Reply to “Methodological and interpretative issues regarding the Phenomenology of Consciousness Inventory - Hypnotic Assessment Procedure: A comment on Pekala et al. (2010a, 2010b)”

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Abstract
Terhune and Cardeña review and comment on two recent papers by Pekala et al. (2010a,b). These papers “attempt to integrate diverse facets of hypnotic responding and reconcile seemingly competing accounts of hypnosis” (Terhune & Cardeña, 2010, p. 105) by the “complementary use of phenomenological and hypnotic suggestibility measures” (p. 105) in an attempt to develop a measure of hypnotic responsivity which, I believe, is clinically viable. Although Terhune and Cardeña “applaud their [Pekala et al.’s] complementary use of phenomenological and hypnotic suggestibility measures” (p. 105), they suggest that Pekala et al. “have sacrificed too much, resulting in a measure with a number of important shortcomings whose empirical utility is questionable” (p. 105). Although many of their comments are justified, the PCI-HAP was developed to be used in a clinical private practice setting which often restricts the use of other tools due to time constraints. Furthermore, the phenomenological “richness” of this approach allows clinicians and researchers to better understand hypnotism from their clients’ and participants’ points-of-view and in a manner that can augment and complement traditional assessment approaches to hypnotism.

Keywords: Altered states of consciousness, consciousness, expectancy, hypnosis, hypnotic depth, hypnotism, psychophenomenology, suggestibility, trance.
I am pleased to respond to the thoughtful commentary by Terhune and Cardeña (2010) on the two papers by Pekala et al. (2010a,b). As Terhune and Cardeña aptly summarized, these papers “attempt to integrate diverse facets of hypnotic responding and reconcile seemingly competing accounts of hypnosis” (2010, p. 105) by the “complementary use of phenomenological and hypnotic suggestibility measures” (p. 105) in an attempt to develop a measure of hypnotic responsivity which, I believe, is clinically viable.

I am in agreement that the PCI-HAP (Phenomenology of Consciousness Inventory - Hypnotic Assessment Procedure) (Pekala, 1995a,b) has “a number of important shortcomings” (p. 105), although I disagree that its “empirical utility is questionable” (p. 105). But I also strongly agree that an examination of the “variability among highly suggestible individuals from the purview of the approach that Pekala et al. have adopted but with a greater diversity of methods” (p. 105) will “likely yield a number of insights into the characteristics and determinants of hypnotic suggestibility and self-perceived hypnotic depth” (p.105) that may be quite useful to researchers and clinicians alike.

Before I address their many comments, a few notes are necessary on the methodological approach upon which the PCI-HAP, and particularly, the PCI, are based. The PCI (Phenomenology of Consciousness Inventory: Pekala, 1982, 1991c), is a third generation self-report instrument developed from the PCQ (Phenomenology of Consciousness Questionnaire) (Pekala & Levine, 1981,1982) and the (A)DCQ (Abbreviated) Dimensions of Consciousness Questionnaire (Pekala & Wenger, 1983), that developed out of my doctoral dissertation (Pekala, 1980). The goal was to develop a phenomenological methodology to quantify consciousness according to C. T. Tart’s (1969, 1972, 1977) (altered) states of consciousness approach to the mind.

The approach, dubbed Retrospective Phenomenological Assessment (RPA), “would retrospectively assess subjective experience in reference to specific stimulus conditions and allow for the various structures of phenomenological experience (i.e. imagery, cognition, attention, affect) to be investigated and evaluated in reference to those conditions” (Pekala & Wenger, 1983, p. 251). That approach was developed to permit states and altered states of consciousness to be defined, measured, statistically assessed, and diagrammed.

Because of the “state” nature of the approach, the PCI (administered retrospectively in reference to a short sitting quietly period, such as during a hypnotic induction) yields data on the fleeting and variable contents of the mind that are quite different from the data of trait instruments such as the Harvard Group Scale of Hypnotic Susceptibility: Form A (HGSHS:A; Shor & Orne, 1962), or the Stanford Hypnotic Susceptibility Scale: Form C (SHSS:C; Weitzenhoffer, & Hilgard, 1962). These later tools assess various suggestibility items and generate scales for quantifying a person’s hypnotic ability. By combining a phenomenological approach to hypnosis with several suggestibility items (Pekala, 1995a, 1995b) and expectancy and imagery measures (Pekala et al., 2010a,b), my colleagues and I hoped to generate an instrument that might be useful in a clinical private practice setting.

