IS HYPNOTIC ABILITY A RISK FACTOR FOR SUBJECTIVE (VERBAL REPORT) PSI, SOMATIZATION, AND HEALTH CARE COSTS?

IAN WICKRAMASEKERA

Health care costs were $544 billion or 11.4% of the Gross National Product in 1989 (Durenberger, 1989). In 1950 health care costs were 4.6% of the gross national product. Somatization is a major factor contributing through *overutilization* of physician visits, hospitalizations, high tech tests, surgeries and iatrogenic illness to the escalating cost of health care in the U.S. (Quill, 1988; Katon, Kleinman, & Rosen, 1982). Somatizers are people who transduce perceptions of psychosocial conflicts into somatic presentations (Wickramasekera, 1989b). Somatization accounts for approximately 50% of all visits to primary care physicians (Katon et al., 1982).

I have hypothesized that "unassimilated" *subjective* psi reports (anomalous experiences) are a measurable risk factor for the presentation of psychological and somatic symptoms (Wickramasekera, 1979, 1983, 1986, 1988) (see Figure 1). Clinically we have observed numerous instances of chronic resistant psychophysiological disorders which rapidly resolved when we provided the patient with an invitation to verbalize, reframe, and assimilate distressing anomalous perceptions which they had previously been afraid to verbalize lest they be regarded as crazy or "possessed." Our data (1979, 1983, 1984, 1988, 1989a) and clinical experience renders us comfortable in issuing such an invitation routinely to all patients and particularly to those who score over 9 on the Harvard scale. Often, but not always, the patients are relieved to talk about the incidents of subjective psi and report that I am reading their minds because they have previously not revealed the psi incidents to anybody.

The hypothesis that subjective psi is a risk factor for somatization may or may not be true, and is a topic for empirical study. For example, have professional psychics had bigger medical expenses than the average person? If this hypothesis is true, then the investigation of subjective psi reports may have economic consequences in terms of reducing

Figure 1. Hypnotic ability and subjective PSI (verbal reports).

1. Wickramasekera 1979	Highs = 80%	Lows = 25% (patients)
2. Wilson & Barber 1983	Highs = 92%	Lows = 16% (non-patients)
3. Wickramasekera 1984	Highs = 63%	Lows = 20% (patients)
4. Wickramasekera 1986	Highs = 71%	Lows = 19% (patients)
5. Wickramasekera 1989a	Highs = 80%	Lows = 32% (college students)
6. Wagner & Ratzeburg 1987	r = .26 ($p < .04$)	(college students)
7. Nadon & Kihlstrom 1987	r = .22 ($p < .01$)	(college students)
8. Council, Greyson, & Huff 1987	r = .34 ($p < .05$)	r = .38 ($p < .01$)
9. Richards 1989	overall	r = .175 (non-significant)
	subset	r = .53 ($p < .01$)

mental and physical health care costs. The empirical study of subjective psi reports as a risk factor for somatization may be an idea that in William James' terms has "cash value" today and whose time has come. The concept of psi as a risk factor for psychological and somatic symptoms may provide incentives to fund basic and applied psi research in the Clinton administration.

I previously (1979, 1983, 1984, 1986, 1988) reported (see Figure 1) several small studies showing that "high" (Stanford or Harvard scores 12–9) hypnotic ability was a risk factor for somatization and subjective verbal reports of psi. These subjective reports of psi in high hypnotizables were found to be *unrelated* to serious psychopathology (psychotic process) in both somatizers and a normal college student sample (Wickramasekera, 1989a). It is noteworthy that one of the most promising empirical sources of evidence for objective psi, the ganzfeld research (Rao & Palmer, 1987), incorporates two procedures (sensory restriction and verbal relaxation instructions for low physiological arousal) that are known to at least temporarily, but reliably, increase hypnotic ability above baseline levels (Pena, 1963; Wickramasekera, 1969, 1970, 1971, 1973, 1977; Barabasz & Barabasz, 1989; Engstrom, 1976).

