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

Devin Blair Terhune
Etzel Cardeña

Lund University

Abstract
In two papers, Pekala et al. (2010a, 2010b) reviewed and empirically assessed the relationships among response expectancies, hypnotic suggestibility, spontaneous alterations in consciousness following a hypnotic induction, and hypnotic depth. We appreciate their attempt to integrate diverse facets of hypnotic responding and reconcile seemingly competing accounts of hypnosis. In addition, we applaud their complementary use of phenomenological and hypnotic suggestibility measures. However, in their attempt to develop a clinically-viable measure of hypnotic responsiveness, we believe that they have sacrificed too much, resulting in a measure with a number of important shortcomings whose empirical utility is questionable. Furthermore, their review and study gloss over a number of important distinctions. Finally, we believe that they over-interpret the relationships between their selected measures and ones previously used in the extant literature. A closer examination of variability among highly suggestible individuals, from the purview of the approach that Pekala et al. have adopted, but with a greater diversity of methods, is likely to yield a number of insights into the characteristics and determinants of hypnotic suggestibility and self-perceived hypnotic depth.

Keywords: Consciousness, hypnotic depth, hypnotic suggestibility, phenomenology, trance.
The characteristics and determinants of hypnotic depth, the extent to which one feels that one has been hypnotized, are neglected topics in contemporary hypnosis research. In particular, how hypnotic depth may influence hypnotic suggestibility and an individual’s response to hypnotherapy are poorly understood. In two papers, Pekala and colleagues review and empirically assess different correlates and predictors of hypnotic depth (Pekala, Kumar, Maurer, Elliott-Carter, Moon, & Mullen, 2010a, 2010b). They adopt a multivariate approach proposing, and presenting evidence to support the claim, that self-reported hypnotic depth (srHD) is determined by a confluence of variables including an individual’s pre-induction expected hypnotic depth, trance state effects during hypnosis (i.e., spontaneous alterations in consciousness), and hypnotic suggestibility (i.e., eye catalepsy and dream suggestions). Although these papers are welcome contributions to this literature, we believe that they possess a number of conceptual, methodological, and interpretative shortcomings that limit any conclusions that may be drawn from them.

Measuring hypnotic responsiveness

The history of the interaction between hypnosis researchers and practitioners has consisted of many good-faith attempts to introduce measures that are useful in both applied and experimental settings. Unfortunately, only rarely have tools met such challenging criteria. The utility of Pekala et al.’s (2010a, 2010b) measure, the Phenomenology of Consciousness Inventory – Hypnotic Assessment Procedure (PCI-HAP; Pekala, 1995), is readily apparent for clinical settings, insofar as its procedure is relatively quick and provides rich phenomenological data. However, it is helpful to place this contribution in context. As a clinical measure that uses only two items, it has similarities to the Hypnotic Induction Profile (HIP; Spiegel & Spiegel, 2004), and like the HIP, the PCI-HAP possesses the limitation of not including a diverse pool of suggestions (Woody, Barnier, & McConkey, 2005), although it has the advantage of including a more elaborate assessment of spontaneous experiential concomitants of hypnosis than the HIP. However, the PCI-HAP clearly does not yield as rich data regarding hypnotic suggestibility as other measures such as the Stanford Hypnotic Clinical Scale for Adults (Morgan & J. Hilgard, 1978). Ultimately, the crux of this issue is whether self-report measures of hypnotic depth are suitable substitutes for behavioral and experiential scoring schedules of hypnotic suggestibility. It is reasonable that clinicians will want to substitute the former for the latter in the interest of time, but the viability of this substitution for empirical research is far less convincing.

