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response

Making the Distinction Between the "Hypnotic" and "Non-Hypnotic"

Author's Response to Commentaries

Graham F. Wagstaff, PhD*

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The commentators raise a wide variety of critical issues, but for obvious reasons of space, in this reply I will confine myself to what I perceive to be the main themes and concerns.

Laurence

Perhaps an obvious place to start is with Laurence's general comment that, "Hypnosis (and the alleged depth of it) may be at best metaphors for an ensemble of phenomena that we either do not grasp yet, that is simply too complex to summarize in a few words, or that may be explained by seemingly irreconcilable theoretical viewpoints." (this issue; p. 110) If we are looking for some single concept or a few ideas that will relate and explain all of the phenomena that have at one time or another been invoked as indicating the presence of 'hypnosis', one can only agree with Laurence's view here (see also Wagstaff, 1981, ix–x). However, the task of attempting to come up with descriptions and explanations of the nature of hypnosis and hypnotic phenomena is surely made more difficult if we cannot even agree on the core subject matter of our investigation. The existence of competing explanatory theories in science is a natural and healthy feature of scientific enquiry; but to have little consensus on how to define the central feature/s of our inquiry rather negates the whole enterprise.

One of the main themes of my paper is that, in terms of definition, the term hypnosis and related terminology might fit together better if they conformed more to their etymological origins, and we made the idea of an altered state of consciousness central to the definition. However, Laurence questions the view that, etymologically, what we now term hypnosis has traditionally been related to the idea of an 'altered state of consciousness'. I would agree that, historically, it is certainly the case that the actual term 'altered state of consciousness' was not made popular until the 20th Century. However, I am not suggesting that the early magnetists and practitioners of hypnosis actually used this terminology, but rather that their ideas implied the presence of such a concept. For example, although Laurence attempts to dismiss Mesmer in this context, it can and has been argued that the roots of the sleep-like state of 'magnetic' or 'artificial somnambulism' as described by the early magnetists, and many of the other phenomena subsequently associated with the term hypnosis, lie in the trance-like appearance of, and many of the behaviours shown and reportedly experienced by, subjects during Mesmeric 'crises'. These behaviours included reverie, drowsiness, delirium, catalepsy, automatism, amnesia and analgesia (see, for example, Sheehan & Perry, 1976; Thornton, 1976; Wagstaff, 1981). Indeed, Laurence and Perry (1988) suggest that Mesmer may have been "the discoverer of artificial somnambulism" (p. 113), and it is notable that de Puységur used the word 'crisis' in reference to the state he later termed 'somnambulistic sleep' (Figuier, 1860). The idea that Mesmeric 'crises' involved a change in consciousness is explicitly acknowledged by commentators such as Mackay (1869), who remarked that, after manifesting a crisis, Mesmer's subjects were subsequently

* Department of Psychological Sciences, University of Liverpool, Eleanor Rathbone Building, Bedford Street South, Liverpool, UK, L69 7ZA

e-mail: gwf@liverpool.ac.uk

"restored to consciousness" (p. 279). Given this, Laurence's comment that Mesmer construed such phenomena as resulting from a neurophysiological process is beside the point; in fact, most modern proponents of the state position on hypnosis assume the existence of some kind of distinctive underlying neurphysiological process (Oakley and Halligan, 2010).

I would also argue that Laurence's emphasis on Abbé de Faria's penchant for blood letting rather misrepresents de Faria's important and highly insightful contribution to the subject. Central to de Faria's (1819) ideas was the concept of 'lucid sleep', which he described as a profound sleep-like condition, which differed from normal waking, and was accompanied by experiences of nonvolition. Significantly, de Faria argued that this condition arose through 'concentration', or focussing attention on sleep, together with the 'power of conviction'. In making the link between lucid sleep, suggestion and expectancy, de Faria was very much a precursor of Bernheim and the Nancy School. As such, de Faria's speculations about 'thin blood' are peripheral but, nevertheless, given medical knowledge at the time, understandable; he thought that thin blood was related to lethargy and a tendency to fall asleep, which might lead to greater susceptibility to the state of lucid sleep.

Continuing the theme of an 'altered state', Bertrand (1826) also argued explicitly that the phenomena associated with artificial somnambulism were the result of an imagination induced state "that differed from awakening, sleep and illness" (p. 3). Moreover, although D'Hénin de Cuvillers (1820) rejected most of the extravagant claims made by the magnetists, the reason why he chose to apply the prefix 'hypn' to a range of actors and phenomena was that he considered the term to be particularly appropriate to situations in which subjects presented with the appearance of being in a trance like state of somnambulism. Braid (1843) also explicitly used the term 'hypnotic state' to refer to a condition that differed from 'the waking condition', brought about primarily by eye fixation and concentration. Like many others, Braid eventually attempted to distance the hypnotic state from sleep; nevertheless, the association between hypnotism and the 'sleep-like' state of artificial somnambulism continued to be popular throughout most of the 19th century and into the beginning of the 20th century (see, for example, Bechterew, 1906; Liébeault, 1866; Lyman, 1885). Indeed, even though Bernheim made a distinction between 'hypnotic sleep' and the broader conception of hypnosis as a 'peculiar psychical condition', references to 'hypnotic sleep' and mention of the accompanying change in consciousness, are still very evident in his writings (see Bernheim, 1889). Again, Charcot's view of hypnosis was that it was a trance-like condition that involved three stages, catalepsy, lethargy and artificial somnambulism, which involved changes in behaviour, perception and experience and differed from what he termed "the state of waking" (1889, p. 307; see also Ellenberger, 1970; Sheehan & Perry, 1976). In sum, notwithstanding the considerable differences in emphasis and explanation, I think it reasonable to propose that the origins of 'hypnosis' and related terms lie in the idea that hypnosis involves some kind of altered state or condition of consciousness that is associated with changes in perception and experience.*

*Terhune alludes to the idea that if we want to stick to the etymological roots of hypnosis the definition should include some reference to 'sleep". One could actually make such a case. For example, metaphors related to sleep continue in many of induction procedures still in use, and, even in the modern era of hypnosis, some have attempted to maintain a connection between hypnosis and phenomena related to sleep (see, for example, Barrett, 1979; Evans 1982). However, given that, in practice, even most supporters of the altered state position no longer make any such connection, I would argue that, on balance, this would make the definition needlessly out of line with most modern scientific opinion.

Moving on to hypnotic depth, Laurence argues that conceptions of hypnotic depth have varied according to different underlying theoretical approaches. This is undoubtedly the case, and is reflected in the discussion of the meaning of depth reports in my paper. However, I would question his statement that, "At the end of the nineteenth century the Nancy school linked the depth of hypnosis to the number of items passed not the depth of the state" (p 110). Put this way, Bernheim's ideas seem problematic; the idea that hypnosis has various degrees of depth independent of the idea of a hypnotic state or condition, sounds like a contradiction in terms. However, what Bernheim (1889) actually says is that it is specifically the presence of 'hypnotic sleep' (not hypnotic state or condition) that does not necessarily correspond to passing suggestions. As previously noted, to Bernheim, the hypnotic condition was not the same as hypnotic sleep, rather it was more broadly defined as a 'peculiar

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psychical condition' related to hypersuggestibility; hence suggestions can more reasonably be used to estimate the achieved depth of the 'peculiar psychical condition'. Accordingly, Bernheim (1889) describes a suggestion based scheme (later referred to as the 'Bernheim Depth Scale', see Sheehan & McConkey, 1982), which he says measures the "different degrees of the hypnotic condition" (p. 10). To Bernheim, therefore, 'depth of hypnosis' did not mean 'degree of responsiveness to suggestions'; rather it meant depth of the *peculiar psychical condition* associated with changes in responses to suggestion (hypersuggestibility). In doing so, Bernheim was one of the early pioneers of the idea of using suggestions to measure the hypnotic state or condition, which, as I noted in my paper, was the rationale behind the original suggestion based scales of hypnotizability. Indeed, Laurence notes that most typical contemporary standardized inductions are still worded along the depth dimension.

Laurence also questions the validity and meaning of depth reports, arguing that "Experienced depth may mean very little other than the participants espousing the metaphors we provide them" (p. 110). However, from the hypnosis as a suggestion position, I would submit that this criticism is misplaced. The idea of hypnosis as a metaphor is not a problem for depth reports, rather it is fundamental to understanding how they operate. According to the hypnosis as a suggestion idea, the main reason why participants make self-attributions about hypnotic depth is precisely because they are responding to the metaphor of hypnosis as an altered state. In other words, hypnotic depth reports are for the moment the best measure we have of whether participants have accepted the suggestion for (or metaphor of) hypnosis (which may then have further effects on behaviour and experience). And for the same reason, depth reports, like standard measures of hypnotic suggestibility, are sensitive to attempts to manipulate how participants construe this metaphor (Barber, 1969; Silva & Kirsch, 1987; Spanos, 1986; Spanos & Chaves, 1989; Spanos, Cobb & Gorassini, 1985; Wagstaff, Cole & Brunas-Wagstaff, 2008).