As the use of this instrument evolved we were able to generate interesting research results (Pekala & Kumar, 2000, 2007; Pekala, Kumar, Maurer, Elliott-Carter, & Moon, 2006; Pekala, Kumar, Maurer, Elliott-Carter, Moon, & Mullen, 2009, 2010a, 2010b) that helped elucidate some of the perplexing dilemmas of our field. Because of the phenomenological nature of the PCI, the PCI-HAP does not measure “hypnotic suggestibility,” but rather “hypnotic responsivity,” which is conceived as a “state” measure of a person’s hypnotic talents at a particular point in time.
This is why we wrote in Pekala et al. that the PCI-HAP was meant to “complement” the traditional trait assessment instruments: “something that fills up, completes, or makes perfect” (Webster’s Seventh New Collegiate Dictionary, 1970, p. 169). Neither the Harvard nor the Stanford C generate an estimate of Weitzenhoffer’s (2002) conceptualization of “hypnosis;” the PCI-HAP does. The hypnoidal state score (see below) is a measure that we find useful and a measure that may be helpful to others, provided other researchers validate its usefulness. Additionally, the PCI generates scores on various dimensions of subjective experience, such as positive and negative affect, altered state of awareness, and other phenomenological dimensions of consciousness, that the trait instruments do not address. Combining the PCI-HAP (which also measures expectancy and pre-hypnotic imagery) with the Harvard or the Stanford C, may generate useful information about the nature of hypnotism that cannot be generated with the traditional instruments alone.

The PCI-HAP includes one cognitive-perceptual suggestion item, the hypnotic dream item. Terhune and Cardeña note that this item is “poorly suited for tapping the upper range of hypnotic suggestibility” (p. 107). However, this hypnotic dream item was specifically chosen so as to not represent such an upper range. As previously mentioned, the PCI-HAP was developed to be used as an initial assessment scale with clients in a clinical private practice setting (Pekala, 1995a,b). The hypnotic dream item, for example, “go on a vacation somewhere to a beautiful place and have a very relaxing and very wonderful time” (Pekala, Kumar, & Maurer, 2009, p. 11) was chosen to help establish a “yes” set (Hammond, 1990) with clients in a manner that, it was hoped, would be positive and affirming and yield information about clients’ imagery that might be useful for subsequent hypnotic or visualization interventions. An agnosia item, as Terhune and Cardeña suggested, would be much less useful clinically. However, I believe that a more research-based version of the PCI-HAP could be developed to include more difficult cognitive-perceptual items and better assess such a cognitive-perceptual ceiling.

The PCI-HAP includes a reliability index (RI) made up of 5 pairs of similar or identical items. In Quantifying Consciousness (Pekala, 1991b), I defined marginal reliability as greater than 2.0 (p. 129) noting that random responding would generate an average reliability index of three. As was pointed out to me by U. A. Ott (personal communication, December 15, 1993), this is not correct; an average random responding RI score would be 2.29. Consequently, the EXCEL program (Pekala, Maurer, & Ott, 2009) currently used to generate a 5-page report for the clinician concerning a client’s hypnotic responsivity, gives three reliability criterion levels: “RI scores of 2.00 or less are considered reliable; RI scores between 2.01 and 2.29 are marginally reliable; (and) RI scores of 2.30 or greater are unreliable” (Pekala, Kumar, & Maurer, 2009, p. 22).

When the PCI was being developed, I tried various reliability index cut-off levels, including 2.25, 2.0, 1.75, 1.50, etc. “The 2.0 cut-off score allowed for a sizable increase in the multiple $R$, without sacrificing too many subjects” (R. J. Pekala, personal communication, December 29, 1993, to U. A. Ott), and that is why the earlier studies used that cut-off. However, due to the subject population with which I work, I have found it useful to “relax” the level to 2.30 to include more participants, which tends to help increase the power of the investigation, but not necessarily increase the reliability and/or validity of the data.