From an empirical standpoint, the most salient limitation of the PCI-HAP is how all of the measures, aside from the hypnoidal state score, are each indexed by a single item. One-item measures are far less reliable than composite measures and, as Pekala et al. (2010b) acknowledge, their internal consistency cannot even be computed. A further weakness of the measure is that it includes different scoring scales for the hypnotic suggestibility items, resulting in the inability to directly compare responsiveness to each. A consequence of such differential scoring is that it is unclear whether the greater predictive utility of the dream suggestion relative to the eye catalepsy suggestion is because of the former’s status as a cognitive-perceptual suggestion, its greater difficulty, its wider scoring range, or its greater salience (see also Wagstaff, 2010). Although Pekala et al. are quick to point out that the PCI-HAP is intended not to substitute but to complement existing behavioral measures, it is unlikely that any researchers will supplement standardized scales 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).
with the suggestions of the PCI-HAP. Rather, they may include the PCI (Pekala, 1991) in reference to a resting epoch embedded within one of these measures, as is commonly done (for reviews, see Pekala & Cardeña, 2000; Pekala & Kumar, 2007). Moreover, if the PCI-HAP is used in addition to typical group scales of hypnotic suggestibility (Bowers, 1993; Shor & Orne, 1962), Pekala and collaborators should make a case as to the benefits of their measure of general hypnotic depth relative to the experiential measures already developed to accompany these scales (Kirsch, Council, & Wickless, 1990; Kirsch, Milling, & Burgess, 1998). Accordingly, it is unclear what benefit the PCI-HAP provides researchers above and beyond existing behavioral and experiential measures and, in particular, whether it is well-suited to examine the questions put to test in Pekala et al.’s (2010b) empirical study.

A further issue, given the dearth of suggestions included in the PCI-HAP, is whether the dream suggestion is an optimal cognitive-perceptual suggestion for inclusion in this scale and whether it most adequately represents the cognitive-perceptual factor of hypnotic suggestibility on the HGSHS:A and SHSS:C (Woody, Barnier & McConkey, 2005). Among difficult cognitive-perceptual items, dream suggestions are actually the easiest to respond to (Weitzenhoffer & Hilgard, 1967), and thereby poorly suited for tapping the upper range of hypnotic suggestibility. What if the authors had administered an inhibitory cognitive suggestion (e.g., agnosia [Weitzenhoffer & Hilgard, 1967]) instead of the dream suggestion? Such an item, which does not explicitly require or encourage imagery-based cognitive strategies, may be a better predictor of srHD. Neglecting to systematically measure hypnotic suggestibility, which has previously been found to be related to all of the measures in this study, represents an important shortcoming because it is very likely that the strength of the different predictors would have been reduced had more hypnotic suggestions been included and that some of the predictors would not have been retained in the regression equation.

A further methodological consideration concerns not the PCI-HAP, but its administration. Participants were instructed to provide pre-hypnotic expectancies for hypnotic depth and subsequently reported their srHD in a posthypnotic debriefing period. It is very likely that participants were able to identify the connection between these two values such that their relationship may be magnified in part by demand characteristics and in particular consistency motivation (Council & Green, 2004). Indexing an individual’s response expectancies for subsequent experiences will undoubtedly influence those experiences by either restricting the individual’s experiential response to within the bounds of their own expectancies or by leading them to reconstruct their responses during the posthypnotic debriefing period in light of their expectancies. Importantly, this potential confound is not unique to this study and we regret that it has not been addressed in previous studies of the influence of response expectancies on hypnotic responding. We hope that future work on response expectancies will address this issue.

Another limitation of Pekala et al.’s (2010b) study pertains to the reliability of their sample. Of the 223 participants in their sample, 43 (19%) exhibited unreliable response patterns, a proportion that is notably higher than in previous studies (9% [Kumar, Pekala, & Cummings, 1996]; 4% [Terhune & Cardeña, in press]). Furthermore, their chosen reliability index threshold of 2.3 was more liberal than that which they have previously used (i.e., 2; Kumar, Pekala, & Cummings, 1996). It is reasonable to infer that adopting the standard criterion would have led to the rejection of even more participants. In addition, another 57 participants were excluded for having missing data. This total exclusion rate not only questions the utility of the PCI-HAP for use in a substance abuse population, but also the representativeness of the sample. After the removal of 100 participants (45%), it is unclear whether the remaining participants are representative of the sample and whether the results can be generalized to other samples.
The history of hypnosis research is replete with ambiguously defined terminology or misleading usage of familiar words. Although Pekala et al. (2010a, 2010b) are for the most part very careful in their distinctions and definitions, at times they overstretch their operationalizations. For example, Pekala et al.’s definition of trance state effects is problematic. They maintain that “[t]rance can be operationally defined as the subjective state the highly hypnotizable person reports in response to a hypnotic induction” (Pekala et al., 2010a, p. 275). This definition is worrisome because it restricts the experience of “trance” effects to highly suggestible (HS) individuals and to hypnotic experiences. Specifically, it makes the questionable assumptions that hypnosis is only effective with HS individuals, which we know is not the case, and that trance is only experienced during hypnosis or that all forms of trance are actually hypnosis.