Laurence then goes onto to argue that my definitions will not change anything. Perhaps not, but I can perceive ways in which, if accepted, they might. For example, when investigating 'hypnosis', researchers might have a clearer and more unified idea of how to test and measure relevant constructs. Hence, they would agree that an investigation into the effects of suggestion per se (though obviously important) is not necessarily an investigation into 'hypnosis'; they might be able to agree more on what does and what does not constitute a hypnotic induction procedure; they might attempt to test, or take account of, the idea that hypnosis is not just what happens when someone is given an induction, rather than ruling out the possibility 'by definition'; they would agree that 'hypnotizability' is not the same thing as 'hypnotic suggestibility', and that accurate measures of both hypnotizability and hypnotic suggestibility require more than simply measuring responses to suggestions after an induction, and so on. Of course, as Laurence emphasizes, the definitions in themselves will not prevent disagreements about whether or not the concept of a hypnotic altered state is a useful explanatory construct, or whether the belief that one is 'hypnotized' fits best with the available data. Personally, I share his reservations about the altered state construct as an explanatory device and prefer the hypnosis as a suggestion (for an altered state) approach (Wagstaff, 1998; 2004); but at least these issues will remain central to the debate, and the continued subject of empirical scrutiny. The idea that the way hypnosis is defined can potentially have a fundamental effect on the way research is conducted and interpreted is something to which I will return in my response to Kirsch.

Laurence also makes the more general criticism that, in practice, definitions of hypnosis are of little value at present because "we still do not understand what exactly is happening during hypnosis" (p. 111). However, again, the question I would pose is, how can we possibly understand what is happening in hypnosis if we cannot even agree on what it is we are trying to understand? Moreover, I disagree with Laurence's view that saying

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something is 'alleged' pre-empts it from being a fundamental part of a definition. For example, an equivalent definition might be one typically applied to a Yeti, such as this from the Cambridge English Dictionary: "a big creature like a human covered in hair that is *believed* by some people to live in the Himalayas" (Yeti, 2013, my emphasis).

Finally, Laurence argues that my definitions are no advance on "a simple descriptive and operational approach" (p. 110). However, as he provides no example of such an approach for purposes of comparison, I am not able to comment further on this. Nevertheless, I do comment shortly on the problems related to a simple procedural definition provided by Terhune, which may be relevant to Laurence's view.

Woody and Sadler

Much of the first part of Woody and Sadler's commentary (this issue) seems to concern apparent similarities and differences between the views I put forward in the present paper and one I wrote 15 years ago (Wagstaff, 1998). Whilst I am not entirely clear as to the relevance of some of this to discussion of the present definition, I will try to address these comments, nevertheless.

In my earlier 1998 paper I argued that to explain hypnotic phenomena, we do not need to postulate the existence of some special hypnotic process or altered state *in addition to* other ordinary psychological variables such as motivation, relaxation, imagination, absorption, expectancies, attitudes, beliefs, concentration, suggestibility, placebo effects, selective attention, role-enactment, compliance with instructions etc. To do so could be construed as a category error; because the phenomena we attribute to hypnosis, including reports of feeling hypnotized, and subsequent changes in behaviour and experience, are ultimately explicable in terms of combinations of exactly these same variables. Woody and Sadler seem to think that I have now abandoned this position. However, as far as I am concerned I still hold to it; moreover, I see no inconsistency between the ideas put forward in the present paper and this perspective.

Woody and Sadler then go on to argue that if one believes one is in a hypnotic altered state of consciousness then one actually is in an altered state, and claim I have now adopted this position. However, whilst I can see some potential merits in this conceptualisation for uniting different shades of opinion, I have not actually adopted this position. I see a number of issues. For example, whilst not rejecting the idea that some hypnotic subjects may experience what could be construed as shifts in the background state of consciousness* because of the nature of the inductions given to them (relaxation, concentration, absorption, arousal, distraction etc.), to say that a person is in *an* "altered state of consciousness associated with etc.", implying a particular kind or type of altered state, simply because he or she decides to label potentially any number of different experiences in this way, seems to me to be a rather empty and overextended use of the idea of 'an altered state of consciousness'. I am also not clear as to how Woody and Sadler perceive the status of this kind of suggested hypnotic altered state vis-à-vis other candidates for the title of hypnotic state. For example, can one be in an altered state of hypnosis without believing that one is in one? And if so, is this latter state the same or different from the altered state that arises when one believes one is hypnotized? But, in any case, none of this is really pertinent to the point I was trying to make in the 1998 paper, which was simply that, for the idea of hypnosis as a suggestion for an altered state to be useful in a definition of hypnosis, or as a way of categorising someone as 'hypnotized', it is actually unnecessary to make any assumptions about whether, in reality, the participant *is* in an altered state of consciousness. I see no reason to change my position on this.

Woody and Sadler further suggest that the definition of hypnosis I propose is an "awkward combination of different things" (p. 113), and give an example of how the syntax involved can look rather strange: i.e. "Obesity can be defined as 1) an alleged altered state of weight . . . or 2) acceptance of the suggestion that one is in such a condition" (p 113). Given

* I am grateful to Professor Irving Kirsch for drawing my attention to the possible relevance of the concept of background state of consciousness to this issue.

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the fairly common consensus that obesity is a medical condition involving excess body fat (not an 'altered state of weight' associated with suggestion), and is something that can be objectively measured in various ways, this definition does, of course, look silly. However, the following definition of a more contentious phenomenon might appear less so: "Spirit possession is the alleged control of someone by a spirit or other disincarnate entity, or the belief that one is being controlled in this way". Moreover, I would argue that it is not uncommon in definitions of scientifically contentious phenomena to use terms that combine what *is* with what is *alleged* or *believed*, as in my previous example of the term 'Yeti'.

Woody and Sadler also state that the definition I provide is too simplistic. Thus they say, "Over the years we have repeatedly pointed to a wide array of evidence that hypnosis is a complex amalgam of social experiences and cultural expectations, individual differences in multiple underlying abilities, and important contextual factors . . . It cannot be reduced to one thing (i.e., the belief that one is in an altered state)" (p. 113). However, I would argue that this is not relevant to the definition of hypnosis per se, rather it is a statement about the factors that influence and contribute to 'hypnosis' as defined, and the phenomena associated with it. We would not argue that a definition of a child as say, "a human between the stages of birth and puberty", is too simplistic because children's bodies are made up of millions of parts and their behaviours are influenced by a complex amalgam of factors. In fact, there is no contradiction between the simple definition of hypnosis as acceptance of the suggestion that one in an altered state (as per my definition), and the view that the way this attribution is made, and its manifestations in terms of effects on behaviour and experience, are determined and influenced by a multitude of factors. Indeed, the multifaceted nature of hypnotic behaviour and experience is one of the hallmarks of the sociocognitive or cognitive behavioural position to which the hypnosis as a suggestion or belief proposition is closely allied (Wagstaff, 1998; 2004).

Finally, Woody and Sadler take exception to the use of the term 'imaginative experiences' which I left in my amended version of the 2003 APA definition; i.e. "A hypnosis procedure will typically involve an introduction to the procedure during which the subject is told that suggestions for imaginative experiences will be presented" (Green, Barabasz, Barrett, & Montgomery, 2005, p. 262). Their main objection is that hypnotically suggested phenomena have a veridical or realistic quality that distinguishes them from imagined events. I agree with Woody and Sadler that, in their preambles, most of the standard hypnotizability scales do not explicitly refer to anything 'imaginative'; Cardeña makes the same point and I will address this in my response to him. However, it could be argued that Woody and Sadler's main objection here is based on a false distinction. Even if hypnotic suggestions are experienced differently (more veridically) than simple instructions to 'use the imagination', this does not mean that profound responses to hypnotic suggestions are not 'imaginative experiences'. For example, like any hallucinatory experience, even if they show equivalent neurophysiological effects, so-called 'real as real' hypnotic hallucinations are not 'real' in the sense of corresponding to or reflecting the influence of an external stimulus; i.e. they are still 'products of the imagination'. Similarly, the Creative Imagination Scale, with or without hypnotic induction, explicitly employs 'imaginative suggestions'; but this is not meant to imply that suggestions cannot be experienced with a veridical quality. Indeed Kirsch and Braffman (1999) describe imaginative suggestions as "requests to experience an imaginary state of affairs as real" (p. 226). Hence, I would argue that the issue addressed in the studies cited by Woody and Sadler in this respect concerns a distinction between the intensity and quality of imaginative experiences following hypnotic suggestions versus non-hypnotic or simple 'imagination' instructions, not a distinction between non-hypnotic imaginative versus hypnotic non-imaginative experiences.

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Terhune

Terhune, like Kirsch, again questions my stance on relating definitions to etymology, so I will return to this later in my response to Kirsch.