Additionally, the EXCEL scoring program does not generate a hypnoidal state score, trance typology profiles, or the PCI (sub)dimension intensity scores if the RI index is 2.30 or greater. But it does with scores less than that, allowing a clinician to decide whether he or she wants to consider the PCI-HAP reliable if the score is between 2.01 and 2.29. (See
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Pekala, 2009b, for the interpretative manual on using the PCI-HAP where this issue is discussed.) Hence, additional research reviewing various RI cut-off levels, is suggested.

Terhune and Cardeña rightly point out that the sample from Pekala et al. (2010b) exhibited 19% who were unreliable at completing the PCI, higher than previous studies (9%; Kumar, Pekala & Cummings, 1996) or their own research (4%; Terhune and Cardeña, in press). This is because the current research was done with individuals who may not have the same IQs as do college students. However, many of these individuals were highly motivated I believe, to do whatever they could to help with their drug and alcohol addiction, therefore, the high expectancy effects found in that study. Additionally, a sizeable percentage of those “missing participants” were individuals who did not complete the PCI-HAP pre-assessment, because it was not yet developed. Even though a more recent study has replicated the results of Pekala et al. (2010b) (see Pekala & Maurer, 2010), more research employing various strata of participants is needed.

As we mentioned in our articles and to which Terhune and Cardeña reiterated, “all of the measures, aside from the hypnoidal state score, are each indexed by a single item” (2010. p. 106) or “One-item measures are far less reliable than composite measures” (2010, p. 106). True, yet this is a necessity, since the PCI-HAP was developed primarily for a clinical setting where time is of a premium.

I believe the PCI-HAP can “complement” the more cognitive-behavioral scales such as the Harvard or the Stanford C, from the more clinical perspective that the PCI-HAP offers, in contradistinction to the more research approach of the Harvard or the Stanford C. In the 1995 paper on the PCI-HAP (Pekala, 1995b), I cited Cohen (1989) who found that only 8% of experienced hypnotherapists “routinely perform a standardized test with their patients” (p. 284). Lynn and Kirsch (2006) suggest that percentage would probably not be appreciably different had a similar survey been done at that time.

Invited commentaries on Cohen’s paper by Barber (1989), Diamond (1989), Frankel (1989), Rossi (1989), and Spiegel (1989), suggested that “better and more independent means of (1) predicting hypnotizability and (2) verifying that an experience is hypnotic in nature” (Barber, 1989, p. 11) was needed. 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” (Orne, 1977, p. 19). To this effect, the PCI-HAP:

“assesses various aspects of hypnotic experience, such as volitional control, absorption, and altered state of awareness, which is the multidimensional approach that Frankel (1989) advocates. Because it assesses predominantly hypnotic experiences, as opposed to hypnotic behaviors, it dovetails with what Diamond (1989) says is currently lacking in hypnotic assessments” (Pekala, 1995b, p. 285).

When doing clinical hypnosis research, use of the PCI-HAP with the Harvard or the Stanford C may provide especially useful information. Neither the Harvard nor the Stanford C asks about negative effects, let alone aspects of positive and negative affect, which the PCI-HAP provides (Pekala, Kumar, Maurer, Elliott-Carter, Moon & Mullen, 2009). Nor does the Stanford C or the Harvard ask about expectancy effects or pre-hypnotic imagery vividness, which the PCI-HAP provides (Pekala et al., 2010a,b). Additionally, the PCI-HAP EXCEL program generates raw and percentile scores for the 26 PCI (sub)dimensions, which allows clinicians and researchers to determine which contents of consciousness are activated during
a sitting quietly period embedded in the hypnotic induction. Clinicians can then generate process-
congruent suggestions consistent with the phenomenology endorsed by the client on the PCI, i.e. high visual imagery, loss of volitional control, low absorption, etc. However, as Terhune and Cardeña indicate, the PCI can be added to a “resting epoch embedded within one of these measures, as is commonly done” (p. 107) to generate the PCI raw and percentile scores.

