

Reply to Wagstaff: “Hypnosis and the relationship between trance, suggestion, expectancy, and depth: Some semantic and conceptual issues”

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Abstract

Wagstaff (2010) reviews and comments on two recent papers by Pekala et al. (2010a, 2010b), concluding that “many of the problems relating to the definition and conceptualization of terms associated with hypnosis . . . may stem from insufficient attention to the role of suggestion and expectancies in producing hypnotic phenomena, and an over-reliance on the role of the procedures and mechanics of the induction process” (p. 47). Although I agree with his semantic and conceptual focus, I believe that a number of these problems are due to not operationally defining terms such as hypnosis, hypnotic state, or trance in a comprehensive phenomenological manner. By using the PCI (Phenomenology of Consciousness Inventory) via retrospective phenomenological assessment, and using a phenomenological state instrument like the PCI-HAP (Phenomenology of Consciousness Inventory - Hypnotic Assessment Procedure) to obtain a state measure of hypnotic responsiveness, a means is available to define and empirically address some of these issues in a way that can significantly further our understanding of the nature of hypnotism. Such an approach might also address Kallio and Revonsuo’s (2005) admonition concerning the need to develop “an internally coherent and widely shared theoretical vocabulary” (p. 51) to better understand consciousness, altered states of consciousness, and related phenomena, such as hypnosis/hypnotism.

Keywords: Altered states of consciousness, consciousness, expectancy, hypnosis, hypnotic depth, hypnotism, psychophenomenology, suggestibility, trance.

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I am pleased to be asked to respond to the erudite commentary by Wagstaff (2010) on the two papers by Pekala, Kumar, Maurer, Elliott-Carter, Moon, and Mullen (2010a, 2010b). I had hoped that the methodology reported in those papers would help to generate discussion among members of the hypnosis research and clinical communities concerning trance, suggestion, expectancy, and hypnotic depth, and this seems to be the case with the present commentary and that of Terhune and Cardeña (2010). Before responding to Wagstaff's comments and interpretations, I want to reiterate Wagstaff's concern with semantics, i.e. "the study of meaning in language, especially with regard to historical changes" (Webster II New Riverside Desk Dictionary, 1988, p. 378).

The Methodology

In developing the methodology described in Pekala et al. (2010b, 2010b), care was taken to operationally define certain variables, such as hypnoidal state, imagoic suggestibility, and self-reported hypnotic depth, for example, in rather specific ways. This was done so as to not only be able to better understand "hypnosis" and "hypnotism" from a clinical and research perspective within the parlance of current theorizing and research, but so that these variables could also be defined and measured by the Phenomenology of Consciousness Inventory¹ (PCI: Pekala, 1982, 1991b) and the Phenomenology of Consciousness Inventory – Hypnotic Assessment Procedure¹ (PCI-HAP: Pekala 1995a, 1995b). As a result, these definitions may be somewhat different from how they were used in the past, as Wagstaff has demonstrated. However, I believe that only by operationally defining these variables so that they can be empirically assessed will we be able to avoid the semantic confusion of the past.

It is also important to be aware of the implicit assumptions, both that of Wagstaff and myself, which may "color" one's perception of specific phenomena. Consequently, I would like to reply to one of Wagstaff's concluding comments first: "rather than considering the idea of hypnosis as some sort of trance state that has an existence independent of, or in addition to, suggestions, imagination and beliefs/expectancies, they might do better to consider altered state experiences in contexts defined as hypnosis as very much a *product* or *outcome* of such processes (Wagstaff, 1998)" (Wagstaff, 2010, p. 56). This statement implies an important assumption underlying Wagstaff's perspective concerning hypnosis and hypnotism.

The regression analyses described in Pekala et al. (2010b) make no assumptions concerning which of the variables used in the regression equation to predict self-reported hypnotic depth may be products or outcomes and/or which variables may be causally antecedent. As we wrote in Pekala et al. (2010b), when "'the goal is to maximize R²' and determine the best subset of independent variables 'to predict the criterion to a high degree of accuracy' (Newton & Rudestam, 1999, p. 253)" (Pekala et al., 2010b, p. 310), then step-wise regression analysis is the multiple regression procedure to use.

A major assumption underlying the undersigned's current approach was delineated in a keynote presentation to the Society for Clinical and Experimental Hypnosis in 2009:

Epistemology precedes metaphysics.² What you know is a function of how you know (what you know). [There are different levels of knowing, and] different levels of knowing and analysis are needed to understand a given phenomena. [Just as] neurophysiological analyses are needed to understand the neurophysiological basis of the mind, [and] behavioral and psychological analyses are needed to understand the behavioral and psychological basis of the mind, phenomenological analyses are needed

to understand the phenomenological basis of the mind. Reducing one level to another level does not necessarily “explain” the phenomenon in question. Cross-pollination amongst levels will lead to a better understanding of the mind or consciousness than analysis at only one particular level. (Pekala, 2009a, slide 17)

The present methodology, as delineated in Pekala (1980, 1985, 1991a); Pekala and Cardeña (2000); Pekala and Kumar (2000, 2007); and Pekala et al. (2010a, 2010b) adds comprehensive phenomenological analysis to the behavioral, psychological, and neurophysiological levels of analysis for understanding hypnosis/hypnotism. Orne (1977) wrote some time ago: “though it is necessary to specify responses in behavioral terms, it should be emphasized that the resulting scores validly reflect the hypnotic process only to the degree that the behavior reflects alterations in the individual’s subjective experience” (p. 19). I posit that there is no way to specifically and accurately determine if “the behavior reflects alterations in the individual’s subjective experience” unless there is a means to reliably, validly, and comprehensively assess that subjective or phenomenological experience.

The problem of reliably and validly measuring subjective experience in reference to hypnosis has been cited before. De Groh (1989) over 20 years ago suggested that inconsistencies in the literature concerning imagery vividness and hypnotizability may be partially “attributed to the inherent ‘imprecision’ of trying to quantify subjective experiences” (p. 44). Sheehan echoed similar observations ten years earlier (1979) when he said that studies of hypnosis “are especially open to the problem of quantifying subjective experience” (p. 404).

