effects in EEGs and other autonomic measures. They were so impressive that I thought that something must certainly be there.

STANFORD: May I ask what kind of effects? What were you looking at? What was the occasion of the effects?

SCHOUTEN: Well, those were not experiments, so it was a very personal impression. But you see the recordings following suggestions and simply with the eye, you can see that the EEG becomes enormously different. Things like that really impress me, but that is no valid evidence whatsoever. Did you look into that literature?

STANFORD: That could lead to a study. The hypnosis literature does not suggest that there is any physiological measure that per se is indicative of being in a state. The state theorists would love that to be true, and it has been looked for a lot, but it simply is not true so far as we know. What you can get on hypnosis in terms of phys-

iology is a response to suggestions. One investigator did a study in which he gave a suggestion that was supposed to frighten highly hypnotically susceptible subjects. They got all kinds of heart reactivity, wild variability and so forth in response to suggestions that were supposed to produce physical anxiety. You can get real physiological changes in response to suggestion with this. It might be a way of seeing if it is catching on. It might catch on in the same person if he was just asked to imagine it outside of hypnosis. But these kinds of scales are not measuring state per se but how much people become involved in the suggestion. But that is obviously very important in our studies where we use suggestions to try to get parapsychological effects. I think that is a very good constructive direction we might want to take a look at.