Trance is a very complicated and complex term, as Terhune and Cardeña have commented when suggesting that “Pekala et al.’s definition of trance state effects is problematic” (p. 108). The definition of trance that is cited in the Pekala et al. (2010a) paper is actually taken from the 2000 paper by V. K. Kumar and I:

“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).

I would submit that any definition of trance is going to be problematic, given its use in common parlance, the hypnosis clinical and research literature, and cross-cultural perspectives. Here we use the term, trance, as clinicians generally use the phrase, “being in trance,” or “being in a hypnotic state,” and how Weitzenhoffer (1989) would use the term: “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” (p. 298). As we said in our 2000 article:

“Knowledge usually proceeds from the general to the specific (Heidegger, 1927/1962). Applying this to an understanding of the nature of trance, we must first come up with a general, but operational, measure of trance. Once that has been established, then this notion can be more carefully scrutinized to determine if these might be different types of trance” (Pekala & Kumar, 2000, p. 111).³

Hence, this measure of trance, as operationalized via regression analyses by the predicted Harvard Group Scale (pHGS) score (Forbes & Pekala, 1993; Pekala, 1991b; Pekala & Kumar, 1984, 1987) and later defined as a hypnoidal state score (Pekala & Forbes, 1988; Pekala & Nagler, 1989), is meant to be a beginning attempt at operationalizing a very “problematic” concept. As we reasoned in 1989:

“We think it is premature, however, to label the state associated with a high pHGS score a “hypnotic” state. It is unknown at this point if experiencing all the phenomenological parameters that would generate a very high pHGS score would be associated with a “deep” hypnotic state. In other words, although the regression equation allows one to determine the average phenomenological parameters associated with being a “deep” hypnotic state, it does not follow that experiencing such phenomenological
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effects would be associated with being able to experience classic hypnotic 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).

Hopefully, through commentaries and replies like these, and with additional research, such conceptualizations as defined above can be made more rigorous.

Additionally, I agree that the definition of trance as discussed in articles such as Pekala (1995a,b), Pekala and Kumar (2000), and Pekala et al. (2010a,b) may be very different from “trance” as “used in the dissociation and cross-cultural literatures (Cardeña, Van Juül, Weiner, & Terhune, 2009; Terhune and Cardeña, 2010, p.108), and welcome the use of the PCI or similar state instruments to determine how “dissociative trance” may be different from “hypnotic trance,” cross-cultural types of trance, etc.

What is important, as Terhune and Cardeña point out, is that consciousness and hypnosis can be assessed via the PCI, phenomenologically quantified, and then correlated in reference to psychophysiological and neurobiological concepts such as attentional networks (Raz & Buhle, 2006), and the gamut of cognitive neuroscience approaches to consciousness (Pekala & Kumar, 2007). This is important since “a growing number of cognitive scientists now recognize the need to make systematic use of introspective phenomenological reports in studying the brain basis of consciousness” (Lutz & Thompson, 2003, p. 31). Similarly, Zelano, Moscovitch, and Thompson (2002) asserted that “there is also the growing realization, however, that it will not be possible to make serious headway in understanding consciousness without confronting the issue of how to acquire more precise descriptive first-person reports about subjective experience” (p. 2).