A psychophenomenological approach to quantifying subjective or phenomenological consciousness in general and hypnotism in particular (Pekala, 1980, 1985, 1991b; Pekala & Kumar, 2000, 2007; Pekala & Levine, 1981, 1982; Pekala & Wenger, 1983) has been developed. It continues a phenomenological tradition in hypnosis research as espoused by Fromm (Fromm, Oberlander & Gruenewald, 1970), Shor (1979), Sheehan and McConkey (1982), and “interactive-phenomenological approaches” to hypnotism (Banyai, 1991; McConkey, 1991; Nadon, Laurence and Perry, 1991; Sheehan, 1991), as delineated in Lynn & Rhue’s *Theories of Hypnosis* (1991). However, I believe this approach assesses subjective experience in a more comprehensive fashion (see Pekala, 1991a; Pekala, 2009a) than has been previously done.

The methodology can help us gain a better understanding of the nature of hypnosis and hypnotism. Instead of assuming that someone may be in a ‘hypnotic state’: “that hypnotic suggestibility (responsiveness to suggestions whilst in the *hypnotic state* [italics added] can be used as a measure of the level of hypnotic *susceptibility* (now known as hypnotizability)” (Wagstaff, 2010, p. 52), the methodology allows the clinician and researcher to actually estimate “hypnotic state,” as defined by the hypnoidal state score. The aforementioned quote highlights the problem with theorizing about “hypnotic state” or trance, without having a specific means to operationally define and measure it. As we wrote in Pekala et al. (2010a):

The question as to how much of hypnotic behavior is due to an “altered or trance state of consciousness” is a controversy that has raged for decades. Lynn and Kirsch (2006) have succinctly summarized the controversy. State theorists, like Hilgard (1977), Bowers (1992), Kihlstrom (2003), Woody (Woody & Bowers, 1994), Gruzelier (1996), and Tart (1979), generally

espouse some variant of altered state effects or a “special process” (Spanos, 1982) . . . In contrast, nonstate theorists, like Sarbin (1950), Barber (1969), Spanos (1991), Lynn (1997), and Kirsch (1991), suggest that “subjects’ beliefs, expectations, and imaginings about hypnosis, and their interpretations of the suggestions of the hypnotist, are sufficient. . . . *A major problem concerning the theorizing of both of these groups of theorists/researchers is that they are inferring or alluding to altered state or special process effects, but such effects are not well defined* [italics added]. (Pekala et al., 2010a, p. 278)

Hence, without operationally defining terms such as “trance,” or “hypnotic state,” it becomes difficult, if not impossible, to scientifically test such assumptions, as Kallio and Revonsuo have written:

If such phenomena as ‘hypnosis,’ ‘consciousness,’ or ‘altered state of consciousness’ exist at all, then for science to describe and explain them coherently, surely the relevant research community in psychology and cognitive neuroscience should aim at developing an internally coherent and widely shared theoretical vocabulary to make genuine progress in their scientific explanation. (2005, p. 51)

Without operationalizing these phenomenological concepts, one can only make *assumptions* about such concepts, assumptions which may or may not be true. As an example, Wagstaff has written: “As originally conceived, therefore, the distinction between hypnotic depth reports and responsiveness to hypnotic suggestions concerns a distinction between two ways of measuring the same thing, hypnotic susceptibility or hypnotizability construed as the capacity to enter *the hypnotic state*” [italics added] (p. 52). But what is such a “hypnotic state,” and why *construe* it if one can estimate it?

I submit that some of the semantic confusion in the past and continuing into the present, as Wagstaff has well documented, concerns making surmises and conjectures concerning such a hypnotic state. A methodology has been developed and presented in the literature (Forbes & Pekala, 1993; Pekala, 1991b, 2002; Pekala & Kumar, 1984, 1987, 2000, 2007) that allows for concepts like hypnosis or trance to be defined and estimated.³ To estimate “hypnotic trance” or what Weitzenhoffer (2002) has called “hypnosis,” a phenomenological measure called the hypnoidal state score, also known as the predicted Harvard Group Scale (pHGS) score, was defined and developed:

Operationally, if we can define trance as the subjective state the highly hypnotizable person achieves in response to a hypnotic induction, then this subjective state can be assessed and described in quantitative terms. Although there may be different types of trance (possibly based on individual differences factors and dependent on the qualitative nature of that trance), there may at the same time be some commonality across these different types of trance, analogous to Spearman’s (1904, 1923) “g” factor for general mental ability, vis-à-vis different types of intelligence, a la Gardner (1983). (Pekala & Kumar, 2000, p. 111)

Based on regression analyses using the PCI (sub)dimensions to predict the total scale score from the Harvard, we generated a predicted Harvard Group Scale (pHGS) score (Forbes & Pekala, 1993; Pekala & Kumar, 1984, 1987) which correlated about .60 with the actual total Harvard Group Scale scores. As we wrote 20 years ago (Pekala & Nagler, 1989), we thought it premature to label the state associated with a high pHGS score a “hypnotic” state, because we did not know if experiencing all the phenomenological parameters that would generate a high pHGS score would be associated with hypnotic effects. In other words, although the regression equation allows one to determine the average phenomenological parameters associated with being in a “hypnotic state,” it does not follow that experiencing such phenomenological effects would be associated with being able to experience classic hypnotic behavioral and cognitive effects:

Hence, we prefer to use the adjective *hypnoidal* to refer to phenomenological experience congruent with what high susceptibles, on the average, would endorse during a hypnotic induction. Since a state is “any well defined condition or property that can be recognized if it occurs again” (Ashby, 1963, p. 17), we define a hypnoidal state as the state, delineated by a regression equation using the PCI (Pekala & Kumar, 1987) that is associated with what high susceptibles would report on the PCI during a short (eyes closed), sitting-quietly interval during the induction of the Harvard Group Scale. (Pekala & Nagler, 1989, p. 232)

The PCI regression analyses thus allow us to generate a measure of such a “hypnoidal state,” a phenomenological construct that we can now use to see how this construct varies with behavioral, cognitive, and neurophysiological levels of analysis.