However, Terhune also presents a number of arguments against the idea of hypnosis as acceptance of a suggestion for an altered state and the use of depth reports as a measure of hypnotizability. For example, he argues that, “Hypnotic inductions do not produce uniform changes in spontaneous experience across, nor within, levels of hypnotic suggestibility . . . and thus spontaneous experiences following an induction do not provide a reliable method of determining whether something qualifies as hypnosis nor how responsive someone is” (p. 116). I would make two main points here. First, according to the hypnosis as a suggestion position, it is not the spontaneous experiences per se that people have following induction that determine whether they accept the suggestion for hypnosis; rather it is the willingness to *interpret* or *label* the context and their experiences as ‘hypnosis’ that determines whether participants decide they have been hypnotized (and thus how they rate their level of depth). Indeed, in my paper I make specific reference to this issue by suggesting that the reason why depth reports correlate so well with suggestion measures of hypnotizability, is not because they accurately reflect the experiences of an altered state or states, but rather they reflect acceptance of the suggestion for hypnosis. But, most important, it is well established that simple self ratings of ‘feeling hypnotized’ (hypnotic depth reports) are actually psychometrically as reliable as suggestion based measures; moreover, the direction of the difference between the depth reports of participants classified according to levels of hypnotic suggestibility is highly reliably maintained throughout a series of suggestions of varying difficulty and on separate occasions; i.e. depth scores show parallel non-overlapping within and between suggestibility tests. Depth scores also correlate as well with suggestion based scales as the suggestion based scales do with each other (Tart, 1970; Wagstaff et al., 2008).

Terhune further suggests that the issue of whether someone is or is not hypnotized “does not provide valuable information above and beyond standardized behavioural and experiential measures of hypnotic suggestibility” (p. 116), and comments that, “Considered within the context of a procedural definition, the (aforementioned) example of the participant who is slightly responsive to hypnotic suggestions but does not believe they s/he was hypnotized indicates that the participant took part in a hypnosis protocol and displayed a certain level of hypnotic suggestibility. There are no complications regarding whether or not the person was hypnotized” (p. 116). As I point out in my paper, purely procedural conceptualisations of hypnosis such as this are highly problematic. For example, if we have no definition of hypnosis beyond a procedure, then what exactly is a ‘hypnosis protocol’ as distinct from a protocol that is not one? And what sense can we make of the idea that the person was displaying ‘a certain level of hypnotic suggestibility’ when we do not know whether he or she was even affected by the hypnosis protocol? This is like saying that that ‘alcoholic behaviour’ is behaviour that follows the offer of an alcoholic drink, even if the drink is refused.

Next Terhune outlines a number of further objections to the use of the depth reports. He says, “*contra* Wagstaff’s claim to the contrary, hypnotic depth measures are actually *far* inferior to measures of hypnotic suggestibility. The instructions for depth reports are overly broad and thus it is not clear what participants are basing their depth reports on. Added to this, hypnotic depth is a gross over-simplification of the panoply of spontaneous experiences that follow a hypnotic induction, particularly those in highly suggestible individuals. Relying on a single numerical value of an individual’s spontaneous experiential response to an induction is untenable because participants will likely differentially weight particular experiential dimensions when computing this value and thus hypnotic depth values cannot be meaningfully compared across individuals” (p. 116; his emphasis).

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Notably this statement makes no reference to the object of the measurement; i.e. far inferior for measuring what? As I mentioned in my paper, as measures of hypnotizability, standard suggestion based measures notoriously confound hypnotic and non-hypnotic suggestibility (see, for example, Braffman & Kirsch, 1999; Kirsch & Braffman, 1999; Weitzenhoffer, 1980). This issue does not arise with depth scales (Wagstaff et al., 2008). Hence, according to the arguments in my paper, hypnotic depth reports are, as yet, probably the best measure we have of hypnotizability or hypnotic susceptibility, i.e. the ability to enter hypnosis defined as an alleged altered state etc., or ability to respond to or accept a suggestion that one is in such a condition (see also Bowers, 1983; Wagstaff et al., 2008). However, hypnotic *suggestibility*, which in my definition is the ability or tendency of a person to respond to suggestions when 'hypnotized' (i.e. in the alleged state, or having accepted the suggestion for one), is possibly measured most accurately by a combination of depth reports and responses to suggestions.

Nevertheless, notwithstanding all this, I would argue that Terhune's comments here miss the point. As I mentioned earlier, although interesting as a research question, from the perspective of the hypnosis as a suggestion position, for measuring hypnotizability, what ultimately matters is not what kinds of experiences participants base their reports on, or exactly how they weight their experiences, but rather the *outcome* of the attribution process; that is, do they believe themselves to be 'hypnotized' and to what degree? As Sheehan and McConkey (1982) have well documented, the experiences and processes that underlie responses to hypnotic suggestions in general are also idiosyncratic, diverse and complex, but this has not prevented researchers from using the outcomes of these experiences and processes, i.e. simple behavioural and subjective rating criteria, to measure hypnotic suggestibility. Also, the fact that Long Stanford Scale style depth reports rely on a single numerical estimate does not, as a matter of principle, make them unreliable or invalid. These are empirical matters. As noted previously, simple hypnotic depth reports are demonstrably as reliable and valid in terms of criterion based validity as the standard suggestion based measures; moreover, there is no evidence that more complex experiential measures of hypnosis, such as the Phenomenology of Consciousness Inventory (PCI) perform any better (Wagstaff et al., 2008).

To illustrate the predictive power of depth reports in measuring hypnotizability, it may be also useful to look at the issue from another perspective. It is generally agreed that whatever hypnosis is, it is enhanced or facilitated by hypnotic induction procedures (otherwise there would be no point in using them). Consequently, if a measure is a good measure of 'hypnotizability', in the sense of capturing participants' responses to whatever it is that is induced or influenced by a hypnotic induction procedure, then from scores on this measure it should be possible to predict statistically who, and who has not, been given a hypnotic induction procedure on a particular occasion. Using this criterion, depth scores will almost always outperform suggestion-based measures for a very obvious reason; unlike scores on most standard suggestions, in conditions where there is no induction, or anything that could be construed as an induction, or used to label the context as 'hypnotic', then LSS scores tend towards zero (Wagstaff et al., 2008).* However, though strikingly obvious and predictable, this is not a trivial point. As noted in my paper and elsewhere, this finding actually offers strong support for the construct validity of depth reports as a measure of hypnotizability; i.e. depth reports tend towards zero in a situation in which, according to the main perspectives on hypnosis, one would not expect hypnosis to be present (at least to any appreciable degree). In contrast, the fact that scores on the standard suggestibility tests continue to be substantial, and to differentiate between individuals in the absence of a context that could be construed as 'hypnotic', severely compromises their construct validity as measures of hypnotizability (which is why I suggest they are better considered as a proxy measure). In sum, given that, empirically, depth reports are at least as reliable as suggestion based measures of hypnotizability, and appear to be superior in terms of construct validity, I can see no empirical basis for Terhune's conclusion

* As an example of predictive value of depth reports, consider the following data that were collected by Chantal Worden and the author. Forty participants were assigned to two conditions, one with and one without hypnotic induction. Those in the hypnotic induction group were given a standard relaxed induction technique adapted from Barber (1969). Following this, they were asked to rate their level hypnotic depth on the LSS (Tart, 1970). They were then given the SSHS:A suggestions for hand lowering, finger lock, arm rigidity and hands moving, and were required to rate their responses to each suggestion on a five point Likert scale according to the degree they experienced the suggested effects ("not at all" to "very much", giving a minimum score of zero and a maximum of 16). As predicted, results showed that both depth and experiential suggestion scores were significantly higher in the induction condition (medians, 2 and 8, respectively), than in the non-induction condition (medians, 0 and 6, respectively), Mann-Whitney $z = 5.47$, $p < .0001$, and $z = 3.022$, $p < .004$, respectively. However, the large effect size for depth ($r = .87$), was considerably greater than the more moderate effect size for suggestion ($r = .48$). Moreover, when depth and suggestion were entered into a binary logistic regression, only depth significantly predicted whether participants had been assigned to the induction and non-induction conditions, $\chi^2(1) = 4.04$, $p = .044$, and $\chi^2(1) = 0.99$, $p = .321$, respectively. (The correlations between depth and suggestions passed were, $r_s = .74$, $p < .001$, and $.18$, $p < .45$, within the induction and non-induction groups, respectively.)