Hypnosis can be defined as the verbal induction, in select subjects, of reliable and large magnitude changes in perception, memory and mood that can have profound psychological and psychophysiological consequences (Spiegel, Bierre, & Rootenburg, 1985; Spiegel, Cutcomb, Ren, & Pribram, 1988; Laurence & Perry, 1983; Dywan & Bowers, 1983; Wickramasekera, 1979, 1986, 1988). For people of high hypnotic ability these psychological and psychophysiological changes do not necessarily require the ritual of a hypnotic induction (Barber, 1981; Council & Loge, 1988) and can under certain additional high risk conditions (Wickramasekera, 1979, 1986, 1988) be associated with the amplifi-

cation *or* attenuation of fear and/or pain (Spiegel et al., 1988; Stam, McGrath, Brooke, & Cosier, 1986).

Prior reviews of the hypnosis and psi literature (Van de Castle, 1969; Honorton & Krippner, 1969; Schecter, 1984) have suggested that hypnotic induction amplifies objective psi performance. But these reviews failed to control systematically for hypnotic ability. Hypnotic ability is a normally distributed stable individual difference variable (Barber, 1969; Hilgard, 1965; Kihlstrom, 1985) that appears to be partly genetically based (Morgan, 1973; Morgan, Hilgard, & Davert 1970).

In 1983, Barber and Wilson confirmed my prior report of an association between high hypnotic ability and subjective psi verbal reports. In a sample of very high hypnotic ability (top 4% of the population) non-patient professional females, they found that 92% of the *high* (N=27) and only 16% of the *lows* (N=25, low and medium hypnotic ability control group) reported subjective psi experiences. Subsequently there have been at least 3 independent replications (see Figure 1) of a positive correlation between hypnotic ability (Harvard scale) and verbal-subjective reports of psi with large normal college student samples (Wagner & Ratzenburg, 1987; Nadon & Kihlstrom, 1987; Council, Greyson, & Huff, 1987). Richards (1989) who studied an unusual sample (Association for Research and Enlightenment members N=120) of older adults (x age=47 SD=10.85), which consisted only of "sheep" (no goats), failed to replicate with his "overall sample" the prior finding of an association between hypnotic ability (Harvard) and subjective psi reports. But in a subset of his own sample, on a separate testing session, he replicated the positive correlation between hypnotic ability (Harvard) and subjective psi reports (N=32, Harvard and psi r=.53 P<.01). While hypnotic ability was normally distributed in Richard's (1989) study, the lack of "goats" in the subjective psi domain may have skewed the distribution and attenuated the correlation in his larger sample.

Wilson and Barber (1983) used a procedure that selects for very high hypnotic ability (somnambules) and described a personality construct they term "fantasy prone personalities" or "fantasy addicts" who report intense imaginative involvements in reading, solitary play, and mystical/religious experiences that date back to their early childhood. These "fantasy prone" personalities report (a) multiple psi experiences, (b) and ability to heal others, (c) out-of-body experiences, (d) fantasy of hallucinatory intensity in several sensory modalities, (e) the ability to reach orgasm without physical stimulation, and (f) false pregnancies with abdominal swelling, breast enlargement and termination of menstrual cycle. This sample of "fantasy addicts" (N=27) were all non-patient females and post graduate professionals without notable his-

tories of psychopathology (without psychosis). These women were all selected for very high hypnotic ability with the Stanford Form C and the Creative Imagination Scale (Wilson & Barber, 1978). Wilson and Barber (1983) proposed that the very high hypnotic ability person, the "fantasy prone personality," and the "psychic" are the same person. Lynn and Rhue (1988) wanted to check out this formulation. They started *not* with hypnotic ability but with the ICMI or Inventory of Childhood Memories and Imaginings (designed by Wilson & Barber, 1981) to study fantasy proneness. They screened over 7,000 college students, selecting those with the top 2–4% of scores on the ICMI to be termed "fantasy prone personalities." They were able to generally revise, extend, and partly confirm some of Wilson and Barber's (1983) prior findings. Most importantly, that hypnotic ability (Harvard) and fantasy proneness correlate only .25. Therefore, "fantasy proneness" is not a reliable predictor of hypnotic ability (about 1/3 of non-fantasizers can be classified as high hypnotic ability persons). Hence, excellent hypnotic ability subjects are not identical to "fantasy prone personalities." A minority (20%-35%) of fantasy prone people show signs of psychopathology, but on grade point average and social desirability measures, high "fantasizers" were not different from low "fantasizers" (control group). The high "fantasizers" hallucinations were found to be imperfect and less "real" than Wilson and Barber (1983) implied. Several other studies (Council, Greyson, Huff, & Swett, 1986; Nadon & Kihlstrom, 1987) using the ICMI and a closely related (Rhue & Lynn, 1989) measure of "fantasy proneness," the absorption scale (Tellegen, 1981), have also reported a strong connection between (r=.63-.51) fantasy proneness and subjective psi reporting (see Figures 2 and 3) in large college student samples.