This definition also fails to appreciate the vagaries of phenomenological response to hypnosis. HS individuals have been repeatedly found to exhibit differential spontaneous experiential response patterns following a hypnotic induction (Pekala & Kumar, 2007; Terhune & Cardeña, in press), a fact that Pekala et al. acknowledge when they refer to “different types of trance associated with hypnotism” (Pekala et al., 2010a, p. 275). There is also evidence that an individual’s experiential response to hypnosis is relatively dynamic; that is, the same person may have qualitatively different spontaneous experiences during hypnosis. For instance, following a hypnotic induction, HS individuals spontaneously experience mostly body image and vestibular alterations during periods described as low in hypnotic depth, whereas they have mostly imaginal and transcendent experiences during periods characterized by greater depth (Cardeña, 2005).

Finally, the use of the term “trance” has a number of additional problems, which have been described earlier in the literature. One of them is that the term refers to a number of different concepts that are not interchangeable, such as absorption, unresponsiveness, spirit possession crises, and so on (Cardeña & Krippner, 2010). Rather than a multifocal, vague term, what we need is the clear specification of what is being defined at the phenomenological and other explanatory (e.g., cognitive, neurophysiological) levels. For this reason, we believe that a more apt approach is that which attempts to relate spontaneous alterations in consciousness following a hypnotic induction to constructs in cognitive science that have received considerable empirical support such as attention networks (e.g., Raz & Buhle, 2006). Finally, “trance” is used in the dissociation and cross-cultural literatures (Cardeña, Van Duijl, Weiner, & Terhune, 2009) in a different way than Pekala and collaborators do. Neglecting the way in which this term is used elsewhere reduces the prospects of integrating experimental hypnosis research with research on relevant phenomena.

A further concern with Pekala et al.’s (2010a, 2010b) arguments is that they repeatedly equate different constructs that are both conceptually and empirically distinct from one another. For instance, Pekala et al. note how one of the characteristic features of the hypnoidal state, the experiential response to hypnosis, is a decrease in spontaneous volition and equate this with the classic suggestion effect, the experience that hypnotic responses occur in an involuntary or extra-volitional manner (Weitzenhoffer, 1974, 1980). It needs to be mentioned that these effects reflect two distinct processes. The distortions in volition indexed by the PCI (Pekala, 1991) refer to spontaneous alterations in volition during a sitting-quietly resting period embedded within the hypnosis session, whereas the classic suggestion effect...
refers to the experience of involuntariness during suggested responses. In their terminology, the former is the product of hypnosis whereas the latter is the signature of hypnotism. Although the spontaneous distortions in volition probably contribute to the classic suggestion effect, until their relationship is more carefully examined, it may be premature to interpret the volition subscale of the PCI as a measure of involuntariness during hypnotic responding.