† Terhune is critical of my statement that depth reports can outperform hypnotic suggestions in predicting hypnotic amnesia, in that the data I report concern only one measure of (...suite next page)

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(...suite)
amnesia; for other measures depth and suggestions predicted amnesia more or less equivalently. Terhune is correct in this observation; I should have made this clearer (see Wagstaff et al., 2008). Nevertheless, these data still clearly contradict Terhune's general view that, "hypnotic depth measures are actually far inferior to measures of hypnotic suggestibility".

that, "hypnotic depth measures are actually *far* inferior to measures of hypnotic suggestibility" (p. 116).†

Terhune finishes by proffering his own procedural definition of hypnosis; i.e. "hypnosis consists of a set of procedures including a hypnotic induction, *intended* to modify suggestibility, followed by the administration of one or more suggestions, *intended* to measure hypnotic suggestibility, modulate a particular psychological phenomenon, or treat a specific symptom" (p. 116; his emphasis). The problems commonly associated with this kind of procedural definition have already been covered in my paper. Again, these include, what is the definition of a hypnotic induction procedure? Is it any procedure that intends to modify suggestibility? If so, presumably we should call any procedure that is intended to motivate participants to respond more to suggestions (such as offering a reward) 'hypnotic'. Moreover, what is 'hypnotic suggestibility' as distinct from 'non-hypnotic suggestibility' or just 'suggestibility'? This distinction is implied but not given in the definition. If hypnotic suggestibility is simply 'suggestibility with or after a hypnotic induction procedure', and an induction procedure is 'any procedure intended to modify suggestibility', some of the implications might seem rather counterintuitive. For example, in the absence of any attempt to modify his or her behaviour, a person who says he or she feels 'very deeply hypnotized', and responds both behaviourally and experientially in an unusually compelling way to a range of suggestions, cannot be exhibiting 'hypnotic suggestibility'; indeed, by definition, his or her behaviour cannot have anything to do with 'hypnosis'. In contrast, someone who has been given a procedure that involves offering him or her a few dollars to motivate him or her to respond to suggestions, and who subsequently responds superficially to one simple suggestion (to which he might have responded anyway), yet denies feeling even the slightest bit hypnotized, *is*, by definition, engaged in 'hypnosis', and is displaying 'hypnotic suggestibility'. Whilst the interpretation of such scenarios might be logically coherent within a procedural definition, I am not sure they would make much sense to most hypnosis practitioners, let alone to participants.

Within the definition I have put forward, a hypnotic induction procedure is specifically a set of instructions and suggestions designed to facilitate entry into the alleged hypnotic state, or convey the suggestion that one is entering such a condition. From this viewpoint, therefore, whilst the *purpose* of a hypnotic induction procedure may be the same has that specified by Tehune, i.e. it is 'intended to modify suggestibility', this is not the same as its *definition*. The way I see it, without this kind of consideration of what is actually meant by a 'hypnotic induction' procedure, as distinct from any other kind of motivating or orienting procedure, attempts to define hypnosis operationally in terms of a procedure or set of procedures will inevitably run into problems. Moreover, to meaningfully define what uniquely characterises 'hypnotic' induction procedure, it is difficult to avoid invoking a further overarching definition of 'hypnosis' that is non-procedural (such as, hypnosis is an alleged altered state or acceptance of the suggestion for one etc.).

Polito, Barnier and McConkey

Polito et al. (this issue) provide a rather more supportive commentary of my paper, though they argue that it is better if a definition can be formulated that fits with, or is driven by available empirical data rather than derived from etymological or ideological reasoning. I would agree that, ideally, this sounds the best approach. However, as I attempt to demonstrate in my paper, in practice, this is often very difficult to do when the terms already exist. Moreover, as I point out in my response to Laurence, it is also very difficult to conduct meaningful empirical investigations when we cannot even agree on the essential subject matter of our investigation. Nevertheless, I would argue that my definition is still very much influenced by the results of empirical research. Hence the use of the concept of an 'altered state etc.', without any reference to sleep, reflects the modern views of those who believe the empirical research supports this idea. Moreover, the term 'alleged' and the idea of 'acceptance of

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the suggestion that one is in an altered state', have been added to reflect the views of those (including myself) who, also influenced by empirical research, are more sceptical about the utility of postulating a hypnotic altered state, or place less emphasis on the idea.

However, the main point that Polito et al., make is that, although hypnotic inductions may increase the probability that participants will respond to suggestions both behaviourally and experiencially, they are neither necessary nor sufficient for this to happen. Polito et al. provide a number of interesting examples to illustrate their point. As they emphasize, their examples add to a large literature showing that a range of factors may contribute to alterations in behaviour, perception and experiences in response to suggestions, beyond the administration of hypnotic induction procedures. On the basis of this, they then comment, "most participants in these studies experienced hypnosis-as-product because they had the ability to do so, and the relevance of an induction procedure was dependent on the specific context" (p. 120). In other words, some people can experience 'hypnosis as a product' without an induction. This fits with my analysis and appears to be a rejection of Terhune's position. However, it can still be argued that, unless we have some other way of determining whether someone has been 'hypnotized' independently of their responses to suggestions, how can we possibly know that someone is experiencing 'hypnosis as a product' when they have not received an induction? The fact that someone may respond profoundly to suggestions without an induction does not necessarily mean they are hypnotized or showing 'hypnotizability'; they could just be exhibiting 'non-hypnotic suggestibility'. Of course we could argue that anyone who responds profoundly to suggestions is 'hypnotized', or showing 'hypnotic ability', but again, without some independent measure of the presence of 'hypnosis', this is circular and begs the question.

In the absence of any definitive indicators of a 'hypnotic state' that can be used with participants who have not been given induction procedures I would suggest that, for the moment, depth reports may provide the best indicator of whether 'hypnosis' is present in participants who have not received an induction (for an example of the use of depth reports in this way, see Hilgard and Tart, 1966).

Cardeña

Like Woody, Cardeña (this issue) correctly points out that, regardless of the role that imagination may play in response to the kinds of suggestions typically used in contexts defined as 'hypnotic', many inductions do not explicitly refer to the concept of 'imagination'. However, there is some ambiguity surrounding the use of the term 'imaginative suggestions' in the 2003 APA definition, as some authorities use the term 'imaginative suggestion' to denote more generally the kinds of suggestions associated with the domain of hypnosis (see, for example, Kirsch & Braffman, 1999; Milling, Kirsch, Allen & Reutenauer, 2005, and Kirsch's commentary here). This may require further clarification in any amended APA definition.

Cardeña also emphasizes the important point that, although the label of hypnosis (whether explicit or implicit) may be necessary for inductions to facilitate responses to subsequent suggestions, and that a variety of inductions may produce similar effects, this does not mean that all inductions will have the same effects. I agree. According to the hypnosis as a suggestion perspective, for an induction to successfully facilitate responses to suggestion through 'hypnosis', the procedures and experiences engendered by those procedures must be consistent with the participant's expectations and assumptions concerning what it means to be 'hypnotized'. Not surprisingly, therefore, as I point out in my paper, the effectiveness of inductions can be substantively, and sometimes dramatically, affected by contextual and attitudinal variables (see, for example, Barber, 1969; Silva & Kirsch, 1987; Spanos, 1986). But also, from the same perspective, individuals are more likely to make the attribution that they have been 'hypnotized' if they have some evidence, such as changes in bodily experiences and experiential shifts in

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the background state of consciousness, that they can use to reinforce this attribution (see Barber, Spanos & Chaves, 1974; Hilgard, 1986). Hence inductions that do not facilitate such shifts may be relatively less effective in promoting the attribution that hypnosis has occurred. Cardeña's reminder that both relaxation and alerting inductions tend to be significantly more effective in enhancing than a label of hypnosis alone, seems pertinent here. Moreover, the findings reported by Cardeña that a long induction may be more effective than a very short one also could be construed as supporting this view, in that a lengthy induction might be more likely to a) fit in better with participant's expectations about what a 'hypnotic induction' should involve, and b) give greater opportunity to generate experiential changes that can be used to reinforce the attribution that 'hypnosis' is occurring.

Cardeña also makes some interesting observations about how shifts in experience generated by induction procedures may used by participants to reinforce their attributions that hypnosis is occurring and facilitate responses to suggestions; for example, inductions may encourage the redirection of attention away from extraneous concerns, result in a shift to a more experiential mental set, and facilitate experiences that for many are atypical in everyday life, such as those accompanying prolonged attention. As I point out in my paper, the idea that the experience of suggestion effects, particularly, involuntariness, can be facilitated through the redirection of attention, has been popular amongst theorists of a variety of persuasions (Crawford & Gruzelier; 1992; Egner & Raz, 2007; Spanos, 1982; Wagstaff, 2004). As Cardeña notes, these considerations also potentially allow comparisons to be made between the characteristics and effects of some induction procedures and other contexts, such as in certain rituals and cases of trauma (as well as meditation, autogenic training etc.). The question remains, however, as to whether any of these factors has any substantive effect on standard suggestibility measures divorced from the contextual label of 'hypnosis', or attributions by participants that hypnosis has occurred.

Notwithstanding these considerations, therefore, I would still argue that manipulating subjects' expectancies and beliefs as to whether hypnosis is present, and what hypnosis is likely to do, has considerably more effect on responses to suggestions following induction, than changing the mechanics of the induction procedure (Banyai & Hilgard, 1976; Barber, 1969; Glass & Barber, 1961; Gibbons & Lynn, 2010; Silva & Kirsch, 1987; Spanos, 1986; Spanos, Cobb & Gorassini, 1985).

