A Response to the Commentaries on Hypnosis, Hypnotizability and Treatment

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Abstract
I respond the various commentaries about my paper entitled, “Hypnosis, Hypnotizability and Treatment.” I attempt to further clarify the discussion about my original three questions: 1) What is Hypnosis? 2) Who gets Hypnotized? and 3) What can you do with Hypnosis? Comments about the four case studies I presented in my original paper are addressed and evaluated. I also respond to specific comments made by each of the eight commentators. Hopefully, this discussion will promote clarification of the issues which are dividing the field of professional hypnosis so that a generally accepted, scientifically validated definition of hypnosis can be developed.

I was surprised by the diversity of comments on my paper, “Hypnosis, Hypnotizability, and Treatment” (Sutcher, 2008). I thank all of the authors who provided commentary and wish to respond to many of their statements. I would like to begin by making some general comments based on my three original questions (What is Hypnosis? Who gets Hypnotized? What can you do with Hypnosis?), my four case studies and then make specific comments about points made by the authors of the commentaries.

What is Hypnosis?
My main point was that there is no generally accepted scientific definition of hypnosis. Fortunately, that has not stopped us from studying it scientifically or utilizing it professionally (clinical and forensic work). I tried to illustrate some of the problems that have promoted this lack of agreement in the hope of clarification. One commentator seems to agree with my characterization (Handel, 2008) while another declines to comment (Kessler, 2008, p. 157) because of his lack of a broad “conceptual base” on the issue.
Response to Commentaries

Two of the commentators proposed that there are “different trance” states (Barretta & Baretta, 2008, p. 167) but did not describe, the nature of these alleged differences or what they all have in common to be considered “hypnotic.”

Geary (2008, p. 170) seems to have ignored the question altogether but goes on to use words or phrases like “hypnotherapy” or “application of hypnotic principles.” I do not believe that hypnosis is a specific type of therapy and another commentator seems to agree with my belief (i.e., Spiegel, 2008). Nevertheless, if you can’t define hypnosis, logically you cannot call any type of therapy “hypnotic” or speak of “hypnotic principles” because there is no way to distinguish between hypnotic and non-hypnotic therapies or principles.

Another commentator (Goodman, 2008, p. 171) uses the word “traditional” hypnosis. I don’t know what this is or how it differs from non-traditional hypnosis. I do know that whatever hypnosis is, it is not science if one cannot describe its nature and measure it.

Lynn and his co-authors (2008, p. 164) seem to believe that there is no such thing as “hypnosis” since they propose that “waking” suggestibility is strongly associated with “hypnotic” suggestibility. I do not understand their need to call suggestibility “hypnotic” if they believe there is no distinction between “waking” and “hypnotic” suggestibility.

Matthews (2008) proposed that there is a generally agreed upon definition of hypnosis and uses the Division 30 (Society for Psychological Hypnosis of the American Psychological Association) definition as an example. But he seems to have ignored that a recent issue of the American Journal of Clinical Hypnosis was devoted to a variety of commentaries about what was wrong with this definition. Not surprisingly, the definition of hypnosis posted on the American Society of Clinical Hypnosis (ASCH) website is different than the Division 30 definition. Indeed, the definition of hypnosis posted on the ASCH website is different than the definition proposed by the ASCH committee who developed guideline for using hypnosis to influence memory (Hammond, Garver, Mutter, Crasilneck, Frischholz, Gravitz, Hibler, Olson, Schefflin, Spiegel & Wester, 1995). If this is what Matthews calls agreement, I wonder what he would consider to be disagreement.

Finally, Spiegel (2008) asserts that there is a generally accepted definition of hypnosis; his definition. While I may not be familiar with all the literature on hypnosis, I do think I am correct in observing that Spiegel’s definition has not achieved general acceptance as the evidence discussed above seems to indicate.

While this discussion has furthered our understanding of the nature of our disagreements, it still has not produced a generally agreed upon definition of hypnosis. Is this goal even attainable?

Who gets Hypnotized?

My main point was that there are wide individual differences in responsivity to whatever hypnosis is and they can be reliably measured by a variety of assessment procedures. But there are different types of reliability. Nevertheless, most of these procedures have empirically demonstrated internal consistency among scale items, excellent levels of inter-rater agreement, and high levels of test-retest reliability. This is science and the study of individual differences in response to hypnosis should be promoting a better scientific understanding of what hypnosis is and general agreement about how to define it. Surprisingly, this has not happened and the study of individual differences in responsivity to whatever hypnosis is may actually be promoting greater disagreement.