The studies of Wilson and Barber (1983) and Lynn and Rhue (1988) are not strictly comparable because the former started out with very high hypnotic ability people and the latter with high "fantasy prone" people. But one important conclusion to be drawn from the above studies is that *high hypnotic ability people and "fantasy addicts" are not*

Figure 2. Fantasy proneness and subjective PSI.

Inventory of Childhood Memories and Imaginings (ICMI)
Wilson & Barber, 1981
52 item checklist assessing fantasy proneness

Council, Greyson, Huff, & Swett 1986
college students: N = 169

1. ICMI and Psi	r = .63	(p < .001)	N = 169
2. ICMI and Psi	r = .60	(p < .001)	N = 337

Figure 3. Absorption and Subjective PSI.

Absorption: measure of predisposition to become highly involved in sensory and imaginative experiences (like fantasy proneness).

1. Council, Greyson, Huff, & Swett	1986		*APA Washington, DC*
Absorption and Psi	r = .62	(p < .001)	N = 68
Absorption and Psi	r = .62	(p < .001)	N = 336
2. Nadon & Kihlstrom, 1987	*Behavioral and Brain Science*		

N = over 1,000 college students
Absorption and Psi r.51 (p < .001)

necessarily the same people (Rhue & Lynn, 1989). This is not an unexpected conclusion because for over 20 years, factor analytic studies (Hammer, Evans, & Bartlett, 1963; Evans, 1965; Monteiro, MacDonald, & Hilgard, 1980) have shown that standardized measures of hypnotizability are a composite of at least two uncorrelated or orthogonal factors. One factor is the capacity to self-generate fantasy of hallucinatory intensity. This factor correlates approximately .50 with standardized tests of hypnotic ability (e.g. Stanford scales). The second factor is a capacity "to make the mind blank" (Hilgard, 1982) and is related to post-hypnotic amnesia. This second factor appears to be a cognitive and/or sensory inhibition factor.

Clearly the capacity to generate rich fantasy is related to subjective psi verbal reports (Wilson & Barber, 1983; Lynn & Rhue, 1988; Council et al., 1986; Nadon & Kihlstrom, 1987). On the other hand, *objective psi* (empirically verifiable) performance may be related more closely to the second factor in hypnotic ability, the capacity to make the mind blank which is related to post-hypnotic amnesia. Investigations of objective psi should use all appropriate experimental controls for psi and focus on superior hypnotic subjects, selected particularly for superior performance on the second factor (mind blank factor) in hypnotic ability (Wickramasekera, 1991).

I have hypothesized that "unassimilated" subjective psi verbal reports are a risk factor for somatization and psychological symptoms. In the last 14 months in association with John Palmer, we have administered both a pilot *overt* and a *covert* test (Stanford, 1974) of objective psi to all patients seen at the Behavioral Medicine Clinic at Eastern Virginia Medical School. The overt test is given together with our standard battery of psychological, hypnotic (Harvard), and psychophysiological tests (Wickramasekera, 1988). The overt objective test of psi is presented (with a straight face), as a test of "subliminal perception" to patients presenting with somatic complaints like muscular or vascular

headaches, chronic low back pain, hypertension, etc. The cognitive dissonance reduction, desensitization, and role playing required to present such an objective test of psi to a patient with physical complaints in a medical school context, took the current investigator two years of personal work. No patient to date has refused to take the overt test of objective psi. Based on the work of Rex Stanford (1974), we are also concurrently using a covert test of psi designed by John Palmer. Our empirical results with these primitive and probably insensitive instruments (overt and covert tests) are still unanalyzed and at too preliminary a stage to report today.