Kirsch

Kirsch's observation that preferences for different types of definition do not seem correlated with theoretical stances on the altered state issue (this issue) is an important one. As a long time proponent of the sociocognitive perspective on hypnosis, I too have vacillated between various definitions. However, contrary to Kirsch's interpretation of my position, I do not actually argue that a broad definition of hypnosis *necessarily* leads to contorted terminology, such as 'hypnotic hypnosis' and 'hypnotic non-hypnosis'. Rather, the point I attempt to make is that such problems tend to occur when one tries to incorporate more traditional concepts and related terminology within a definition of hypnosis which equates hypnosis with responding to suggestion. As far as I can see, this is more or less the same point that Kirsch makes with regard to mixing up narrow and broad definitions. It is interesting to speculate, therefore, how we might go about defining our terms more consistently within a broader definition.

For example, suppose, to use Kirsch's words, we define hypnosis broadly as "responding to imaginative suggestions [with or] without the induction of hypnosis, regardless of the presence or absence of a hypnotic state" (noting, of course, that Kirsch is not actually offering an endorsement of this position; p. 124). Within this definition as stated, we would still need to define what is meant by 'the induction of hypnosis' and 'hypnotic state'; that is, we need to untangle the form, "Y is X with or without the induction of Y or a state of Y". This might have

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some significant implications. For example, if by 'the induction of hypnosis' we mean the presence or use of an hypnotic induction procedure, then if hypnosis is simply responding to imaginative suggestions ('hypnotic' does not refer to an altered state/ acceptance of a suggestion for one), then a hypnotic induction procedure would presumably mean something like, 'any procedure designed to increase responses to suggestions' (including offering a reward etc.). In fact, it might make more sense to abandon the term 'induction' altogether in favour of something less archaic, such as 'facilitation' (hypnosis facilitation procedure etc.). If we wanted, we could still allow the concept of a 'hypnotic state' to coexist with this broader definition (meaning something like, 'a state associated with changes in response to imaginative suggestions'); thus we could logically have '(hypnotic) state hypnosis', and 'non-state hypnosis'. However, the concept of 'non-hypnotic (imaginative) suggestibility' would have to go. Hence the comment on the HUUK (2012) website that, "People respond better to suggestions while in hypnosis" would become conceptually meaningless. In the same way, 'hypnotic suggestibility' as it is normally understood (i.e. as response to standardized scale type suggestions in the presence of hypnosis and/or after hypnotic induction) would have to disappear or be replaced by something like 'facilitated hypnosis' (otherwise it would become a tautology, equivalent to 'hypnotic hypnosis').* Other implications might include the alternative labelling of studies of primary suggestibility, such as those of Eysenck and Stukat, as studies of 'hypnosis or hypnotism'. Also, many practitioners of CBT and other psychological therapies using suggestive techniques might find themselves re-categorised as 'hypnotherapists'. In fact, depending on whether they use hypnotic induction/facilitation procedures, they could be categorised as practising 'facilitated' and 'non-facilitated' hypnotherapy. But perhaps most important, the results of years of research and numerous studies purporting to assess the effects of hypnosis by comparing responses to suggestions with and without hypnotic induction would have to be reinterpreted (or written off?) as demonstrating the effects of induction procedures (or suggestion facilitation instructions) on hypnosis, *not* the effects of hypnosis on suggestion. So, for example, by definition, using the hypnotic induction versus no induction paradigm, Woody would not be able to test the hypothesis that suggestions are experienced more veridically with hypnosis, only whether hypnotic experiences are more veridical after attempts have been made to facilitate them.

* It can be noted here that one could argue that "hypnotic suggestibility" is equivalent to responsiveness to primary or imaginative suggestibility, because one must be in a hypnotic state to experience these kinds of suggestions. However, apart from begging the question, this is simply another way saying that what essentially distinguishes hypnotic and non-hypnotic suggestibility is the presence of an altered state of consciousness; i.e. hypnosis is defined as an altered state of consciousness.

How well such a definition might be received amongst academics and other hypnosis professionals is difficult to say. I would imagine that the abandonment of the idea that one can respond to (imaginative) suggestions "in and out of, or with and without, hypnosis", could be a major sticking point. My personal view is that it makes a lot more sense to construe hypnosis, as did Bernheim, as belonging within the broad domain of suggestion, rather than taking the view that suggestion (or at least imaginative suggestion) is within the broad domain of hypnosis (which to me, is a bit like categorising a motor vehicle as a type of bus). However, it might be interesting to conduct a survey of hypnosis researchers and practitioners, pitting a detailed version of this broad definition against my more narrow definition.

Like Terhune, Kirsch also questions my arguments regarding adherence to the use of the etymological origins of terms. Kirsch draws attention to the fact that some scientific definitions can and have changed over time. This is, of course, true. However, at the same time one can also draw attention to numerous scientific concepts that have been accepted as false or are the subject of scepticism and differences in opinion, but whose definitions have not changed to suit (cold fusion, spontaneous generation, phlogiston theory, luminiferous aether, phrenology, alchemy , transmutation of species, vitalism, telegony, Miasma disease theory, mental telepathy, graphology, acupuncture etc.). However, it is important that this issue is not turned into a straw man. I am not advocating that, as a matter of principle, scientific definitions should of necessity conform to natural language and be constrained

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by their etymology. Indeed, as I note in my response to Polito, Barnier and McConkey, my definition actually deviates somewhat from the strict etymological roots of hypnosis and related terms. Rather, my arguments are that, 1) definition and explanation are not the same thing; i.e. to make sense, definitions do not need to conform to scientific opinion or accommodate a range of opinions; 2) in practice, semantic and conceptual problems will invariably result if one ignores etymology and tries to 'neutralise' definitions in an attempt to accommodate different theoretical and explanatory viewpoints; 3) modern attempts to define hypnosis are very much prone to this kind of difficulty; and hence 4) there may be merit in attempting to formulate a definition of hypnosis that conforms more closely with its etymological roots.

Kirsch goes on to question the utility of hypnotic suggestibility scales as indirect proxy measures of hypnotizability. I tend to agree with him on this point. The main value of proxy measures generally is that they can be used when more direct measures are not available or are more difficult to administer. However, given that depth report scales are so easy to administer it is difficult to see how this argument would favour the use of imaginative suggestibility scales in most hypnosis contexts. It could be argued perhaps that, in practice, suggestibility measures can provide the researcher with useful and important additional information about how participants will respond to other suggestions; but even then, depth reports after induction appear to be as good predictors of responses to imaginative suggestions as imagination suggestions are of themselves (Wagstaff et al., 2008). Also, in practical terms, because of the time involved in administering the standardized suggestion based scales, depth scales may have a further advantage in terms of efficiency. So, in the end, perhaps the best argument I can come up with for advocating imaginative suggestibility measures as proxy measures is simply that they have been used extensively in the past studies, so, retrospectively, they are all we have to go on when interpreting these studies. Also, if Terhune's commentary is anything to go by, many people will continue to show a preference for using them.

Connors

Connors (this issue) asserts that, "the distinction that Wagstaff draws between hypnotic suggestibility (responsiveness to suggestions whilst in the hypnotic state) and hypnotic susceptibility (the ability to enter a hypnotic state) can be expressed without invoking the construct of state: that is, as responsiveness to suggestions and self-perceived engagement in the hypnotic experience respectively" (pp. 126–127). I have been over what I perceive to be the problems with these types of definition a number of times, both in my original paper and my responses to other commentators, so I will not detail them again here. Suffice it to say, if hypnotic suggestibility is simply responding to suggestions, and, therefore, by definition, hypnotic susceptibility is 'self-engagement in experience of suggestions', there is nothing to distinguish hypnotic suggestions from any other kinds of suggestions, hypnotic experiences from any other kinds of suggested experiences or 'hypnotic induction procedures' from any other kind of procedure that could be used to motivate people to engage in responding to suggestions. In effect, terms such as 'hypnosis', 'hypnotic susceptibility' become redundant, equivalent to 'suggestion' and 'suggestibility', and concepts such as hypnotic and non-hypnotic suggestibility end up as meaningless tautologies equivalent to, 'suggestive and non-suggestive suggestibility'. (For the possible implications of narrowing down hypnosis more specifically to responses to 'imaginative suggestions', see my previous response to Kirsch.)

Connors goes on to argue that the concept of an altered state is problematic in various respects; for example, it is not clear whether it is to be used simply as a descriptive concept or whether it is supposed to have a causal role in the production of hypnotic phenomena. I explicitly make reference to this issue in my paper and have deliberately worded my definition of hypnosis as an altered state so that it is neutral on the causality issue. Nevertheless, as I have noted previously, as a long time advocate of the non-state socio-cognitive view, I very much share Connors' reservations about the utility

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of the altered state concept as an explanatory construct in relation to hypnosis. Indeed, this why my personal preference is for the hypnosis as a suggestion idea (see also, Wagstaff, 1998, 2004). But, having said this, I would still maintain that, notwithstanding disagreements about the exact characteristics of the altered state concept, a definition of hypnosis that includes this idea makes far more sense than one that equates hypnosis with suggestion. Moreover, to meaningfully define hypnosis in terms of an 'alleged' altered state of consciousness, neither Connors nor I have to commit to the idea that such a state actually exists, or that it influences behaviour, any more than we have to believe in the reality of spirits of the dead to define what is meant by 'séance'.

