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Dear Ian:

Thank you for sending me your interesting reprint on "Risk factors leading to chronic stress-related symptoms." You have collected an amazing range of references, and your own experience has been very rich.

I have had too little clinical experience with cases such as you treat to add much from my own experience with respect to the correlation of hypnotic responsiveness scores and kinds of psychophysiological symptoms. The correlations between scale scores and personality measures have been very low, but your own hypotheses suggest why this could be so, because if both lows and highs develop symptoms, and mediums do not, then low correlations would be expected unless scores were defined as departures from the median, without regard to sign. I can conceive of this as something that might be tested, even though the symptoms of highs and lows differ.

You are a bit uncritical in your generalizations from the literature, particularly from reports given at meetings that have not been published and so are not subject to critical review. I don't mean to be picky, but hypnotic research is pretty difficult to replicate in many instances.

I have no fault to find with your observations, except that I noted that you slipped in describing the two cases whose profiles are given in Figures 3 and 5. The 'slip' is a minor one, but possibly worth calling to your attention. The Bakan and Gur findings, although some of the work was done in my laboratory, were very weak, and done while they were postdoctoral visitors, so that I had little to do with them. The relationships were not strong enough to justify using them as part of a test battery.

It seems to me that your two exhibits to which I have referred point this out. For the one who scored 12 on the Harvard scale (Figure 3) scored about 2 on the CLEM, just as did the one who scored 2 on the Harvard (Figure 5). Instead of ignoring the CLEM in both cases, it is ignored in the first case, but is said to be "confirmatory" in the second case. The issue is, Why was it not "nonconfirming" in the first case? The true situation is that the correlation is not high enough to justify its use in the individual case.

The Harvard Group Scale is OK as a preliminary scoring device in picking subjects for large-scale studies, but for individual (including clinical) use, ought to be supported by a better scale, such as the SHSS-C. A change of a few points on the Harvard scale is too frequent to count very much on it in the individual case. (I am referring to its research use. Many clinical purposes can be served by very much abbreviated scales.)
While I find your predictions interesting, they appear to be mostly arguing in one direction, that is, from the symptoms of patients to their hypnotizability, while the predictions are in the opposite direction: from their hypnotizability to their symptoms. This is a big leap. For example, high hypnotizables are high in imagery, but it does not follow that all who are high in imagery are high hypnotizables. All correlations are too low. The Tellegen-Atkinson correlations are high enough to be interesting from a theoretical point of view, but they are too low to be of much help in the measurement of hypnotic response.

I take it that you do some testing on non-patient samples to confirm the results on patients. It is difficult to test your predictions in that way, because many of them will not produce the kinds of symptoms that will bring them to treatment. Still you have enough suggestions of sensory and physiological responses to produce some sort of personality battery that could be used to test your hypotheses on a nonpatient group. I know how easy it is to tell a busy and responsible person to do something else. If I still had a laboratory, I would propose to do it, but my lab was closed in 1979, after I was already retired for 10 years, and Stanford psychologists showed no inclination to keep it going.

You are doing interesting and important work, and I wish you well in your next steps.

Sincerely,

Ernest R. Hilgard, Ph.D.