In conclusion, it may be important to run single blind studies of objective psi, with subjects selected for high belief in psi (sheep) and who are also high on factor 2 (post-hypnotic amnesia) of hypnotic ability. We should also check on the rate of somatization and other psychological symptoms in these highly select subjects (e.g., professional psychics, etc.). It would be useful from a funding point of view to concurrently study the rate of somatization and other psychological symptoms in "sheep," "goats," and those who report "unassimilated" psi experiences. Is the assimilation of "psi" associated with remission of somatic symptoms and reduction in medical expenses (hospital costs, medical tests, drugs, etc.)?

REFERENCES

Barabasz, A. F., & Barabasz, M. (1989). Effects of restricted environmental stimulation: Enhancement of hypnotizability for experimental and chronic pain control. *International Journal of Clinical and Experimental Hypnosis, XXXVII*(3), 217–231.

Barber, T. X. (1969). *Hypnosis: A scientific approach*. New York: Van Nostrand Reinhold.

Council, J. R., Greyson, B., Huff, K. D., & Swett, S. (August, 1986). Fantasy-proneness, hypnotizability, and reports of paranormal experiences. Paper presented at the meeting of the American Psychological Association, Washington, DC.

Council, J. R., Greyson, B., & Huff, K. D. (1987). Fantasy-proneness, hypnotizability, and reports of paranormal experiences.

Council, J. R., & Loge, D. (1988). Hypnotizability and expectancy-induced hallucinations.

Durenberger, D. (1989). Providing mental health care services to Americans. *American Psychologist, 44*, 1293–1297.

Dywan, J., & Bowers, K. (1983). The use of hypnosis to enhance recall. *Science, 22*, 284–285.

Engstrom, D. R. (1976). Hypnotic susceptibility, EEG-alpha and self-regulation. In G. E. Schwartz & D. Shapiro (Eds.), *Consciousness and self-regulation*. New York: Plenum Press.

Evans, F. J. (1965). *The structure of hypnosis: A factor analytic investigation*. Unpublished doctoral dissertation, University of Sydney, Australia.

Hammer, A. G., Evans, F. J., & Bartlett, M. (1963). Factors in hypnosis and suggestion. *Journal of Abnormal and Social Psychology, 67*, 15–23.

Hilgard, E. R. (1965). *Hypnotic susceptibility*. New York: Harcourt, Brace & World.

Hilgard, E. R. (1982). Hypnotic susceptibility and implications for measurement. *International Journal of Clinical and Experimental and Hypnosis, 30*, 394–403.

Honorton, C., & Krippner, S. (1969). Hypnosis and ESP performance: A review of the experimental literature. *Journal of the American Society for Psychical Research, 63*, 214–252.

John, R., Hollander, B., & Perry, C. (1983). Hypnotizability and phobic behavior: Further supporting data. *Journal of Abnormal Psychology, 92*, 390–392.

Katon, W., Kleinman, A., & Rosen, G. (1982). Depression and somatization: A review. *The American Journal of Medicine, 72*, 127–135.

Kihlstrom, J. F. (1985). Hypnosis. *Annual Review of Psychology, 36*, 385–418.

Laurence, J. R. & Perry, C. (1983). Hypnotically created memory among highly hypnotizable subjects. *Science, 222*, 523–524.

Lynn, S. J., & Rhue, J. W. (1988). Fantasy proneness: Hypnosis, developmental antecedents, and psychopathology. *American Psychologist, 43*(1), 35–44.

Monteiro, K. P., MacDonald, H., & Hilgard, E. R. (1980). Imagery, absorption, and hypnosis: A factorial study. *Journal of Mental Imagery, 4*, 63–81.

Morgan, A. H. (1973). The heritability of hypnotic susceptibility in twins. *Journal of Abnormal Psychology, 72*, 55–66.

Morgan, A. H., Hilgard, E. R., & Davert, E. C. (1970). The heritability of hypnotic susceptibility of twins: A preliminary report. *Behavioral Genetics, 1*, 213–224.

Nadon, R. & Kihlstrom, J. F. (in press). Hypnosis, psi, and the psychology of anomalous experience. *Behavioral and Brain Sciences.*

Pena, F. (1963). *Perceptual isolation and hypnotic susceptibility.* Unpublished doctoral dissertation, Washington State University. Pullman, WA.