For example, while scores derived from these various assessment procedures are reliable, the correlations between a score derived from one test and a score from a different
test (concurrent validity), while usually statistically significant, are still not high enough to characterize the different test scores as interchangeable measures. In other words, the various hypnotic assessment procedures in common use today are not measuring the same thing. While there is some communality, it is obvious that significant differences also exist. I used the example of a study published by Frischholz and his colleagues (Frischholz, Lipman, Braun \& Sachs, 1992) which found a correlation of .60 between the Induction Score (IND) of the Hypnotic Induction Profile (HIP) and the Stanford Form C scale in a clinical sample. Nevertheless, IND scores significantly discriminated patients suffering from a schizophrenic disorder from the “normal” college comparison sample (discriminatory validity). But scores on the Stanford Form C scale did not significantly discriminate between the schizophrenics and normals. Obviously, scores on the HIP and Stanford Form C scale, while significantly intercorrelated, are nevertheless measuring different things as well.

The Barretta’s (2008) and Goodman (2008) seem to imply that everyone is hypnotizable. But that is not what the science of hypnotizability assessment indicates. In contrast, the remaining six commentators appear to accept that “reliable differences do exist” (e.g., Geary, 2008, p.169).

Several of the commentators (Geary, 2008; Lynn, Boycheva \& Barnes, 2008; Mathews, 2008; and Spiegel, 2008) remarked about the “state versus non-state debate” (see Geary, 2008, p. 169). This debate centers on whether or not hypnotized subjects are entering a “hypnotic trance” state which is qualitatively different from the subject’s normal “waking” state of consciousness. I briefly remarked on this issue when I acknowledged the survey findings reported by Christensen (2005). The survey says that respondents believed that exposure to a hypnotic induction ceremony produces a unique hypnotic “trance” state by a ratio of 4:1. But this is not scientific evidence any better than the opinions of four million Frenchmen.

Furthermore, logic alone dictates that if can’t define “hypnosis”, you can’t scientifically define a “hypnotic” trance state. And if you accept the fact that there are reliable differences in responsivity to whatever hypnosis is, then you also accept the fact that some people are just not hypnotizable (Hilgard, 1965; Spiegel \& Spiegel, 1978). By definition, non-hypnotizable subjects cannot enter “hypnotic” trance states no matter how many “hypnotic” inductions they are exposed to.

Finally, there is the great debate about what is the best label to use in describing individual differences in responsivity to whatever hypnosis is. The Barretta’s (2008) have no need for such a label because they consider everyone hypnotizable. Geary (2008), Handel (2008) and Spiegel (2008) use the term “hypnotizability” in their commentaries while Goodman (2008) seems to use “hypnotizability” and “susceptibility” interchangeably. But Handel (2008, p. 174) also states that Crasilneck (and others) assert “the greater the perceived need (urgency), the greater the hypnotic response.” But if we accept that some subjects are not hypnotizable (as scientific evidence clearly demonstrates; Hilgard, 1965: Spiegel \& Spiegel, 1978), then even the “perceived need” or “urgency” is not going to produce a “hypnotic” response.

Kessler (2008) uses the terms “capacity,” “hypnotic capacity,” “imaginative capacity,” “hypnotizability,” “hypnotic responsiveness” and “hypnotic involvement.” Lynn, Boycheva and Barnes (2008) use the term “hypnotic suggestibility,” but claim it’s just about the same thing as “waking suggestibility.” Lynn, Boycheva \& Barnes (2008) also cite a paper by Braffman and Kirsch (1999) where an argument is made for using the term “imaginative suggestibility.” Can you imagine the American Society of Clinical Imaginative Suggestibility? Matthews (2008) uses the terms “hypnotic responsiveness” and “hypnosis suggestibility scale.” Surprisingly, not one of the commentators considered the great “label” poll by Christensen (2005) where the most preferred terms were either “hypnotic responsivity” or “hypnotizability.” And the beat goes on!
Response to Commentaries

What can you do with Hypnosis?

Perhaps, I should have more clearly stated my question as “What can you do with a “hypnotizability” score?” I acknowledged at the outset that hypnotizability scores are good predictors of positive treatment outcome for both “hypnotic” and “non-hypnotic” treatments for pain. But what else? I also acknowledged that a variety of different variables (other than “hypnotizability scores” contribute to positive treatment outcomes) and my interpretation of the various commentaries is that each commentator agrees with me. I am confused by those commentators who have tried to enlighten me about this issue (Geary, 2008; Goodman, 2008; Handel, 2008; Kessler, 2008; Lynn, Boycheva & Barnes, 2008; Matthews, 2008; and Spiegel, 2008) when it is one of three points on which we all seem to agree.

Case Studies

I acknowledge that the case studies I presented were selected to illustrate two points: 1) patients who demonstrate high levels of hypnotizability do not necessarily profit from hypnosis; and 2) patients who demonstrate even very low levels of hypnotizability can still profit from treatments which incorporate hypnosis. Again, my interpretation of the commentaries is that all the commentators also agree with me on these two points.