Levels of Analysis

Reducing, or equating, one level of analysis to another may not be especially edifying. For example, I disagree when Wagstaff (2010) writes: “Once we conceive of both the hypnoidal score and the HGSHS as essentially attempting to tap the same thing, depth of hypnosis or depth of trance achieved (true hypnotic susceptibility or hypnotizability), Pekala et al.’s validation procedure makes sense” (p. 53). The hypnoidal state score and the HGSHS total score are not “essentially attempting to tap the same thing.” The hypnoidal state score is tapping a phenomenological process (during a sitting quietly period embedded in a hypnotic induction) associated with, and predicted from, the cognitive-behavioral items associated with the Harvard total score (Forbes & Pekala, 1993; Pekala & Kumar, 1984, 1987). Although the hypnoidal state score was nomothetically derived from the Harvard; idiographically, the phenomenological (as measured by the PCI) versus the cognitive-behavioral level of analysis (as measured by the Harvard total score), may be quite different when looking at the individual participant (as I will illustrate below).

Additionally, I agree with Wagstaff that it is quite appropriate to semantically question how those definitions are so defined, as a prerequisite to determining if they may be reliable and/or valid. As an example, I feel that “making awkward and somewhat arbitrary semantic distinctions, such as the distinction between ‘hypnosis’ and ‘hypnotism,’ on the basis of whether ‘suggestion’ is deemed to be present or not” (Wagstaff, 2010, p. 56), is neither awkward nor arbitrary. It was Weitzenhoffer (2002), one of the developers of the Stanford scales (Weitzenhoffer & Hilgard, 1959, 1962), who made such a distinction.

Additionally, the chapter, “The hypnotic state,” in his book, *The practice of hypnotism: Volume 1: Traditional and semi-traditional techniques and phenomenology*” (1989a) is an excellent review of “hypnosis” and its relationship to trance. Nevertheless, it is quite appropriate to debate such semantic distinctions.

If one admits that phenomenological analysis is a legitimate level of analysis for understanding human behavior and experience, then questions related to the epiphenomenality of “trance” and/or its causal relationship to suggestibility, expectancy, etc., become questions that can now be more easily empirically investigated. It may be the case that “hypnoidal state” is only an explanatory construct that is statistically a function of the regression equations previously cited (Pekala et al. (2010b), and not causally related to hypnotic suggestibility or hypnotizability. But these are questions that can now be empirically addressed because the technology is available to do so.

The Nature of Hypnotism

However, because phenomenological analysis can give us a more comprehensive methodology to assess mind or consciousness and its relationship to hypnosis/hypnotism, this does not mean that we should not take a very close look at semantics and the various conceptualizations and presuppositions concerning hypnotic processes, as Wagstaff has done. A good example concerns the Gandhi and Oakely (2005) study which Wagstaff cites:

in which participants were given identical relaxation/induction procedures; however, for one group the procedure was labeled as ‘hypnosis’ and the other ‘relaxation.’ The results showed significant increases in suggestibility (responses to suggestions) on behavioural and experiential measures, and also perception of involuntariness when the induction was labeled as ‘hypnosis.’ There were, however, no significant increases in these measures when the identical induction was labeled as ‘relaxation.’ (Wagstaff, 2010, p. 54)

As Wagstaff reported, “other studies also support the view that the label ‘hypnosis’ is the main factor accounting for the increased responsiveness to suggestions” (2010, p. 54), highlighting the importance of the “suggestion” of doing hypnosis, as related to the results obtained. I would agree that the “suggestion” of doing hypnosis had a significant influence. But I would also interject that it may be the “positive expectancy” (Kirsch, 1985, 2000) associated with the term, “hypnosis,” that helped mediate those results.

As an example, I had a client suffering from what is now called Dissociative Identity Disorder, previously called Multiple Personality Disorder, with whom I worked for a number of years. Her husband was quite fearful that if I used “hypnosis” with her, I would instill “false memories.” I told the client that I would not do “hypnosis” with her for that reason. However, via relaxation and visualization I was able to integrate and fuse alters, during which time she would have hallucinatory imagery of the integrations and fusions. The procedures I would do with her were identical to what I would do when using “hypnosis” with someone suffering from DID, but I substituted “visualization” for “hypnosis” with this particular client. I never did give this client a PCI-HAP so I do not know if “trance” (hypnoidal) state effects were related to my success with her. However, I believe her positive expectancy and her hallucinatory visual imagery (high imagoic suggestibility) were crucial determinants in her successful recovery, along with the empathic therapeutic rapport that was established.

I admit that I was reluctant to embrace Weitzenhoffer’s (2002) distinction between “hypnosis” and “hypnotism,” because almost everyone uses the word “hypnosis,” and few

use “hypnotism.” However, I think the distinction that he made a very important one, i.e.: “I will otherwise generally reserve the term *hypnosis* for the state, and the term, *hypnotism*, for the production, study and use of suggestion with the state of hypnosis presumably being present, whether or not it adds anything tangible to the situation” (p. 210).⁴

Just because the term hypnotism is not commonplace, does not mean that the term may not be an appropriate one when looking at hypnotic phenomena. Additionally, one needs to acknowledge the use of such terms in reference to their historical context, and hence there is no need to redefine Bernheim (1889), Hull (1933), Charcot (see Pintar & Lynn, 2008, pp 77-80), or others’ references to hypnosis as that of hypnotism, since they were using the term, hypnosis, in reference to the accepted terminology of the time.