Quill, T. E. (1985). Somatization disorder. *Journal of the American Medical Association, 254*(21), 3075–3079.

Rao, K. R., & Palmer, J. (1987). The anomaly called psi: Recent research and criticism. *Behavioral and Brain Sciences, 10*(4), 566–643.

Rhue, J. W., & Lynn, S. J. (1989). Fantasy proneness, hypnotizability and absorption—a re-examination. *International Journal of Clinical and Experimental Hypnosis, XXXVII*(2), 100-106.

Richards, D. G. (1989). Hypnotic susceptibility and subjective psychic experiences. Presented at the Annual Convention of the Parapsychological Association, San Diego, August.

Schecter, E. I. (1984). Hypnotic induction vs. control conditions: Illustrating an approach to the evaluation of replicability in parapsychological data. *Journal of the American Society for Psychical Research, 78*, 1–27.

Spiegel, D., Bierre, P., & Rootenburg, J. (1988). Hypnotic alteration of somatosensory perception. Presented at the Annual Meeting of the American Psychiatric Association, Montreal, Canada, May 7–12.

Spiegel, D., Cutcomb, S., Ren, C., & Pribram, K. (1985). Hypnotic hallucination alters evoked potentials. *Journal of Abnormal Psychology , 94*(3), 249–255.

Stam, J. S., McGrath, P. A., Brooke, R. I., & Cosier, F. (1986). Hypnotizability and the treatment of chronic facial pain. *International Journal of Clinical and Experimental Hypnosis, XXXIV*, 3, 182–191.

Stanford, R. G. (1974). An experimentally testable model for spontaneous psi events 1. extrasensory perception. *Journal of the American Society for Psychical Research, 68*, 34–57.

Tellegen, A. (1981). Practicing the two disciplines for relaxation and enlightenment: Comment on "Role of the feedback signal in electromyographic biofeedback: The role of attention" by Qualls and Sheehan. *Journal of Experimental Psychology: General, 110*, 217–226.

Van de Castle, R. L. (1969). The facilitation of ESP through hypnosis. *American Journal of Clinical Hypnosis, 12*(1), 37–56.

Wagner, M. W., & Ratzeburg, F. H. (1987). Hypnotic suggestibility and paranormal belief. *Psychological Reports, 60*, 1069–1070.

Wickramasekera, I. (1969). The effects of sensory restriction on susceptibility to hypnosis:

A hypothesis, some preliminary data, and theoretical speculation. *International Journal of Clinical and Experimental Hypnosis, 17*, 217–224.
Wickramasekera, I. (1970). Effects of sensory restriction on susceptibility to hypnosis: A hypothesis and more preliminary data. *Journal of Abnormal Psychology, 76*, 69–75.
Wickramasekera, I. (1971). Effects of EMG feedback training on susceptibility to hypnosis: Preliminary observations. Proceedings of the 79th Annual Convention of the American Psychological Association, 6(pt. 2), 783–784.
Wickramasekera, I. (1973). Effects of electromyographic feedback on hypnotic susceptibility: More preliminary data. *Journal of Abnormal Psychology, 83*, 74–77.
Wickramasekera, I. (1977). On attempts to modify hypnotic susceptibility: Some psychophysiological procedures and promising directions. *Annals of the New York Academy of Sciences, 296*, 143–153.
Wickramasekera, I. (March, 1979). A model of the patient at high risk for chronic stress related disorders: Do beliefs have biological consequences? Paper presented at the Annual Convention of the Biofeedback Society of America, San Diego, March.
Wickramasekera, I. (August, 1983). A model of people at high risk. Paper presented at the International Stress and Tension Control Society, Brighton, England, August.
Wickramasekera, I. (1986). A model of people at high risk to develop chronic stress-related symptoms: Some predictions. *Professional Psychology: Research and Practice, 17*(5), 437-447.
Wickramasekera, I. (1988). *Clinical behavioral medicine: Some concepts and procedures*. New York: Plenum.
Wickramasekera, I. (1989a). Risk factors for parapsychological verbal reports, hypnotizability and somatic complaints. In B. Shapin & L. Coly (Eds.), *Parapsychology and human nature* (pp. 19–35). New York: Parapsychology Foundation.
Wickramasekera, I. (1989b). Somatizers, the health care system, and collapsing the psychological distance that the somatizer has to travel for help. *Professional Psychology: Research and Practice, 20*(2), 105–111.
Wickramasekera, I. (1991). Model of the relationship between hypnotic ability, psi, and sexuality. *Journal of Parapsychology, 55*, 159–174.
Wilson, S. C., & Barber, T. X. (1978). The Creative Imagination Scale as measure of hypnotic responsiveness: Applications to experimental and clinical hypnosis. *American Journal of Clinical Hypnosis, 20*, 235–249.
Wilson, S. C., & Barber, T. X. (1981). Vivid fantasy and hallucinatory abilities in the life histories of excellent hypnotic subjects ("somnambules"): Preliminary report with female subjects. In E. Klinger (Ed.), *Imagery: Concepts, results, and application* Vol. 2. (pp.133–149). New York: Plenum.
Wilson, S. C., & Barber, T. X. (1983). The fantasy-prone personality: Implications for understanding imagery, hypnosis, and parapsychological phenomena. In A. A. Sheikh (Ed.), *Imagery: Current theory, research, and application* (pp. 340–390). New York: Wiley.