Case 1 (Diana)

I assessed Diana to be a virtuoso hypnotic subject based on the fact she easily exhibited every hypnotic behavior I could think of suggesting. Given that this evidence indicates she was highly hypnotizable, Dr. Spiegel may be right that I wasted 4 minutes and 40 seconds time on a hypnotic induction that he could have been done in 20 seconds (Spiegel, 2008). Nevertheless, the patient did respond positively to her dental treatment anxiety, hypnotically suggested analgesia, and control of her post-extraction bleeding. I am sorry that Dr. Goodman “would have preferred” to have bleeding time measurements (Goodman, 2008, p. 171). But what would this information tell us about hypnosis? I acknowledged that I do not know what exactly caused the positive response to her post-extraction bleeding (Sutcher, 2008, p. 62). What was interesting about this case is that it showed positive treatment response without specific hypnotic suggestion for some symptoms but also that the patient needed specific suggestions to successfully resolve other symptoms.

Case 2 (Tammy)

I selected the case of Tammy to illustrate that a patient who does not appear to be hypnotizable still profited from a technique I had read about which purported to induce a hypnotic trance through the use of tension. I concluded that Tammy was not hypnotizable based on my observation of her lack of response to a variety of hypnotic induction techniques utilized by different practitioners skilled in the use of clinical hypnosis. I am sorry that Dr. Spiegel does not think that this was sufficient evidence of her lack of hypnotizability.

Tammy had not responded to other medical treatments for her symptoms over a 6 month period. Nevertheless, Tammy initially experienced a reduction in her previous uncontrolled coughs and sneezes after being exposed to my trance-tension improvisation. Later, these symptoms gradually disappeared without further treatment. This fact mitigates Dr. Goodman’s concern that perhaps she might have been suffering from “possible Tourette’s Syndrome” or “Petit Mal Epilepsy” (Goodman, 2008, p. 171). These syndromes usually require extensive medical and psychiatric treatment. Instead, a brief exposure to a trance-tension technique was followed by initial symptom mitigation and later complete symptom extinction. And
again, I don’t know why this happened, but I’ll bet Tammy is glad it did.

**Case 3 (Lilith)**

I selected the case of Lilith as another demonstration that a patient who does not seem to respond to a hypnotic induction can still respond positively to a treatment for her smoking addiction. I disagree with the Barretta’s assertion that Lilith was in any kind of “trance” state. She didn’t think so and I didn’t think so. Again, I am sorry that Dr. Spiegel (Spiegel, 2008) is concerned that I have not provided sufficient objective evidence (i.e., a zero or low hypnotizability score) that Lilith was not hypnotizable. I sure tried my best to hypnotize her.

Dr. Handel’s hypothesis that Lilith’s positive treatment response may have been caused by either “expectancy” or “compliance” (Handel, 2008, p. 174) is pure speculation just as Dr. Spiegel’s hypothesis (Spiegel, 2008, p. 150) that Lilith experienced “spontaneous hypnosis” (whatever that is). I don’t know what happened to help her stop smoking nor did Lilith. But she did stop smoking.

**Case 4 (Aphrodite)**

I found the case of Aphrodite (of course that’s not her real name!) to be interesting for several reasons. First, she earned almost perfect scores on the Spiegel Eye Roll Sign (3/4) and HIP IND score (9/10). I am glad that Dr. Spiegel is gratified by this data (Spiegel, 2008). Aphrodite was not surprised to learn that she was highly hypnotizable. She knew it for years; just didn’t know that it was called “hypnosis.”

Second, Aphrodite experienced an initial positive treatment response (i.e., she experienced no taste of cigarettes for a couple of days) which gradually withered over time. I respectfully disagree with Dr. Spiegel’s speculation (Spiegel, 2008) that her ultimate negative response to treatment was based on bad treatment (i.e., focusing on something negative rather than something positive). I believe Aphrodite’s confession that she just didn’t really want to quit smoking. Dr. Lynn and his colleagues (Green & Lynn, 2000; Lynn, Boycheva & Barnes, 2008) claim that there is empirical evidence which supports my hypothesis that Aphrodite was not sufficiently motivated to quit smoking. Who knows for sure what happened in this particular case? Not me.

Third, I’m sure Dr. Spiegel was impressed that I didn’t waste any time inducing hypnosis for pain control with someone who earned almost perfect scores on his hypnotizability assessment procedures.

Fourth, I disagree with the Barretta’s assertion (Barretta and Barretta, 2008) that Aphrodite called me because of an unconscious awareness. I’m pretty sure she consciously decided to call me because she believed my earlier hypnotic treatment of her smoking addiction caused her to experience no taste of her cigarettes. But who knows for sure?

Fifth, I believe that Aphrodite’s almost immediate response to my hypnotic intervention for pain control was probably based on her high hypnotizability. I have already acknowledged that scientific evidence does exist to support the predictive validity of hypnotizability assessments for positive responses to both hypnotic and non-hypnotic psychological treatments for pain control (Sutcher, 2008, p. 61).