However, this does make for some controversy when different theorists use the same words to mean different things. That is why my collaborators and I went to some length to operationalize and quantify these concepts specifically as a function of the PCI and the PCI-HAP. Hence these concepts need to be acknowledged within that context, and with an understanding of how to administer and interpret the instrument. (See the administrative and interpretative manuals for the PCI-HAP.)¹ Additionally, disputation can occur as to whether the concepts are appropriately defined, but that will have to wait for another commentary.

I also agree with Wagstaff that measures of hypnotic depth are not usually done retrospectively: “Virtually all standard measures of hypnotic depth involve reporting hypnotic depth *during* the hypnosis procedure as this is assumed to be the most accurate measure of the depth of hypnosis” (Wagstaff, 2010, p. 49). However, the self-reported hypnotic depth (srHD) score was conceptualized to be a very important measure clinically, i.e., “If the client believes that they were not hypnotized, what are the odds that they will practice self-hypnosis, or be open to hypnotic interventions?” (Pekala, 2009b, p. 19). As such, this measure needs to be taken at the end of the intervention so the clinician can then determine, in collaboration with the client, how he or she may pursue hypnotism or self-hypnosis training further.

Additionally, although I also agree with Wagstaff that the srHD score is similar to the Long Stanford Scale of Hypnotic Depth (LSS), “which is one of the most widely used measures of hypnotic depth in the literature” (Wagstaff, 2010, p. 49), the distribution of the LSS is not normal, but positively skewed. The srHD score is less skewed than the LSS (Pekala, Maurer, & Kumar, 2008), and hence may be easier to utilize for statistical analysis purposes than the LSS.

Hypnotic Depth, Trance, and the Hypnoidal State Score

I appreciate the semantic analysis that Wagstaff has done to elucidate the meanings behind the concepts discussed in the section on “Hypnosis, depth, suggestibility, and hypnotizability” (p. 50). It is important to know “where we have been.” As an example, Wagstaff states that in “the literature, the term ‘hypnotic depth’ tends to be used synonymously with ‘depth of trance’ (Bowers, 1983; Tart, 1966, 1970, 1979; Wagstaff et al., 2008)” (Wagstaff, 2010, p. 51). However, the PCI-HAP makes an important conceptual and operational distinction between these two terms.

‘Hypnotic depth’ is operationalized as ‘self-reported hypnotic depth’: “On a ‘1’ to ‘10’ scale how hypnotized do you feel that you became. Let ‘1’ = not hypnotized at all, and let ‘10’ = the most hypnotized that you can imagine” (Pekala, Kumar, and Maurer, 2009, p. 20). This, as we said in our papers (Pekala et al., 2010a, 2010b), is an attribution, we believe, that the participant makes when trying to globally estimate how hypnotized they felt they were after the hypnotic deinduction.

On the other hand, the hypnoidal state score was operationalized as a function of the unstandardized regression coefficients multiplied by the PCI (sub)dimensions for the 10 (sub)dimensions that were found, via regression analysis, to predict the total Harvard Group Scale score (Pekala & Kumar, 1984, 1987). It is a phenomenological measure of the state of a person's mind at the time of assessment for the stimulus period (eyes closed sitting quietly) assessed. We feel it has some construct validity as we pointed out in Pekala et al. (2010a), although replication and validation by other researchers is needed.

Wagstaff writes concerning the hypnoidal state score: "like most of the standardized scales for assessing hypnotic susceptibility or hypnotizability, it confounds the effects of the induction per se, with responsiveness to suggestions per se" (Wagstaff, 2010, p. 52-53). Any induction ceremony that includes suggestions is going to do this, and why should it not? This is how traditional hypnotic inductions are typically choreographed (Lynn, Rhue, & Kirsch, 2010; Weitzenhoffer, 1989a, b). Measuring the content of this particular phenomenological level is not going to tell us *how* or *why* the person thought or imagined what they thought or imagined, i.e. whether it may have been due to suggestions, relaxation, expectancy, etc. Rather phenomenological analysis can only tell us *what* the client or research participant thought or imagined, and with what intensity. Here we have to distinguish between the "what" versus the "how" or "why" concerning phenomenological assessment.⁵

Obviously, how people come to feel they have alterations in consciousness (the PCI altered state of awareness dimension) or a loss of volitional control (the PCI volitional control dimension), may be a function of the relaxation/deepening procedure, the suggestions that were passed or failed (i.e., a person having a hallucinatory dream will probably feel quite different from a person who only has a thought about that dream), and/or that person's neurobiological constitution, which may make it easier for him or her to have alterations in awareness/consciousness (Oakley & Halligan, 2010). Because of its phenomenological focus, the correlation or covariation of phenomenological effects with "the effects of the induction per se, with responsiveness to suggestions per se," etc., may well likely be a function of several influences.

However, assessing the phenomenological nature of consciousness as a subjective or "first person" (Chalmers, 2007) phenomena may be quite important in and of itself. Besides using this approach as a means to begin to address the "hard problem of consciousness" (Chalmers, 1995), it allows for human subjective experience to be more "finely differentiated" due to the strong *quantitative* nature of the approach. As an example, the two concepts of self-reported hypnotic depth and hypnoidal state, although significantly correlated ($r = .48$, $p < .001$, Pekala et al., 2010b, p. 298), may be very different for different people, and it is important to be able to make this distinction because this may help us better understand the subjective experience of our clients and our research participants.

As an example, I had a client who obtained an adjusted hypnoidal state percentile score from the PCI-HAP Excel scoring program (Pekala, Maurer, and Ott, 2009) of 95, but her self-reported hypnotic depth percentile score was only 20. Interestingly, although this client felt she was "not hypnotized," her depth of "trance" (hypnoidal state) score was quite high.⁶ Although this person might not normally have wanted to use hypnotism ("I wasn't hypnotized, Doc", so she said), we have, to date, recorded for her three different self-hypnosis protocols to help her with her concerns.