Continuing, Connors states that "both definitions—at least as quoted by Wagstaff—define an altered state entirely in terms of the subject's own self-report, which may overlook other aspects of hypnosis, such as the interpersonal interaction involved and the behavioural responses that it can produce" (p. 127). In response, I would point out that neither element of my definition defines hypnosis in terms of verbal report, entirely or even in part. In fact, in defining the essence of hypnosis, I make no reference at all to verbal report. The only reference to verbal report comes later with the argument that, in the absence of a set of discrete physiological or behavioural markers, at the moment, perhaps depth reports may be the best way we have of measuring hypnotizability. Moreover, neither changes in behaviour nor social interaction have been 'overlooked'. The fact that hypnosis may elicit changes in behaviour is explicitly acknowledged in the definition. And 'social interaction' is not mentioned as a defining feature of hypnosis because it is generally accepted that hypnosis often occurs in the absence of social interaction (as in self-hypnosis).

Connors further suggests that the concept of increased suggestion is not critical to hypnosis. I do not see the problem here. I make exactly the same point in my article (twice, with supporting references); indeed, I point out that, in some circumstances, a hypnotized person may show a significant decrease in suggestibility. Hence, although some have equated hypnosis with hypersuggestibility, in my definition, I state only that hypnosis is a condition "normally associated with increased suggestibility etc.", and I add a supporting comment explicitly acknowledging this issue.

In Connors view, post-hypnotic suggestion also poses a problem for the hypnosis as a suggestion position. However, again I fail to see the difficulty. From the hypnosis as a suggestion position, a post-hypnotic suggestion is simply a suggestion given to someone in a context defined as hypnosis to respond in a particular way when they are no longer in that context. As a result, particularly for those who believe they have been hypnotized, the use of post-hypnotic suggestions within the hypnosis context may raise expectancies and motivation to respond to cues delivered outside the context of hypnosis; there is no need to postulate that participants need a second suggestion to 're-enter a hypnotic state' to respond to such cues.

Connors ends his commentary by citing Kihlstrom's definition as a possible alternative. Unfortunately, however, if we apply Kihlstrom's definition in unmodified form, we seem to end up more or less back where we started with the APA and BPS definitions: i.e. "hypnosis is a process in which one person, designated the hypnotist, offers suggestions to another person etc." Apart from the fact that it implies that hypnosis cannot exist in the absence of a social interaction (i.e. self-administered suggestions and resulting experiences do not qualify as hypnosis), there is nothing in the definition that explicitly tells us what characteristics apply to the designation 'hypnotist' (as distinct from say, a therapist or experimenter), what exactly constitutes a hypnotic suggestion as distinct from any other kind of suggestion, and what would constitute a hypnotic induction procedure. Interestingly, however, it may be possible to make some inferences in these respects. For example, if, as the definition states, 'classic' hypnotic suggestions are suggestions that are experienced in an especially compelling way and

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reflect 'altered states of consciousness', then presumably, one could infer that what essentially distinguishes hypnotic and non-hypnotic suggestion is the presence or otherwise of 'altered states of consciousness'. If so, then presumably a hypnotic induction is a procedure designed to induce these altered states of consciousness, and a hypnotist is someone whose specific role is to induce such altered states. Moreover, a hypnotizable or hypnotized person is not simply someone who 'experiences suggestions', it is someone who experiences 'altered states of consciousness' and shows unusually high levels of suggestibility (displaying delusions, compulsion etc.). I would argue that this is beginning to look very familiar.

O'Neil

O'Neil (this issue) begins his commentary by questioning the need for what he terms a 'final definition' of hypnosis, and argues that there are semantic and empirical hazards to a 'final' definition". As an example of the former, he then refers to what he perceives to be a problem of self-reference in the Oxford Dictionary's definition of the term 'definition' (which I cite in my paper). As far as I can see, this is a red herring. Whilst the Oxford Dictionary may involve an element of self-reference in its definition of 'definition', O'Neil does not actually specify any semantic problems with my particular definition of hypnosis. In the next part of his commentary, he then states that the two main components of my definition are 'lame' and (to his apparent gratification) the definition has failed as a 'final definition'. These judgments appear to be based on the fact that the main components of my definition are non-committal with regard to whether an altered state we can meaningfully label as hypnosis actually exists. Hence, he goes on to say, "We intend our words to designate real entities or phenomena, apart from how accurately they do so", and, "I assume that 'hypnosis' intends to designate a real state of consciousness which is different from the average expectable alert waking state" (p. 129). I fail to see the logic of this argument. It does not follow that because some words may designate real entities or phenomena, hypnosis has to be one of them, or that we have to assume or present it as real in order to define it. As the various commentaries here show, modern expert opinion remains very divided about the actual existence of an entity we could meaningfully label a hypnotic altered state of consciousness; hence, in my view, although not absolutely necessary, I see no reason why this should not to some extent be reflected in a definition of hypnosis. As I have already pointed out, there are a number of precedents for defining terms in this way.

Although having ostensibly rejected the need for what he calls a 'final definition' of hypnosis, O'Neil then proceeds to provide an account that assumes the reality of the concept of a hypnotic state and describes how, in his view, it is operationalised in clinical practice. O'Neil's examples raise some interesting issues about how the concept of a hypnotic state might be operationalised in clinical practice; however, I do not think they are particularly helpful with regard to whether or not we should accept the concept of a hypnotic state as an actual entity. For instance, in support of his view, O'Neil argues that that, "just as there are a variety of EEGs for a variety of seizures, there may well be a variety of indicators for various altered states of consciousness which would map out what a hypnotic state and a meditative state have in common, and how they differ" (p. 130). Clearly this statement speaks for itself in implicitly acknowledging that, as yet, despite extensive research, there are no definitive physiological markers, EEG or otherwise, that can be used as evidence for the existence of a hypnotic state (Lynn, Kirsch, Knox, & Lilienfeld, 2006; Mazzoni, Venneri, McGeown, & Kirsch, 2013; Oakley & Halligan, 2010).

O'Neil then argues that "In clinical practice, the reality of a hypnotic state is generally judged by the whole practical context of the encounter: the quality of the working alliance, what is required to induce a hypnotic state (rapidity, pacing, repetition), what can be accomplished in the hypnotic state, what is required to re-alert from the hypnotic state (again, rapidity, repetition, pacing), and the quality of recall of the hypnotic state upon re-alerting" (p. 130). However, whilst this

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may be a reflection of what some clinicians do, to the best of my knowledge, it has never been empirically established that any of these indicators, either individually or in combination, qualifies as evidence for the reality of a 'hypnotic altered state'. For example, as I note in my paper, a variety of researchers have found that reports of the experience of hypnosis from hypnotizable individuals tend to be indistinguishable from those given by participants who have undergone relaxation training, or instructions in the use of imagery (Barber, 1969; Barber, Spanos & Chaves, 1974; Kirsch, Mobayed, Council & Kenny, 1992; Lynn, Myer & Mackillop, 2000). Also, with regard to what can be accomplished during hypnosis, it is notable that expectancy and motivation tend to be more reliable predictors of how participants will respond to suggestions following hypnotic induction than other variables such as absorption, fantasy proneness or dissociation (Braffman & Kirsch, 1999; Dienes et al., 2009; Kirsch & Braffman, 1999; Spanos, Arango & de Groot, 1993). Moreover, a variety of research also indicates that recall following hypnosis is strongly influenced, if not entirely determined by, ordinary social psychological and cognitive variables (Coe, 1999; Silva & Kirsch, 1987; Spanos, 1986; Wagstaff, 2004). Hence, in standard sociocognitive terms, one could just as well argue that what clinicians are picking up when assessing the variables described by O'Neil is the degree to which clients are willing and able to think and imagine along with, and respond to the themes and suggestions given to them, including the suggestion for hypnosis and feelings of relaxation etc.; i.e. there is no need to postulate the existence and intervention of a 'hypnotic altered state' to explain what is happening.

A similar analysis could be applied to O'Neil's idea that there is a possible distinction to be made between what he terms 'easy shallow' hypnotizability and 'arduous deep' hypnotizability. Thus he says, "A patient may require a long and onerous induction to enter a hypnotic state, but then appear to be in very deep hypnotic trance, not only because of what is accomplished in that state, but also in the long an onerous re-alerting required to bring the patient back to the here and now" (p. 130). Again this statement begs the question of whether there is a meaningful entity we can call a 'hypnotic state'; however, if we substitute 'hypnotic state' with something more empirically verifiable, like 'reported depth', and/or 'increased response to suggestion', then this could be construed as paralleling Cardeña's finding that, for some participants, a long induction may be more effective in enhancing hypnotic responding than a very short one. And, as such, it could be explained in the same way; i.e. for some individuals, a lengthy induction might be more likely to a) fit in better with their expectations about what a 'hypnotic induction' should involve, and b) give greater opportunity to generate non-specific experiential changes that can be used to reinforce the attribution that 'hypnosis' is occurring. Moreover, having experienced a lengthy induction, it might not be surprising if participants were to anticipate a proportionately lengthy 're-alerting' time and to act according to their expectations. In other words, once again, one can come up with an interpretation that does not require the postulation of a hypnotic altered state.