**Response to Specific Comments**

I would also like to respond to some of the specific comments made by different commentators which I have not addressed above.

*Barretta’s*
Response to Commentaries

I thank the Barretta’s (Barretta & Barretta, 2008, p. 167) for their complimentary observation that I have a sense of humor. I don’t know if that’s true, but people have been laughing at what I say almost all my life. However, I want to assure the Barretta’s that other dentists also have a sense of humor (e.g., read Dr. Goodman’s commentary (Goodman, 2008).

**Geary**

I believe Dr. Geary’s observation that “clinicians do not have the benefit of suspending judgment while the process of empirical study unfolds through the testing of hypotheses and revision of theoretical conjectures” (Geary, 2008) is both prescient and correct.

However, his subsequent dismissal of the value of hypnotizability assessment is disappointing. I asked a question about how hypnotizability assessment can inform the clinician on how to best treat the patient. I did not say such assessment was “worthless” (Goodman, 2008, p. 171). I said it appears to be “irrelevant” in some cases and presented four case studies to support my assertion.

**Goodman**

I agree with Dr. Goodman that when you see what you are doing with a particular patient isn’t working, then “just switch techniques” (Goodman, 2008, p.171). This is precisely what I did with the case of Tammy. I observed other clinicians using “hypnotic” techniques which were not working (although I would have tried them first as well) and “switched” my approach in order to serve up the “hypnotic cocktail du jour” (Goodman, 2008, p. 171). My observation of what wasn’t working can be characterized as a form of assessment. I saw what wasn’t working so I tried a different approach. Whether or not it produced the subsequent symptom reduction/extinction is something I’ll never know. But it has helped me to figure out what does and does not work with particular types of clients with a particular symptom presentation.

**Handel**

Dr. Handel need not be concerned that I unconsciously looked “back over a series of cases (or over a career’s worth of cases as Sutcher attempts to do) and attempt to dissect or analyze the response to any one approach” (Handel, 2008, p. 173). I deliberately selected the cases I presented as clinical evidence for my argument. To not look back at one’s cases in order to understand what worked or didn’t work seems like clinical negligence. My observations or conclusions may be flawed but they are not negligent. I believe that thoughtful reflection about one’s work is the best way to avoid having something evolve “willy-nilly…like Topsy” (Handel, 2008, p.175; Sutcher, 2008).

**Kessler**

Dr. Kessler’s seems to have misunderstood my paper (Kessler, 2008). I certainly didn’t treat my patients willy nilly. I tried to carefully assess what their problems were and designed my interventions on the basis of my training, reading of the scientific literature, and clinical experience.

While I try to conceptualize what I am doing during treatment I realize that a positive treatment response does not validate my hypothesis. I believe we can only determine “what is or isn’t by ‘data derived from scientific investigations’” (Kessler, 2008, p. 157).

I cannot be held responsible if others misinterpret my paper. I’m not saying, “We don’t need no stinkin’ assessment” (Kessler, 2008, p. 158). We need to know when hypnotizability assessment is relevant and what it is relevant for.
Lynn, Boycheva & Barnes

Lynn, Boycheva & Barnes (2008, p. 161) assert that “anecdotal observations can generate useful hypotheses; however, they often have limited evidential value and are no substitute for well-controlled studies with relatively large numbers of patients.” What nonsense! They must be connected with some University where it is possible to do such research. While I value scientific investigation (which I have repeatedly said), I also think it is important to learn from your own mistakes and those of others so you don’t repeat them. Dr. Lynn and his colleagues ought to read Geary’s (2008) quote which I cited above several times so they can understand how real clinicians operate.

I never said (or implied) that hypnotizability assessment was “completely irrelevant to outcome” as Lynn, Boycheva & Barnes assert (Lynn, Boycheva & Barnes, 2008). I noted its relevance in predicting outcome for pain control treatment. I also noted how it was not relevant in my four cases studies. But I did not learn anything new about how hypnotizability assessment is relevant in all cases from any information provided in their commentary.

Matthews

Matthews (2008, p. 154) states, “I see no basis to think that hypnotic responsiveness would be a legitimate predictor of clinical outcomes any more than it would be the Wechsler Adult Intelligence Scale (WAIS).” I guess he has never worked with brain damaged patients. Any assessment procedure must be good for something. Otherwise, why would we waste time doing it?

Spiegel

Dr. Spiegel (2008) asserts that hypnosis can at least be partially defined as the ability to concentrate and pay attention. Does that mean that since I am concentrating and paying attention to what he has written that I have been hypnotized? Dr. Spiegel (2008)) writes about what is necessary for “trance” to occur and then cites his own work as proof. This may be proof of something, but that something has nothing to do with hypnosis.

References

Response to Commentaries