The PCI-HAP administration was helpful to her, allowing her to realize that her "trance (hypnoidal) state" was in the top 5% of individuals in the data base, even though she felt she was only mildly hypnotized per her self-reported hypnotic depth score. (I believe she felt this because her imagery vividness during the hypnotic dream was quite low and she

very easily opened her eyes during the eye catalepsy item). This “hypnoidal state” information, I feel, changed her *expectation* of herself from only “mildly hypnotizable” to having a significant ability related to one of the several “domains” of hypnosis/hypnotism (Brown & Fromm, 1986; Wagstaff, 1981).

Concerning theoretical orientation, embracing the theorizing of Barber (2000) and especially Holroyd (2003) as a model for better understanding hypnosis/hypnotism does lead to hypnotism being defined in a particular way, i.e. “a product of imagination and suggestibility, altered state effects, and expectancy” (Wagstaff, 2010, p. 49). Because no model is complete; whatever model one adopts will be held hostage to the constraints and limitations of that model. I believe the usefulness of the model will ultimately reside in how well that model may best explain phenomena, its ability to stimulate additional theorizing and research, and to what extent other theorists and researchers adopt that model in their writings and research. Once the semantics associated with those models have been debated (as we are doing now) and accepted, rejected, and/or modified (only time will tell), then such models can be evaluated in terms of how they may help us better understand a particular phenomenon of interest.

I am quite appreciative of that fact that Wagstaff reported: “These papers make a very important contribution to the literature in a number of ways” (2010, p. 48). I agree that the approach may be useful for evaluating several conceptual issues, such as the “relaxation and generic trance experiences” (Wagstaff, 2010, p. 53) to which Wagstaff addressed with considerable acumen. I believe that “trance” is a more general concept than “hypnosis.” Additionally, although hypnosis may be conceptualized as a subset or type of trance; trance is not necessarily a subset or type of hypnosis.

In our “trance” paper a decade ago (Pekala & Kumar, 2000), we wrote:

Weitzenhoffer, in his seminal volume *The Practice of Hypnotism* (1989a), pointed out that “there has been a tendency since Braid, but particularly in modern times, to use the term, *trance*, as synonymous with hypnosis” (p. 298). Yet the term *trance* “appears to be a much broader concept than hypnosis; its usage is very old and antedates the latter. One of the earliest uses of the word *trance* in English language can be found in a 1386 work of Chaucer” (p. 298).” (Pekala & Kumar, 2000, p. 108)

The reader is referred to this earlier paper, since it lays the theoretical and conceptual groundwork concerning the nature of “trance” for the Pekala et al. (2010a, 2010b) papers. In that paper we talk about the need to develop “a general, but operational, measure of trance” (Pekala & Kumar, 2000, p. 111). We also go on to make distinctions concerning qualitative differences in trance, and generate “trance typology profiles,” looking at how “trance” experiences may vary across different types of low to high hypnotizables. In that paper, we define “trance as the subjective state the highly hypnotizable person achieves in response to a hypnotic induction” (Pekala & Kumar, 2000, p. 111), because we were hoping to operationalize the concept, “trance,” as clinicians use the term when talking about hypnosis and hypnotism as “being in trance, “ or “being in a hypnotic state”: “as being a sleeplike, or a half awake, half asleep state. Decreased sensitivity or responsiveness to external stimuli, including a total lack of it, is usually considered a characteristic feature, as is a shift from voluntary to automatic activity” (Weitzenhoffer, 1989a, p. 298).

With that said, as Wagstaff has indicated, procedures such as systematic relaxation, autogenics training and meditation have commonalities with “hypnosis.” In addition, “generic trance experiences” can lead to the “impression that *any* altered state to which we might apply the term ‘trance’ is, in fact, hypnosis” (1010, p. 53). I agree, and I believe this confusion stems partly from the way “hypnosis” has been defined in the past, as Wagstaff has meticulously documented. As a clinician, I have attended many ASCH (American Society of Clinical Hypnosis) and SCEH (Society for Clinical and Experimental Hypnosis) workshops wherein the instructor

has said that the client was “in hypnosis” or “in trance” because . . . ; usually citing behavioral signs such as increased lacrimation, slowness to open ones eyes after deinduction, lethargy, etc. The hypnoidal state score allows us to obtain an estimate of “being in hypnosis” or “being in trance,” as seen within the context of the aforementioned research. I feel it a reliable and valid measure, based on the research to date, and one consistent with the literature concerning “trance” and “hypnosis,” (see Pekala et al., 2010a). However, it is a concept in need of much further replication and validation. Whether “being in hypnosis” is causally, or etiologically, related to enacting various hypnotic suggestions is another issue entirely, but, via this approach, we have a means to begin to empirically address this issue, since we are using a phenomenological algorithm to quantify a phenomenological process.

To more easily illustrate how phenomenologically quantifying such effects may be useful, Figure 1 (taken from Pekala & Forbes, 1988) shows the hypnoidal state scores associated with several relaxation strategies (hypnosis,⁷ progressive relaxation and deep abdominal breathing) and a baseline eyes closed condition, as defined as a function of four hypnotizability groups (low, low-medium, high-medium, and high - as measured by the Harvard). What is interesting here is that the hypnoidal effects for progressive relaxation versus hypnosis are not significantly different for low-medium, high-medium, and high susceptible.