DISCUSSION

HARARY: I haven't seen any evidence that the ganzfeld, either (a) reduces the signal to noise ratio or (b) makes it more possible to use one's psi ability. What I have seen is that ganzfeld tends to make people internalize and free associate more and also influences the way in which they express possible psi information in relation to other kinds of mental noise processes. There is a problem when we look for some unusual

way to bring about psi; a number of experiments are performed and a certain amount of psi is seen, we therefore conclude that this procedure is the thing that brought it about. For example, if you compare the remote viewing results with the ganzfeld results, you'll see excellent results in remote viewing without all the paraphernalia. We could argue about whether people are in an altered state of consciousness while remote viewing; but, they are certainly not hypnotized and they are certainly not in a state of sensory deprivation. They tend to free associate less in their descriptions and be more specific. So rather than seeing one approach causing psi to occur, we tend to see either approach influencing the way in which people respond to psi information, the way in which they express themselves, and how they change that information around in their minds. Also, the idea that people with a certain internal focus also experience subjective psi, is probably true. But then it's a question of investigating the basis for that. That is to say, are you a person who is not familiar with looking at your own internal imagery or paying attention to your feelings? Or are you a person who is prone to thinking about your feelings, paying attention to your ideas, your images, your dreams and so forth? The latter focus might also sensitize you to other creative, perceptual and communicative processes. But it may not be that one part of the process is causing the other. It may be that your general focus brings you in touch with all kinds of things. So, I wonder, don't we have to be careful in how we're defining psi? If we define psi as a certain kind of performance in a certain kind of experiment, we are limiting ourselves to a very narrow definition. Isn't it also possible that we are setting people up to begin with? If we look at the process in a completely different way and don't ask people to be hypnotized, for example, we also find people doing well in psi experiments. Are you saying that you have to be in an altered state of consciousness to function using psi? Are you saying that you have to be a fantasy prone individual? Or, are you saying something else?

WICKRAM: I have no axe to grind. I'm not a parapsychologist and my living and my income are totally unrelated to the rise or fall of psi. I want to make it very clear that I was using only the most prestigious literature. Palmer and Rao replied to the National Science Foundation's critique of psi research. The research that they quote as probably the best objective evidence for psi, is the ganzfeld research. I'm not going to argue about whether that is the best research or not. Palmer and Rao mentioned that research. In the recent review of *Behavioral and Brain Sciences*, Rao and Palmer had an article in which they cite the best evidence, methodologically, of empirical psi as being the ganzfeld

research. Now, I do not know what the mechanism of potentiation of a signal there is and I don't know even if it is a signal. The point is that the empirical results are most impressive for that methodology. Now, I haven't the foggiest idea why that is and I don't even care to speculate on it. But I would point out that if you decompose that procedure, take it apart, it can constitute techniques that are known to potentiate hypnotic ability temporarily. Those are empirical things. You can do it yourself. You don't need to use psi. People who spent time in the South Pole experienced an increase in suggestibility. Astronauts have this same problem. That is partly why they feel nausea—the GI tract is extremely responsive to suggestion.