O'Neil further proposes that a variety of other clinical diagnoses, such as Dissociative Identity Disorder, PTSD, Somatoform Disorder, and other Dissociative Disorders have what he calls significant 'autohypnotic components'. However, I would argue that the term 'autohypnotic' here is again question begging. For example, relationships between measures of dissociative experiences and standard measures of hypnotizability are known to be inconsistent and subject to contextual influences (see, for example, Bryant, Guthrie & Moulds, 2001; Dienes et al., 2009; Kirsch & Council, 1992; Nadon, Hoyt, Register & Kihlstrom, 1991; Spanos, Arango & DeGroot, 1993). Comparisons between such conditions and hypnosis are also subject to methodological problems. For example, in his seminal paper on Multiple Personality Disorder, Spanos (1994) presents a variety of evidence for the view that MPD is fundamentally socially constructed; that is, "it is context bounded, goal-directed, social behaviour geared to the expectations of

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significant others" (p. 143). In his view, the reason why patients diagnosed with MPD sometimes also score high on measures of hypnotizability is not because they have fallen into some kind of common 'altered state', but because, when MPD and hypnotizability are assessed, they are construed by patients as having similar role requirements to report and enact 'as if' experiences. Similar issues are raised in Lilienfeld et al.'s (1999) review of Dissociative Identity Disorder, and Bryant et al.'s (2001) discussion of the relationship between acute dissociative reactions to trauma and hypnotizability in clinical patients.

Importantly, O'Neil then goes on to say, "I am struck by the ubiquity of 'suggestion' throughout the paper, as if hypnosis can't be addressed except in the context of suggestion, and as if the discussion were limited to 'normal' subjects. I wondered if the author's concerns reflect the research world of hypnosis quite distinct from the clinical world of hypnosis" (pp. 130–131). With regard to the latter point, I would say that this is far from the case. It has been my personal experience that for most practitioners who use hypnosis as an adjunct to therapy, suggestions are an integral feature of hypnosis procedures (hence the emphasis on suggestion in the APA and BPS definitions). Moreover, the well known adage cited by O'Neil that "all hypnosis is self-hypnosis", of course, fits very well with the sociocognitive view that hypnotic participants do not spontaneously or passively fall into a 'hypnotic state', but rather they are active, cognizing agents, thinking and imagining along with the suggestions given to them (Barber, Spanos & Chaves, 1974; Spanos, 1986).

However, O'Neil proceeds in his attempt to distance hypnosis from suggestion by arguing that, "Once in the hypnotic state, suggestion is generally a decreasing part of the therapeutic work" (p. 131). For the reasons I have just stated, I would consider this to be a somewhat idiosyncratic view of the use of hypnosis and suggestion in clinical hypnosis practice (for examples of the extensive use of hypnotic suggestions in clinical work, see, for example, Lynn, Rhue & Kirsch, 2010). Nevertheless, even if O'Neil's observation here were accurate, there would still be no necessary contradiction between the fact that hypnotherapy may involve techniques or 'therapeutic work' other than suggestion, and the idea that hypnosis per se is a product of suggestion, or that the practice of hypnosis (hypnotism) typically involves the use of suggestion. This is because the general consensus amongst members of the scientific and medical community appears to be that hypnosis per se is not a therapy and is not equivalent to hypnotherapy; rather hypnotherapy involves the *addition* of hypnosis to the therapeutic techniques that practitioners usually employ within the scope of their professional work (see, for example, the BPS report on the Nature of Hypnosis, 2001; Lynn, Rhue & Kirsch, 2010). So, for example, according to the hypnosis as a suggestion view, if the patient has been given and has accepted the suggestion for hypnosis, then for so long as the suggestion is accepted by the patient, it still remains at what O'Neil terms the 'core of hypnosis' regardless of what else the therapist might end up doing in his or her hypnotherapeutic regime.

I would also take issue with O'Neil's claim that, because patients may respond selectively to suggestions (i.e. accepting some but resisting others), this negates a relationship between hypnosis and suggestion. He says, "If they [patients] were globally 'suggestible' in the conventional sense, then they could be easily suggested back to mental health" (p. 131). Perhaps so, but there has long been a consensus amongst hypnosis researchers that hypnotic suggestibility is not an indicator of some trait of 'global suggestibility'. Indeed, it is doubtful whether a trait of 'global suggestibility' even exists (Evans, 1967; Stukat, 1958). Hence, hypnotic suggestibility is not the same as, for example, gullibility or persuasibility (Hilgard, 1973; Moore, 1964). Rather, most high hypnotizables are best construed as active deliberating agents who remain in control of their actions and can selectively resist responding to suggestions as they see fit. Thus, as Lynn et al. (2010) remark, "subjects retain the ability to control their behaviour during hypnosis, to refuse to respond to suggestions, and even to oppose suggestions" (p. 7). Consequently,

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if, for reasons of pathology, otherwise suggestible hypnotic participants may decide to resist certain suggestions, this does not in any sense contradict an association between hypnosis and suggestibility, or the idea that hypnosis is a product of suggestion.

O'Neil finishes his commentary by asking whether "a subset of subjects have a nocebo effect to the suggestion—a specific inhibition to entering an altered state when told that this is the intent" (p. 131). It is actually quite well established that if participants are tested with and without hypnotic induction, a number show a decrease in suggestibility (see, for example, Braffman & Kirsch, 1999; Hilgard & Tart, 1966). One difficulty, however, has been determining whether such effects are simply a consequence of statistical and methodological artifacts such as random fluctuation and regression to the mean. However, beyond this, a variety of evidence indicates that such responses are most likely to reflect the systematic effects of negative attitudes and expectancies. For example, some investigators have reported a 'negative subject effect' (Jones & Spanos, 1982; Jones and Flynn, 1989); that is, some participants, particularly those who do not consider themselves to be hypnotizable, may actually reject or perform counter to the suggestions given to them in the context of hypnosis such that they perform worse than under non-hypnotic conditions. Jones and Spanos (1982) suggest that these results fit with others showing that participants who are relatively low on measures of hypnotizability are more likely to report that they were purposely uncooperative when responding to hypnotic test suggestions. Moreover, common to such negative responding is the subject's unwillingness to succumb to what he or she perceives to be manipulation by the experimenter. The fact that some individuals may react negatively to situations and procedures defined as hypnosis is also found in some therapeutic situations. In my experience, when this occurs, most therapists will try and educate patients more about the realities of hypnosis as it is now construed by most authorities (i.e. hypnotized individuals do not lose consciousness or become subject to the arbitrary will of the hypnotist), as well as offering alternative procedures, including those similar to or the same as those that one would have employed in a hypnotherapeutic context, but devoid of any mention of 'hypnosis', or implication that it is involved (see, for example, Wagstaff, Daniel, Lynn, & Kirsch, 2009; Wagstaff & Davies, 1991).

Conclusion

My general conclusion is that, in terms of my intention to come up with a definition of hypnosis that would be acceptable to all, regardless of theoretical persuasion, I have clearly failed. However, hopefully, I have encouraged more consideration of the matter. As I pointed out in my response to Laurence, theoretical debate is an essential and healthy feature of scientific enquiry; however, it is very difficult to proceed in a systematic and coherent manner if we cannot even agree on what it is we are actually supposed to be investigating. Issues of definition are not trivial; if we cannot agree on them we will not have a common language to establish the goals of our inquiry and guide our research.

From the responses to my paper, I would gauge that the main objections to my definition will come from those, such as Laurence and Terhune, who prefer some kind of simple operational, procedural approach. However, as I have repeatedly tried to emphasize here, I believe this kind of approach will fail unless there is some kind of further evaluation of what supposedly differentiates a 'hypnotic suggestion' from a 'non-hypnotic suggestion', and a 'hypnotic induction procedure' from any other kind of procedure. In other words, to define what an 'X' suggestion or induction procedure is, we need a definition of 'X'. Of course we could say that what makes a procedure, 'hypnotic', is the literal presence of the word 'hypnosis' in an induction protocol. Indeed, this view is mentioned in the 2003 APA definition and description. But if we adopt this position, we might want to question whether it really makes sense to exclude other procedures and situations in which the word 'hypnosis' is not explicitly used, but participants self-attribute the descriptor 'hypnosis' to the situation or procedure. Moreover, and perhaps most important, if adding the word 'hypnosis' is so

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critical in defining a context as 'hypnotic', it might be worth reflecting on what concept is implied or conveyed by the word or label 'hypnosis' that makes it so important and accounts for its effectiveness (see, for example, Kirsch, Montgomery & Sapirstein, 1995). I would suggest it is something that is the same or very similar to the idea that is the central feature of my definition.

References

Barber, T. X. (1969). *Hypnosis: A scientific approach*, New York, NY: Van Nostrand.