The PCI-HAP, I believe, is an extremely rich instrument for quantifying and statistically assessing phenomenological consciousness during hypnotism. 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 PCI allows for this to be done in a manner that should further our understanding of hypnotism and other interesting phenomena, such as meditation (Venkatesh, Raju, Shivani, Tompkins, & Meti, 1997), fire-walking (Hillig, & Holroyd, 1997/98; Pekala & Ersek, 1992/93), shamanistic trances (Rock, Wilson, Johnston, & Levesque, 2008), and possibly other altered states of consciousness phenomena (Kallio & Revonsuo, 2003, 2005).
The volitional control item of the PCI assesses the sense of volitional control, or lack thereof, that the client or research participant endorses during an embedded period of sitting quietly in the hypnotic induction. As such, it measures the phenomenological sense of loss of control, which is different from the “experience of involuntariness during suggested responses” (Terhune & Cardeña, 2010, p. 109). However, the eye catalepsy item of the PCI, being a motor challenge item, does assess the classic suggestion effect as defined by Weitzenhoffer (1974, 1980). The research cited by Pekala et al. (2010b) only used the “yes/no” participant response in the regression equation in attempting to predict self-reported hypnotic depth. However, a 7-point assessment of being (un)able to open ones eyes is also scored with the EXCEL program, and the summary page of the EXCEL print-out lists a “classic suggestion effect total score” that is an average of PCI volitional control item and the behavioral scoring of the eye catalepsy item (see Pekala 2009b, page 20).

A paper presented in 2009 (Pekala & Maurer, 2009) at the Society for Clinical and Experimental Hypnosis reviewed the classic suggestion effect, as associated with the eye catalepsy item, and phenomenological effects associated with the sitting quietly period via the PCI volitional control dimension. Data analysis found a nonsignificant correlation between volitional control as measured by the PCI, and the 7-point eye catalepsy item. This suggests that different processes are involved with the behavioral item (versus the phenomenological item) as Terhune and Cardeña have suggested.

I also agree with Terhune’s and Cardeña’s assertion that the relationships between hypnotic suggestibility, imagery/imagination, and non-hypnotic suggestibility are extremely complex. In 1995 (Pekala, 1995a) I wrote that the hypnotic dream item was added to make a distinction between “fantasy-high and “classic-high” individuals, both types of high hypnotizables (based on cluster analysis research, Pekala, 1991a). The former reported “a great deal of vivid imagery and positive affect during a hypnotic induction procedure” (Pekala, 1995a, p. 275). To the contrary, the latter appeared to “empty out” their minds, reporting the “lowest level of memory, rationality, internal dialogue, imagery, and self-awareness” (Pekala & Kumar, 2000, p. 121) of the nine different cluster groups. So the rationale for including the hypnotic dream to possibly differentiate between low and high imagery, high susceptibles, is consistent with Terhune and Cardeña, who suggest that “a number of studies suggest that only a subset of HS individuals have strong imagery abilities” (p. 9). Additionally,

“this item was added in the hope of being able to differentiate those individuals who might have vivid imagery but who do not test to be that hypnotizable (Rhue & Lynn, 1989) and for which implementation of visualization strategies might be quite therapeutic despite their low hypnotizability” (Pekala, 1995a, p. 275).

The hypnotic dream item taps what we called imagoic suggestibility, a subset of Kirsch and Braffman’s (2001) imaginative suggestibility “requests to experience an imaginary state of affairs as if it were real” (p. 59). When completing the debriefing questionnaire, participants are asked to rate their hypnotic dream on a 1 to 10 point scale, with “10” representing “as real and vivid as actually being there” (Pekala, Kumar, & Maurer, 2009, p 17). Hence, although the relationships between hypnotic suggestibility, imagery/imagination, and related concepts are complex, earlier papers on the PCI-HAP by Pekala (1995a,b) and Pekala and Kumar (2000, 2007) review the reasons why various items that were utilized for the development of the PCI-HAP were added. I believe these are internally consistent and
congruent with the research that Terhune and Cardeña cite.

Pekala et al. (2010b) wrote that step-wise regression analyses were chosen with the goal to “maximize $R^2$” (p. 310) and to “determine the best subset of independent variables ‘to predict the criterion to a high degree of accuracy’ (Newton & Rudestam, 1999, p. 253)” (p. 310). No mention was made in Pekala et al., (2010a,b ) that correlation predicts causality. Additionally, the standardized regression coefficients describe only “the relative contributions of each predictor to the overall effect” (Grimm & Yarnold, 1995, p. 41). Consequently, if we operationally define concepts or variables specific ways, the regression analyses discussed in Pekala et al. (2010b) can only reveal how these variables may be statistically related to the variable of interest. I believe the Woody, Barnier, & McConkey (2005) article accurately reflects possible causal relationships between pre- and post-hypnotic expectancies and hypnotizability, with aptitude and attitude each having an effect on the other.