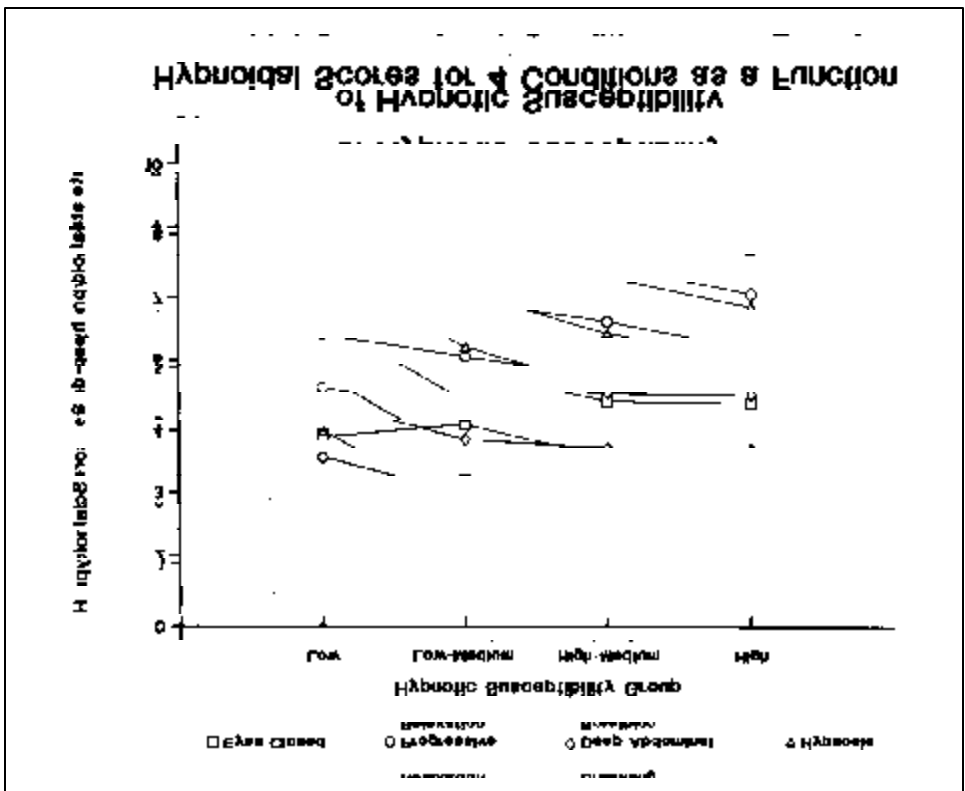


Figure 1: [Taken with permission from: Pekala, R. J., & Forbes, E. (1988). Hypnoidal effects associated with several stress management strategies. *Australian Journal of Clinical and Experimental Hypnosis*, 16, p. 127.]

However, there is a significant difference between hypnosis and progressive relaxation for lows. Lows actually have significantly greater hypnoidal effects for progressive relaxation than hypnosis, and hypnosis for lows is no different than eyes closed sitting quietly concerning the hypnoidal state score. Pending replication, this significant interaction supports the high risk model of Wickramasekera (1989), who suggests using biofeedback and ostensibly non-hypnotic relaxation strategies with low hypnotizables (who respond better to the self-control motif associated with biofeedback). This research hence supports the clinical fact that hypnotism may not be useful for everyone and highlights the need for a hypnotic assessment (Barnier & Council, 2010).

Another way of illustrating the importance of phenomenological analysis in understanding hypnotism is to review the subjective experiences of low susceptibles during the Harvard. Figure 2.

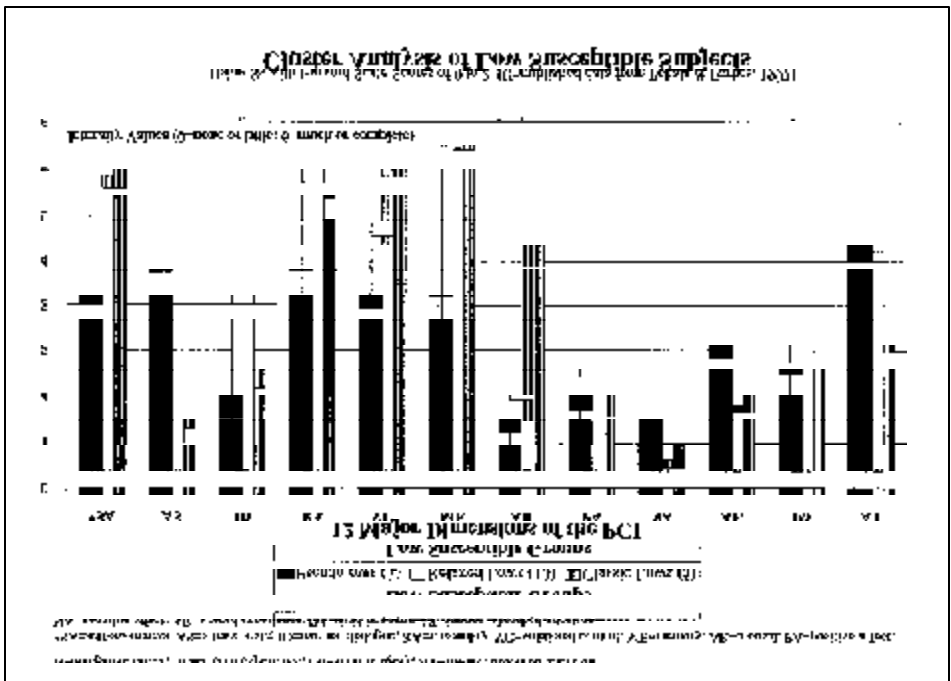


Figure 2: [Taken with permission from: Pekala, R. J., & Kumar, V. K.. (2000). Operationalizing “trance” I: Rationale and research using a psychophenomenological approach. *American Journal of Clinical Hypnosis*, 43, p. 118.]

Taken from Pekala & Kumar (2000, p. 118), shows a cluster analysis of low susceptible participants who scored 0 to 2 on the Harvard. What is quite interesting here is that there are a group of participants, labeled “pseudolows,” who have phenomenological experiences consistent with being moderately hypnotizable (i.e., drops in self-awareness, volitional control, and memory, and increased alterations in altered state of awareness), even though behaviorally (as assessed by the Harvard) they appear to be, at most, only mildly hypnotizable (scores of 2 or less). Using only the Harvard will assign a client to a particular hypnotizability level based on the participant’s score, but their subjective experience may be quite different.