We similarly think that Pekala and colleagues do not afford sufficient attention to the complexities of the relationships between hypnotic suggestibility, imagery/imagination, and non-hypnotic suggestibility. First, it needs to be noted that the relationship between hypnotic suggestibility and imagery is not sufficiently clear to warrant the latter being heralded as a “major predictor” of the former as Pekala et al. (2010a, p. 278) maintain (see also Laurence, Beaulieu-Prévost, & du Chéné, 2008). In fact, a number of studies suggest that only a subset of HS individuals have strong imagery abilities (Wallace, 1990; Wallace, Allen, & Propper, 1996). Furthermore, imagery is only habitually used by a subset of HS individuals during hypnotic responding (Hargadon, Bowers, & Woody, 1995), which Pekala et al. (2010a) acknowledge elsewhere. Pekala et al. (2010a) make similar questionable assumptions when they implicitly equate imagery/imagination with imaginative suggestibility, that is, responsiveness to suggestions for imagining a particular event or experience (Kirsch & Braffman, 2001). Imaginative suggestibility can be parsed into non-hypnotic and hypnotic forms. Although we appreciate Kirsch and Braffman’s (2001) goal of advancing the precision of terminology used in hypnosis research, we believe the term imaginative suggestibility is misleading because only a subset of the hypnotic suggestions on standardized scales of hypnotic suggestibility includes explicit instructions for imagining a particular state of affairs. This term assumes that imagination plays a fundamental causal or mediatory role in responsiveness to both non-hypnotic and hypnotic suggestions, whereas the evidence for such an assumption is far from clear (Hargadon, Bowers & Woody., 1995; Laurence, Beaulieu-Prevost & DuChene, 2008).

**Interpretation**

We also would debate the manner in which Pekala et al. (2010b) interpret their results and link them with the extant literature. In a number of cases, they over-emphasize superficial similarities or gloss over important complexities. For instance, the result that post-hypnotic expectancies for therapeutic efficacy of hypnosis predicted srHD was interpreted by Pekala et al. as consistent with the finding that post-induction response expectancies for one’s level of hypnotic responding predicted subsequent behavioral and experiential hypnotic responding (Council, Kirsch, & Hafner, 1986). In addition to the fact that the latter study involved the measurement of response expectancies following an induction, whereas Pekala et al. measure therapeutic expectancies following a de-induction, the study by Council et al. concerned hypnotic suggestibility whereas Pekala et al.’s study pertains to hypnotic depth. These effects are plausibly related but their significant differences warrant that greater caution be exerted when trying to relate them.

Pekala et al. (2010b) also argue that post-hypnotic therapeutic expectancies partially account for srHD. In other words, they maintain that an individual’s expectancies for how efficacious hypnotherapy will be for them in the future contributed to their self-perceived hypnotic depth. As with any correlational analysis, one has to be cautious in interpreting the causal direction of this relationship. Nevertheless, we actually find the inverse relationship,
namely that one’s perceived hypnotic depth influences their expectancies for the therapeutic efficacy of hypnosis in the future, more plausible. At the very least, this relationship is unclear and amalgamating pre-induction and post-hypnotic response expectancies into a composite predictor of self-reported hypnotic depth (srHD) is misleading if one contributes to srHD and the other is potentially determined by srHD.

In a similar fashion, Pekala et al. (2010b) interpret the fact that imagoic suggestibility, hypnotoidal state, and response expectancy collectively predict srHD as supportive of Barber’s (1999) model, which argues that there are three distinct subtypes of HS individuals. Although Barber did acknowledge that these three variables contributed to hypnotic responding, the influence of each variable was argued to be selectively pronounced in a particular HS subtype. To borrow Pekala et al.’s preferred terminology, Barber (1999) argued that the hypnotic responding of the fantasy-prone subtype is more reliant upon imagery, the amnesia-prone subtype is more dependent on achieving a hypnoidal state, and the positively-set subtype is more greatly influenced by response expectancies and other sociocognitive factors. In addition, Barber’s (1999) model used these variables as explanatory constructs in order to account for high hypnotic suggestibility — not hypnotic depth per se — in the three groups. Furthermore, the finding that imagoic suggestibility predicts srHD does not specifically address the causal and mediatory relationships underlying hypnotic responding in the fantasy-prone subtype. Barber (1999) argued that superior fantasy and imagery abilities in this group facilitated hypnotic responding, not that responsiveness to dream suggestions facilitated hypnotic depth. The foregoing effects may be related to Barber’s (1999) model, but Pekala et al.’s (2010b) interpretation does not acknowledge the complexity of these relationships, in particular how the different effects may be restricted to, or augmented among, certain subtypes of HS individuals.

