Commentary

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In “Hypnosis, Hypnotizability, and Treatment” the author poses two basic questions: (1) what is “hypnosis” and (2) what is the relationship between hypnotic responsiveness and clinical outcomes? With regards to these two questions the author suggests that “many contemporary clinicians and researchers without defining what they are doing, are doing something. These measurements may be reliable, but their significance is questionable” (p. 60). I have no evidence to suggest that the first part of this statement is accurate or not, nor does the author offer any evidential support for this claim. Sutcher’s observation that reliability says essentially nothing about validity is trivially true.

Sutcher claims there is not agreed upon definition of hypnosis. However, Division 30 of the American Psychological Association provides a fairly comprehensive definition of hypnosis which may vary based on the purpose and goals of the clinical or research endeavor. At least the committee who developed the definition had some agreement. The author then raises the “state” debate in reference to hypnotic responsiveness. Ongoing research in this area of study will either likely refine empirical support for a state theory (c.f. de Pascalis, 1999; Gruzelier & Brow, 1985; Lubar, Gordon, Harris, Nash, Mann, & Lacy, 1991; Ranville, Hofbauer, Bushnell, Duncan, & Price, 1999) or move the field in a different direction. With respect to the latter point, I would be more inclined to agree with Chaves (1997) that while the state debate has been useful in the development of research questions and techniques etc., the maturity of cognitive sciences may provide an entirely different and more scientifically useful perspective from which to consider hypnosis rendering the “state v. non-state” debate less central and/or relevant to the field. Based on the aforementioned points, the author’s initial focus on “what is hypnosis, it’s definition, the state debate, the work of Mesmer, Braid, and Charcot, et al. etc., is not particularly relevant to the question of hypnotic responsiveness and clinical outcome.
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Sutcher points out (via case anecdote) that response to a hypnosis suggestibility scale says little about that same individual’s response to the clinical context. However, why should those who are high responders as measured by any hypnosis scale (e.g., as SHSS, HIP, SHCS, BSS, etc.) predict a successful clinical outcome? The reverse is equally true; why should low responders to the aforementioned scales necessarily predict an unsuccessful clinical outcome? The author correctly observes that high reliability (i.e., test-retest, internal consistency) says nothing about construct validity. The technical adequacy of any given hypnosis scale only speaks to the issue of test-retest reliability, internal consistency and some aspect of validity (as measured by concurrent and/or predictive validity). The issue of construct validity is an entirely different question.

However, I see no basis to think that hypnotic responsiveness would be a legitimate predictor of clinical outcomes any more than would the Wechsler Adult Intelligence Scale (WAIS). I think the author has asked the wrong question or conflated hypnotic responsiveness with psychotherapy.

There is a large body of empirical evidence regarding the prediction of successful clinical outcomes in psychotherapy such as work in motivational interviewing (Miller & Rollnick, 2002), expectancy theory (Kirsch, 1999); use of Axis II to predict psychotherapy outcomes, (Ogrodniczuk & Piper, 2001); generalized expectancies for negative mood regulation (Cantanzaro & Mears, 2004), use of relational measures for psychotherapy process and outcome (Gurtman, 2004), to identify but some of the plethora of psychotherapy outcome research. The point being, that hypnotic responsiveness is not necessarily a relevant factor in the clinical context.

With regard to the four case studies presented by the author, the reader has no way of evaluating if any of the four clients experienced hypnosis as the author did not make clear what procedure (e.g., how long was the session, what type of suggestions were given, etc.) was used to elicit hypnotic responsiveness. Nor was there any attempt to assess the depth of the client’s hypnotic response. What the author presented were four anecdotal reports of his clinical intervention. As Stanley and Campbell (1963) have noted, causal inference in one-shot case studies is not possible. Minimally these case studies seem to reflect good clinician/client rapport, good client motivation to produce a behavioral change, in a high demand clinical context. In his discussion, the author comes to the obvious conclusion that hypnotizability is not necessarily a relevant construct for psychotherapy. Who said it was?

References