Hence, this approach allows for the assumptions that Wagstaff reviews to be empirically addressed. As an example, on page 52 he wrote:

It was further assumed that the deeper one goes into this state the more suggestible one becomes (Bowers, 1983; Tart, 1966; Weitzenhoffer, 1953), hence, by measuring participants' responsiveness to a set of suggestions that increase in difficulty, one can determine their level of hypnotic depth (Davis & Husband, 1931; Friedlander & Sarbin, 1938; Orne & O'Connell, 1967; Sheehan & McConkey, 1982)." (2010, p. 52)

These assumptions were made because, at the time of that theorizing, it was not possible to measure depth of trance via a measure such as the hypnoidal state score. When we do that, the data suggest that the higher the number of items passed on the Harvard, when averaging across a group of participants, the greater the hypnoidal state effects. However, there are individual differences and some participants do not have high hypnoidal state scores in spite of the passage of particular items. Different processes or abilities (Woody, Barnier, & McConkey, 2005) may be involved in the passage of different types of items. Additionally, some items may be passed due to high expectancy for some individuals, high imagoic suggestibility for others, and/or alterations in state of consciousness for still others (Holroyd, 2003). Finally, the "qualitative" nature as to the type of "trance" experienced, can vary substantially per given score on the Harvard (see Table 3, page 122 from Pekala and Kumar, 2000).

Wagstaff concludes his commentary by making the suggestion that we should construe the concept of trance or "hypnosis *as a species or subcategory of suggestion*; i.e. hypnosis can be considered fundamentally as a type of suggestion or instruction that one is about to enter or has entered a special state or condition, usually identified with hypersuggestibility" (p. 56). One may construe trance or "hypnosis" as a type of suggestion, just as one may construe some suggestions as types of expectancy modifications (Kirsch, 2000). How one *comes to be in a "state of hypnosis" or "trance,"* via suggestions or deepening routines or expectancy effects, etc., however, is a different issue from the nature of, the "qualia"⁸ (Van Gulick, 2007) of, the subjective or phenomenological experience being assessed.

No doubt, the etiological route concerning how such qualia occur is an important issue to address. However, it is a different issue from the importance of assessing and including such qualia in better understanding and utilizing hypnotism. Whether one considers ones *hypnoidal state* to be a product of some type of extended suggestive process, the result of expectancy modifications, or neurophysiological parameters (Oakley & Halligan, 2010), the fact remains that this particular level of analysis gives us a means to measure phenomenological consciousness, which appears to be associated with some of the *relative* variance associated with ones self-reported hypnotic depth (Pekala et al., 2010b; Pekala & Maurer, 2010). (See also the subsection of Weitzenhoffer, 1989a. pp. 353-359: "Hypnotism without hypnosis: The social-psychological view" for a discussion of a somewhat similar point of view, but from a different perspective, concerning this issue.)

In practical terms, I recently recorded a self-hypnosis protocol for a client who obtained percentile scores on the PCI-HAP across the following 4 major domains: (hypnoidal state: 76th percentile; imagoic suggestibility: 100th percentile; average total expectancy: 91st percentile; and self-reported hypnotic depth: 89th percentile). Her hypnotic responsivity index (HRI),⁹ the average of the four scores, was the 89th percentile. With the client, I was able to have her experience herself (during hypnotism) as the female protagonist, Leigh Anne, from the movie, *The Blind Side*. She has found this self-hypnosis protocol useful in helping her become more assertive in her male dominated work environment. She reported that when she imagines herself as Leigh Anne during the sitting quietly periods embedded in the

protocol (as she rehearses herself as the assertive Leigh Anne during several vignettes drawn from the movie), she actually “becomes” Leigh Anne while still retaining her own identity and reports that she *talks in a southern accent* - which she found quite amusing! I find in my private practice individuals who have such high domain scores, especially those with high scores on both imagaic suggestibility and hypnoidal state, are an especially interesting group with whom to work.

Limitations

This psychophenomenological methodology is not without its limitations (Pekala, 1980; 1991b; Pekala & Cardeña, 2000; Pekala & Kumar, 2000, 2007). By comprehensively assessing this phenomenological level of hypnosis/hypnotism, one has a better idea as to which phenomenological processes have been activated during the sitting quietly period embedded in the hypnotic induction for which the PCI was completed. However, because one’s level of trance or hypnotic depth can vary during a hypnotic induction and is probably a function of several variables, it is unknown to what extent the hypnoidal state score or the PCI (sub)dimensions scores would vary had a different time period during the hypnotic induction been assessed. Additionally, completing the PCI in reference to a 2-minute sitting quietly period during the Harvard will presumably be quite different from completion of the PCI in reference to the hypnotic dream item or the eye catalepsy item, due to the *principle of stimulus state specificity*:

across groups of randomly selected individuals, the same behaviors in the same stimulus settings (the same stimulus conditions) will be associated with the same intensities and patterns of phenomenological experience (the same phenomenological state), while different stimulus conditions will be associated with different intensities and/or patterns of phenomenological experience. (Pekala & Wenger, 1983 p. 255)

This principle was posited to allow for valid comparisons between various stimulus conditions and variations in phenomenological experience associated with those stimulus conditions (such as baseline conditions like eyes opened and eyes closed sitting quietly and other conditions, i.e., reading erotica, relaxation/meditation, etc., Pekala, 1980). The reader is referred to Pekala (1991a) for a review of support for this principle, and how this principle may modulate the usefulness of this approach for phenomenologically quantifying consciousness.¹⁰

Additionally, because of the fleeting and highly variable nature of consciousness (Angell, 1907; Titchener, 1898; Boring, 1921, 1953), the phenomenological variability across individuals and across varying types of stimulus conditions may be significant. Earlier research (Pekala, 1980; Pekala & Levine, 1981, 1982; Pekala & Wenger, 1983) suggested that that variability appears dependent upon the instructional set, the context, and the nature of the stimulus condition targeted. Hence, much more *basic* research on retrospective phenomenological assessment (Pekala, 1991a), and its use with baseline (eyes open and eyes closed sitting quietly) and other types of stimulus conditions still needs to be addressed. To date, the PCI has been used in reference to such stimulus conditions (other than hypnotism) as: meditation (Venkatesh, Raju, Shivani, Tompkins, & Meti, 1997), fire-walking (Hillig, & Holroyd, 1997/98; Pekala & Ersek, 1992/93), an OBE within an NDE (Maitz & Pekala, 1991), shamanistic trances (Rock, Wilson, Johnston, & Levesque, 2008), religious/spiritual narratives

(Wildman & McNamara, 2010), a virtual reality environment (Huang, Himle & Alessi, 2000), drumming (Maurer Kumar, Woodside, & Pekala, 1997), and psi phenomena associated with “shamanic-like journeying” (Rock & Storm, 2010).