HARARY: I only want to say that just because researchers have done a lot of experiments of a particular kind and they have gone pretty well, it does not mean that the approach taken in that experiment is the only way to do it. Also, we should not then draw premature conclusions about what it takes to get psi going. Sometimes people draw scientific conclusions for political reasons. Clearly, if we look at the remote viewing work, we are looking at the way the process is affected in different circumstances rather than at something external making it happen.

WICKRAM: I am not familiar with the remote viewing material. I just know that when I was preparing this paper I looked at the literature that was supposedly the most scientifically impeccable literature in the field.

NEPPE: I have several comments and questions, if I may, and I think it's probably easiest subdividing these into the various parts. Much of this comes from what has just been discussed. The first is, you have spoken about measuring subjective paranormal experience. What kinds of parameters are you, in fact, using?

WICKRAM: The scales that I have developed have true-false questions. "Have you experienced telepathy, or precognition?" Things like that.

NEPPE: What kinds of terminology?

WICKRAM: I can refer you to those actual scales if you want. The other scales and the other studies have been independent confirmation of my work. Barber and Wilson developed a scale called the Inventory of Childhood Imaginings and Memories which has numerous questions dealing with paranormal events. Kihlstrom developed a scale which also asks multiple questions, exclusively devoted to a wide range of paranormal. I don't have those scales with me but if you ask me about specific items, I can give you the references.

NEPPE: I am asking this because you have used the term subjective psi experience. As the creator of the term subjective paranormal ex-

perience, in the 1970s, I feel constrained to comment that I would feel far more comfortable if the measures of subjective paranormal experience you were using, were measures which were being used conventionally in mainstream parapsychology. It may be insufficient that people outside the area of parapsychological research have developed questionnaires which they feel are adequate to measure subjective paranormal experience. Additionally you have used the term subjective psi experience but I have tended to shift away from the term subjective *psi* experience because the term *psi* implies a certain objectivity in the context of phenomena which one can not fully conceive. I would suggest that you use the term paranormal instead of psi. I wonder how adequately one can measure these psi phenomena. Based on my experience many people do not understand terms like "precognitive." They do not understand many terms that we use formally. We need to translate them down to their levels.

WICKRAM: If you look at the items on the scale that Kihlstrom designed, the one that I used, we ask questions like,"How often have you been able to predict a telephone call or the death of someone before it occurred?" We don't talk about precognitive or telepathic.

NEPPE: Obviously not. What I'm trying to get at is the fact that you may find it useful using more conventional kinds of measures which have been used within the literature. For example, Palmer and Dennis developed an excellent measure in that regard.

WICKRAM: Three of these studies that replicated my work used the Palmer scale. Kihlstrom, Wagner, and Ratzeburg used it so there have been independent confirmations with traditional measures.

NEPPE: The second thing is, in my original work, I used a three level scale which I'll just mention to you. The first level consisted of two very broad screens because I was trying to get people who either had no subjective paranormal experiences or had had a large number. So, the first screen was just something sent through the mail asking people about their psychic experiences and how they rank those experiences. One was looking for a rank of no psychic abilities and no psychic experiences, and the other extreme included rankings of moderate or profound "psychic" abilities and large numbers of psychic experiences. Once one had these initial subgroups, they were then exposed to telephone interviews where they were given a broad screen of nine fundamental kinds of subjective paranormal experience. Then, they were given a detailed questionnaire. One therefore had three internal built-in mechanisms trying to, if not validate, at least replicate, ones original findings within that population. One finds that people misinterpret phenomena in a variety of different ways particularly in heterogeneous

groups and one has got to be extremely careful interpreting what subjects regard as "psi." It is probably worthwhile, therefore, even couching ones terms, saying that patients with high hypnotic ability *claim* a larger number of subjective paranormal experiences than those without high hypnotic or with low hypnotic ability, as opposed to saying they do or don't have it. That aspect, as we heard yesterday, is just an expression of the extent of significance to which they are attributing their particular kinds of experiences. I think this whole framework is something that is of enormous relevance and enormous importance. The other component which is the emphasis of what Dr. Harary was saying, is the translation from subjectivity to objectivity, where the two may not correlate very well at all.