For Pekala et al. (2010b), several sets of regression analyses were completed, some of which were not included in the final paper. Because the combined total expectancy variable accounted for a large percentage of the relative variance across the various analyses, it was included. I find this variable especially important when reviewing the results of the hypnotic assessment with clients.

As an example, I recently recorded a hypnotic protocol for a client to practice self-hypnosis in coping with various environmental stressors. She obtained percentile scores of 37 for hypnoidal state, 64 for imagoic suggestibility, and 100 for average total expectancy. She reported the protocol to be “extremely helpful.” I believe this was not so much because of her hypnoidal state score, or her imagery under hypnotism, but because of her extremely high expectancy score. I find the total expectancy score (of the estimated/expectancy scores) the most useful in my private practice. However, it may not be the best variable to use when we attempt to determine the causal basis for hypnotism, as Terhune and Cardeña have written.

Although Terhune and Cardeña cite Barber (2000) as supportive of the theoretical model underlying the PCI-HAP, it was really Holroyd’s (2003) model which focuses on the three processes, as opposed to the three types, that the PCI-HAP is more theoretically based: “Holroyd’s model (2003) of hypnotism can be operationally defined with aspects [italics added] of the model quantified and estimated using the PCI-HAP” (Pekala et al., 2010b, p. 292).

I am in agreement with Terhune and Cardeña concerning the complexity of the relationships among the processes activated in high susceptibles. Although Terhune and Cardeña write that “Pekala et al.’s (2010b) interpretation does not acknowledge the complexity of these relationships,” (2010, p. 110), the paper by Pekala and Kumar (2000) does. Terhune and Cardeña do acknowledge the “series of earlier studies (by) Pekala and colleagues” (2010, p. 111) where these issues were addressed. After defining trance in general, and using the hypnoidal state score as a measure of such, a considerable body of Pekala and Kumar (2000) is devoted to “trance typology profiles,” including a cluster analysis of individuals who scored 10 to 12 on the Harvard (see Figure 2, Pekala & Kumar 2000, p. 117). We cite four studies (Forbes & Pekala, 1996; Pekala, 1991a; Pekala & Forbes, 1997; Pekala, Kumar, & Marcano, 1995) where qualitative variations in phenomenological experience were reviewed across low and high susceptibles (HS) and also across the full range of Harvard scores.

I applaud the recent work of Terhune and Cardeña (in press) where “one of two derived subtypes of HS individuals, labeled the dissociative profile, exhibited greater involuntariness during hypnotic responding - the classic suggestion effect (Weitzenhoffer, 1974, 1980) - despite displaying equivalent hypnotic suggestibility relative to the other HS subtype” (p. 111). If we are to follow Orne’s (1977) admonition about the importance of an
individual’s subjective experience in determining hypnotic susceptibility, then I must agree with Terhune and Cardeña (2010): “We are confident that examining individual differences in hypnotic responding among HS individuals with a diversity of methods and a multivariate approach will not only strengthen our knowledge of the characteristics and determinants of hypnotic depth but will also help us to develop a more nuanced understanding of hypnosis” (p. 111).

The PCI-HAP and the retrospective phenomenological assessment (RPA) methodology (Pekala, 1980, 1991b; Pekala & Kumar, 2000, 2007; Pekala & Wenger, 1983) upon which the PCI is based, allows clinician and researchers to look at clients’ and research participants’ experiences of hypnotism in a manner that quantifies relevant aspects of participant’s subjective experiences. However, there are problems specific to this methodology, including the lower reliability of the state nature of the PCI (sub)dimensions. There are also many issues that plagued classical introspection 100 years ago (Angell, 1907; Boring, 1929/50; Titchener, 1898). Yet, I believe, these are partially addressed with the current methodology (Pekala, 1980, 1991b, 2009a).