Finally, Pekala et al. (2010b) interpret their findings regarding the prediction of srHD as supportive of Woody et al.’s (2005) componential model. Although there are some noteworthy similarities, we believe that this comparison also reflects some over-interpretation. Woody, Barnier & McConkey (2005) principal finding was that when factor analyzed in conjunction, the HGSHS:A and SHSS:C are comprised of a single main factor and four ancillary factors, which they interpreted as indicating that hypnotic suggestibility is represented by a core ability and a secondary set of componential abilities. In linking their findings with those of Woody, Barnier & McConkey (2005), Pekala and colleagues seem to be arguing that expectancy, imagoic suggestibility, and hypnotoidal state effects represent componential abilities. However, Woody, Barnier & McConkey (2005) componential abilities reflect responsiveness to different sets of suggestions. Moreover, it is unclear how expectancy and hypnotoidal state effects compare to the behavioral componential abilities derived by Woody and colleagues. In any case, we believe that interpreting the relationship between Pekala et al.’s findings and those of Woody et al. is by no means clear and requires greater caution.

**Heterogeneity in high hypnotic suggestibility**

Despite the aforementioned concerns about their methodology and interpretation of their data, we are very enthusiastic about the multivariate approach adopted by Pekala et al. (2010a, 2010b) and hope their work inspires others to consider a wider variety of determinants of individuals’ responses to hypnosis. Although they do not address heterogeneity among HS individuals in detail, Pekala et al. (2010a,b) acknowledge variability in this population in trance state effects following a hypnotic induction and in strategy
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utilization during hypnotic responding. In a series of earlier studies, Pekala and colleagues found that HS individuals fell into two or three discrete clusters that exhibited distinct spontaneous experiential response patterns following a hypnotic induction (for a review, see Pekala & Kumar, 2007). We have since conceptually replicated this finding (Terhune & Cardeña, in press) and found that one of the two derived subtypes of HS individuals, labeled the dissociative profile, exhibited greater involuntariness during hypnotic responding, that is, the classic suggestion effect (Weitzenhoffer, 1974, 1980), despite displaying equivalent hypnotic suggestibility relative to the other HS subtype. This finding underscores the importance of examining individual differences among HS individuals and further suggests that HS individuals may be comprised of discrete subtypes with dissimilar phenomenological profiles, an idea advanced by the dissociative typological models of high hypnotic suggestibility (Barber, 1999; Cardeña, 1996; Carlson & Putnam, 1989).

Dissociative typological models provide a number of testable predictions that may help to further clarify some of the relationships with which Pekala et al. (2010a, 2010b) are concerned. For instance, imagoic suggestibility and response expectancy may be greater determinants of hypnotic responding and hypnotic depth in non-dissociative HS individuals, whereas spontaneous alterations in awareness, perception, and control may have a greater influence in dissociative HS individuals (see also Brown & Oakley, 2004; King & Council, 1998). Alternatively, HS individuals may not be comprised of distinct subtypes, but rather may possess different configurations of abilities that differentially interact with, or rely upon, contextual and cognitive variables (Laurence, Beaulieu-Prevost, duChene, 2008; Woody, Barnier & McConkey, 2005). In noting the importance of variability among HS individuals, Nadon, Laurence, and Perry (1991, p. 492) wrote that considering such variability will contribute to our “examination of the interaction between and among personal dispositions, beliefs, and subjective experiences.” We further maintain that future research on these issues should attempt to employ a greater diversity of methods, as there is no single method or perspective “from which the world can appear as an absolute, single fact” (James, 1896, p. ix). Such an approach may also help to clarify inconsistencies; for instance, whereas Pekala et al. (2010a) report that “trance” effects following a hypnotic induction were negatively related to alterations in body image, other results suggest the opposite (Cardeña, 2005), namely that those changes indeed contribute to an individual’s self-perceived hypnotic depth (Wagstaff, 2010). Our best shot at clarifying hypnotic phenomena will probably come out of investigating deep questions through various methods and perspectives, rather than restricting ourselves to a single one. 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.

References

Method and interpretation of the PCI-HAP