In summary, the nature of ones subjective experience, as the functionalists (Angel, 1907; James, 1890/1950) have documented a century ago, are quite dependent upon the instructional, contextual, and procedural effects associated with the stimulus condition for which subjective experience is assessed. Slightly changing the instructional set/stimulus environment may significantly change the phenomenology that is reported. These are all important areas that will need to be addressed in future research.

Conclusions

The aforementioned approach allows for the phenomenological level of mind to be quantified and then compared with other levels of traditional cognitive-behavioral and neurophysiological analyses. In turn, this methodology may offer a better understanding of not only hypnosis and hypnotism, but possibly also other types of phenomena, such as states and altered states of consciousness, as Kallio and Ronvonsuo (2003, 2005) have advocated. Just as semantic analysis can help us better understand our past and present controversies concerning hypnosis/hypnotism and related issues, phenomenological analysis can help us address present and future controversies concerning some of these issues. Wagstaff (2010) has allowed us to better conceptualize some the major controversies concerning hypnotizability, susceptibility, and hypnosis that have occurred over past decades. Hopefully, the aforementioned methodology will invite further dialogue and help to generate more comprehensive theorizing and research (Terhune & Cardeña, 2010) concerning the enigma of hypnosis and hypnotism.

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Footnotes

This reply is in reference to comments to two published papers that are partially based on two \$5,000 grants received from the Veterans Administration Stars and Stripes (VISN4) Healthcare Network. The author wishes to thank Mr. Ron Maurer for his helpful comments on an earlier version of this manuscript. The contents of this presentation do not represent the views of the Department of Veterans Affairs nor the United States Government.

¹Copies of the PCI (Pekala, 1982, 1991b), the PCI-HAP (Pekala, 1995a, 1995b), the therapist and self-report pre- and post-assessment forms, the administration (Pekala, Kumar, & Maurer, 2009b) and interpretative (Pekala, 2009b) manuals, and the EXCEL scoring program (Pekala, Maurer, & Ott, 2010) are available at www.quantifyingconsciousness.com.

²This statement summarizes the philosophical position that the nature of metaphysical reality is constrained by how we come to know and understand that reality; traceable, in part, to Immanuel Kant's "Copernican revolution" and his *Critique of Pure Reason* (Kant, 1781/1966).

³Some of this semantic confusion was addressed in a paper published 10 years ago by V. K. Kumar and myself (2000) wherein we reviewed the rationale and research behind this psychophenomenological approach for operationalizing trance, and also types of trance, via the hypnoidal state score and trance typology profiles. The reader is referred to that earlier paper for some of the conceptual groundwork for this approach. Since knowledge "proceeds from the general to the specific (Heidegger, 1927/62)" (Pekala & Kumar, 2000, p. 111), we first used a generally accepted criterion behavioral measure, the Harvard Scale, to generate a phenomenological measure correlated with it, to have a "starting point" concerning an empirical measure for a phenomenological basis of trance.

⁴I will herein make the distinction between hypnosis and hypnotism, as did Weitzenhoffer (2002).

⁵"How" a person arrives at rating themselves as low on volitional control, high on altered state or rationality, and moderate on self-awareness, etc. is a different issue from "what" they say they are experiencing. A review of the history of introspection and phenomenological assessment in classical and contemporary inspection suggests that individuals can be fairly accurate at retrospectively assessing the content (the "what") of subjective consciousness, especially when completed in reference to standardized questionnaires immediately afterwards (Kukla, 1983; Pekala, 1991a; Singer & Kolligian, 1987). Such individuals are much less accurate at determining the "how" or "why" of their experience (see Nisbett and Wilson, 1977, and Smith and Miller's, 1978, reply).

⁶The client said in reference to the sitting quietly period for which the PCI was completed: "I was conscious; I was very relaxed. All of a sudden "Ship Rock" appeared and a flash of the water and I had the thought: 'Mother Mary comes to me' - like stream of

consciousness types of stuff.” The aforementioned suggests an increase in primary process mentation during this sitting quietly period (Nash, 1987).

⁷So as not to confuse the reader, “hypnosis” (see x-axis of the Figure 1) was defined in that paper as most researchers and clinicians currently use the term. This paper was published over 20 years ago, long before I decided to cite Weitzenhoffer’s distinction between hypnosis and hypnotism.

⁸“Qualia” refer to “the sensory qualities associated with our phenomenological experience, the way in which the world appears to us in experience” (Van Gulick, 2007, p. 385).

⁹The EXCEL program (Pekala, Maurer, & Ott, 2009) generates an average score for the 4 major hypnotic domains assessed by the PCI-HAP, called the Hypnotic Responsivity Index (HRI). It is the average of: the adjusted hypnoidal state score, the imagoic suggestibility score, the average total expectancy score, and the self-reported hypnotic depth score. See the interpretative manual¹ (Pekala, 2009b) for details.

¹⁰Pekala (1991a, pp. 210-211; 225-228) reviews the principle of specificity, including same- and different-condition specificity, and its relevance and importance for comparing subjective experience or “states of consciousness” across similar and dissimilar stimulus conditions.