The present methodology allows contemporary introspection, what Hilgard (1980) called phenomenological assessment, to be more reliable and valid than classical introspection. Yet, additional basic research to more fully understand and implement this phenomenological approach to consciousness, in general, and hypnotism in particular, has yet to be done (Pekala, 1991b; 2009a). However, this is an approach that has changed over three decades, evolving with the data and the critiques and suggestions from many. I’m sure it will continue to evolve, partly in response to the spirited discussions such as those provided by Terhune and Cardeña (2010), Wagstaff (2010), and hopefully, many others.

Terhune and Cardeña’s seasoned critique points out some of the conceptual, methodological, and interpretative issues for hypnotism that will need to be addressed in future research. Interested researchers are referred to Pekala (1980, 1991b, 2009a, 2009b, in press), Pekala and Cardeña (2000), Pekala and Forbes (1988), Pekala and Kumar (2000, 2007), Pekala and Nagler (1989), and Pekala and Wenger (1983) for a discussion of related methodological and statistical issues that concern introspective and retrospective phenomenological assessment, the PCI, and this psychophenomenological approach to hypnotism. I believe the PCI-HAP allows clinicians and researchers to look at a client’s, and research participant’s, experience of hypnotism in a manner that quantifies relevant aspects in a manner conducive to a better understanding of hypnotism. It is hoped that the methodology from which the PCI-HAP was developed will continue to evolve into a comprehensive methodology useful to both clinicians and researchers alike.

**Footnotes**

This reply is in reference to a commentary 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 contents of this presentation does not represent the views of the Department of Veterans Affairs nor the United States Government. The author wishes to thank Mr. Ron Maurer for his helpful comments on an earlier version of this manuscript, and Stephen R. Lankton for his deeply appreciated editorial assistance with this paper.

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, 2009a) manuals, and the EXCEL scoring program (Pekala, Maurer, & Ott, 2009) are available at www.quantifyingconsciousness.com.

2I confined my comments to comparisons with the Harvard and the Stanford C. The Hypnotic Induction Profile (HIP: Spiegel & Spiegel, 2004), is also a well researched clinical
and research instrument that is used in hypnotic assessment. It includes phenomenological assessment, assessing phenomenological items such as tingling, dissociation, amnesia, and floating sensations, in addition to behavioral items. It probably comes closest to the PCI-HAP in terms of methodological similarities.

I recommend that researchers review this particular paper (Pekala & Kumar, 2000), as it lays the conceptual and operational groundwork for the notion of “trance” as discussed.

Standardized coefficients, however, do not report the unique variance accounted for by a particular predictor. For that, semi-partial correlation analyses are needed. The unique variance attributable to a particular predictor is “the amount by which the R² would be reduced if that variable were removed from the regression equation” (Newton & Rudestam, 1999, p. 271), called a semi-partial correlation. Hence, the standardized regression coefficients can only tell the relative influence on the dependent variable. Such standardized coefficients are useful “when assessing the effects of different variables within a single regression equation or population” (Newton & Rudestam, 1999, p. 268) as was done in Pekala et al. (2010b). Such regression coefficients are not useful when comparing across populations.

Terhune and Cardeña write on page 111 that Pekala et al. (2010a) found that “trance effects following a hypnotic induction were negatively related to alterations in body imagery, other results suggests the opposite (Cardeña, 2005).” The regression equation (2010a, p. 280) shows an unstandardized regression coefficient that delineates a negative relationship between altered body image and hypnoidal state, but the following page (p. 281) states that the Pearson r was actually opposite, i.e. positive, as would be expected. This is because, we believe, altered body image is functioning as a suppressor variable (Grimm & Yarnold, 1991) within the regression equation.

A PowerPoint presentation (Pekala, 2009a), available by emailing pekalar@voicenet.com, reviews some of the major puzzles and perplexities of hypnotism, how phenomenological analysis is helpful, and some of the constraints and limitations of this particular approach.

It is recommended that readers procure the administrative (Pekala, Kumar, & Maurer, 2009) and interpretative (Pekala, 2009b) manuals for the PCI-HAP, as they provide information on the PCI-HAP that is not referenced in the peer reviewed publications.

References

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