WICKRAM: I'm not clear what you mean by that.

NEPPE: When one talks about people who claim large numbers of subjective paranormal experiences, this might not mean that in any kind of objective test of psi, be it formally in a lab, or informally in some kind of field or spontaneous framework, these people will necessarily exhibit more psi than those who do not. This is something that has to be proven.

WICKRAM: We can get as meticulous as we need to, but the point is, as long as we remain at the level of verbal report data, the field could be dismissed as a function of psychopathology or a function of hypnotic ability. Until we can actually show objective psi (i.e. things that are empirically verifiable) we cannot begin to get into what might considered paradigm shift. The data that I presented to you simply demonstrates a correlation between verbal report and a personality characteristic. That is very fruitful. For example, if you want to find tigers, it would be nice to have a tiger detector. How do you identify tigers? What part of the woods do they exist in? So, you can identify the organisms that generate these verbal reports at a high frequency or a high density. Whether there is any validity to their verbal reports is a totally separate issue and that remains for future investigation. In my opinion, the objective issue, if psi exists independent of the reports of it, is a separate issue.

WEST: What I want to ask is a follow-up on the last question. As I understand it, what is being said is that one factor in hypnotizability is what is being called fantasy addiction, being able to self-generate hallucinations and also having control over physiological functions, so that when one imagines an illness, this can be accompanied by actual physical and measurable changes. This is a personality attribute of some people and is correlated with reporting a lot of psychic experiences. Now, it seems that the speaker is suggesting that these subjective experiences,

because they occur in these fantasy prone personalities, may perhaps have no objective basis at all, may be purely imaginary. I would suggest that that is an assumption. It may be that the people who are capable of producing these fantasies and producing these physical changes are also the same people who are capable of having objective psi experiences. It need not always be imaginary.

Now, my second point is that we are told that there are two factors in hypnotizability, the other factor being ability to make the mind blank. It is suggested that making the mind blank may be associated, not with subjective or imaginary psi, but with an objective psi ability. We have heard that objective tests have been given to these people to see if they have demonstrable psi ability in an experimental situation. We don't know what the results of that are yet. Now the question I would like to ask is, what is the reason for assuming that there is a connection between objective psi and the ability to make the mind blank? This seems to be a hypothesis which came out of the blue. I would like to know the reason behind this hypothesis?

WICKRAM: You understood my first point correctly. Subjective psi, in terms of my research and the independent replication of other people, seems to be related to subjective psi reporting. There is another component in hypnotic ability, which is orthogonal to the hallucinatory component, and that is the ability to make the mind blank—the ability, in terms of actual items on the Stanford or Harvard scales correlated with amnesia. The notion that that may be correlated with objective psi is something we are currently testing. Why it might be correlated with objective psi, or why we think it may have a relationship is totally speculatory. The basis for speculation is that if either external noise or internal noise interferes with a signal that is very weak, then, maybe the ability to put aside all preconceptions or all contrary beliefs may improve the reception of a weak signal. That is purely speculation, I have no empirical evidence to support that. I do know that in the yogi literature, literature on the samadhi state, they talk about a "blank mind" as an important factor in the ability to not just do psychophysiological tricks, but also to do things like levitate and the other kinds of supposed claims of paranormal experiences.

WEST: I believe that in fact, you do have a little more than pure speculation to go on, because you have argued that hypnotic ability and hypnotic states are psi conducive. I know that Keith Harary has challenged that, but it has certainly been suggested in the literature. Now, if hypnotic ability is correlated with objective psi, it is also correlated with fantasy productions, but you don't believe that fantasy productions are correlated with real psi. Then, there must be some

other factor in hypnotic ability which makes for real psi. Since you have another factor, then you have a reason for looking at that other factor. So, maybe you have more reason behind your hypothesis than you have explained.

WICKRAM: Thank you. The important point is to see what the data suggests. For example, fantasy proneness has been shown to correlate .25 with measured hypnotic ability. It is not a perfect correlation. There is much variance that is unaccounted for. So, we don't know until we see what the empirical results are. It is possible that the fantasy factor in hypnotic ability is only *modestly* correlated with empirically verifiable psi but that the cognitive-memory inhibition factor in hypnotic ability is strongly correlated with empirically verifiable psi.