Barber, T. X., Spanos, N. P., & Chaves, J. F. (1974). *Hypnotism, imagination and human potentialities.* New York, NY: Pergamon.

Banyai, E. I., & Hilgard, E. R. (1976). A comparison of active alert hypnotic induction and traditional relaxation induction, *Journal of Abnormal Psychology, 85*, 218–224.

Bernheim, H. (1889). *Suggestive therapeutics: A treatise on the nature and uses of hypnotism (2nd ed.)* New York, NY: Putnam.

Bertrand, A. J. F. (1826). *Du magnéstisme animal en France*. Paris, France: J. B. Ballière.

Barrett, D. (1979). The hypnotic dream: Its relation to nocturnal dreams and waking fantasies. *Journal of Abnormal Psychology*, *88*, 584–591.

Bechterew, W. V. (1906). What is hypnosis? *The Journal of Abnormal Psychology, 1*, 18–25.

Bowers, K. S. (1983). *Hypnosis for the seriously curious*, New York, NY: Norton.

Braffman. W., & Kirsch, I (1999). Imaginative suggestibility and hypnotizability: An empirical analysis. *Journal of Personality and Social Psychology, 77*, 578–587.

Braid, J. (1843). *Neurypnology: Or the rationale of nervous sleep considered in relation with animal magnetism*. London, UK: Churchill.

British Psychological Society (2001). *The nature of hypnosis*. Leicester: the British Psychological Society.

Bryant, R. A., Moulds, M., & Guthrie, R. M. (2001). Hypnotizability in acute stress disorder. *American Journal of Psychiatry*, 158, 600–604.

Cardeña, E. (2014). Spinning in circles. *Journal of Mind–Body Regulation*, *2*(2), 121–123.

Charcot, J. M. (1889). Clinical lectures on diseases of the nervous system [Leçons sur les maladies du système nerveux] (Vol. 3). London, UK: The New Sydenham Society.

Coe, W. C. (1989). Posthypnotic amnesia: Theory and Research, (in) N. P. Spanos, & J. F. Chaves (Eds.), *Hypnosis: The cognitive behavioral perspective* (pp. 110–148). Buffalo, NY: Prometheus.

Connors, M. H. (2014). Defining Hypnosis: Altered States and the Need for Parsimony. *Journal of Mind–Body Regulation, 2*(2), 126–128.

Crawford, H. J., & Gruzelier, J. H. (1992). A midstream view of the neuropsychology of hypnosis: Recent research and future directions. In E. Fromm & M. R. Nash (Eds.), *Contemporary Hypnosis Research*. (pp. 227–266). New York, NY: Guilford Press.

Dienes, Z., Brown, E., Hutton, S.,Kirsch, I, Mazzoni, G., & Wright , D. (2009). Hypnotic suggestibility, cognitive inhibition, and dissociation. *Consciousness and Cognition, 18*, 837–847.

D'Hénin de Cuvillers, E. F. (1820). *Le magnétisme éclair ou Introduction aux archives du Magnetisme Animal.* Paris, France: Barrois, Treuttel et Wurtz, Belin-Le Prieur, Bataille et Bousquet.

Einstein. A. (1916). *Relativity: The special and general theory* (trans R. W. Lawson). London, UK: Methuen.

Evans, F. J. (1982). Hypnosis and sleep. *Research Communications in Psychology, Psychiatry & Behavior*, *7*, 241–256.

Egner , T., & & Raz, A. (2007). Cognitive control processes and hypnosis. In G.A. Jamieson (ed.), *Conscious states: The cognitive neuroscience perspective* (pp. 29–50). New York: Oxford University Press.

Ellenberger, H.F. (1970). *The discovery of the unconscious: The history and evolution of dynamic psychiatry*. New York: Basic Books.

Evans, F.J. (1967). Suggestibility in the normal waking state. *Psychological Bulletin*, 67, 114–129.

Faria, J. C. Abbé de. (1906/1819). *De la cause du sommeil lucide; ou étude sur la nature de l'homme* (2nd ed.). D. G. Dalgado (ed.), Paris, France: Henre Jouvet.

Figuier, L. (1860). *Histoire du merveilleux dans les temps modern* (Vol. 3). Paris, France: Hachette.

Gibbons, D. E., & Lynn, S. J. (2010). Hypnotic inductions: A primer. In J. Rhue, S. J. Lynn, & I. Kirsch (Eds.), *Handbook of Clinical Hypnosis* (pp. 267–291). Washington, DC: American Psychological Association.

Glass, L. B. & Barber, T. X. (1961). A note on hypnotic behaviour: The definition of the situation and the placebo effect. *Journal of Nervous and Mental Disease*, *132*, 539–541.

Green, J. P., Barabasz, A. F., Barrett, D., & Montgomery, G. H. (2005). Forging ahead: The 2003 APA Division 30 definition of hypnosis. *International Journal of Clinical and Experimental Hypnosis, 53*, 259–264.

Hilgard, E. R. (1973). The domain of hypnosis: With some comments on alternative paradigms. *American Psychologist*, 972–982.

Hilgard, E. R. (1986). *Divided consciousness: Multiple controls in human thought and action*. New York, NY: Wiley.

Hilgard, E. R., & Tart, C. T. (1966). Responsiveness to suggestions following waking and imagination instructions and following induction of hypnosis. *Journal of Abnormal Psychology, 71*, 196–208.

Hypnosis Unit UK (2012). *What is Hypnosis?* Retrieved from http://www.hypnosisunituk.com/what.html

Jones, W. J., & Flynn, D. M. (1989). Methodological and theoretical considerations in the study of 'hypnotic' effects in perception. In N. P Spanos, & J. F. Chaves (Eds.), *Hypnosis: The Cognitive Behavioral Perspective* (pp. 149–174). Buffalo, NY: Prometheus.

Jones, B., & Spanos, N. P. (1982). Suggestions for altered auditory sensitivity, the negative subject effect and hypnotic susceptibility: A signal detection analysis, *Journal of Personality and Social Psychology, 43*, 637–647.

Kirsch, I. (2014). Wagstaff's definition of hypnosis. *Journal of Mind–Body Regulation, 2*(2), 124–125.

Kirsch, I., & Braffman, W. (1999). Correlates of hypnotizability: The first empirical study. *Contemporary Hypnosis, 16*, 224–230.

mbr.journalhosting.ucalgary.ca | EISSN 1925-1688

Kirsch, I., & Council, J. R. (1992). Situational and personality correlates of suggestibility. In E. Fromm & M. Nash (Eds.), *Contemporary hypnosis research* (pp. 267–291). New York, NY: The Guilford Press.

Kirsch, I., Mobayed, C.P., Council, J.R., & Kenny, D.A. (1992). Expert judgments of hypnosis from subjective state reports. *Journal of Abnormal Psychology*, *101,* 657-662.

Kirsch, I., Montgomery, G., & Sapirstein, G. (1995). Hypnosis as an adjunct to cognitive-behavioural therapy: A meta-analysis. Journal of Consulting and Clinical Psychology, *63,* 214–220.

Laurence, J.-R. (2014). When in doubt, forbear! *Journal of Mind–Body Regulation, 2*(2), 109–111.

Liébeault, A.-A. (1866). *Du sommeil et des états analogues considérés surtout du point de vue de l'action du moral sur le physique*. Paris, France: Masson.

Lilienfeld, S. O., Lynn, S. J., Kirsch, I., Chaves, J. F., Sarbin, T. R., Ganaway, G. K., & Powell, R. A. (1999). Dissociative Identity Disorder and the Sociocognitive Model:Recalling the Lessons of the Past. *Psychological Bulletin, 125*, 507–523

Lyman, H. M. (1885). *Insomnia and other disorders of sleep*. Chicago, IL: W. T. Keener.

Lynn, S. J., Kirsch, I., Knox, J., & Lilienfeld, S. (2006). Hypnosis and neuroscience: Implications for the altered state debate. In G. Jamieson (Ed.), *Hypnosis and conscious states: The cognitive-neuroscience perspective*. New York, NY/Oxford, UK: Oxford University Press.

Lynn, S. J., Myer, E., & Mackillop, J. (2000). The systematic study of negative posthypnotic effects: Research hypnosis, clinical hypnosis and stage hypnosis. *Contemporary Hypnosis*, *17*, 127–131.

O'Neil, J. A. (2014). Hazards of 'final' definition. *Journal of Mind–Body Regulation, 2*(2), 129–131.

Rhue, J., Lynn, S. J. and Kirsch, I. (Eds.), *Handbook of Clinical Hypnosis*. Washington, DC: American Psychological Association.

Mackay, C. (1869). *Memoirs of extraordinary popular delusions and the madness of crowds*. London, UK: Rouledge.

Mazzoni, G.,Venneri, A., McGeown, W. J., & Kirsch, I. (2013). Neuroimaging resolution of the altered state hypothesis. *Cortex*, *49*, 400–410.

Milling, L. S., Kirsch, I., Allen, G. J., & Reutenauer, E. L. (2005). The effects of hypnotic and nonhypnotic imaginative suggestion on pain. *Annals of Behavioral Medicine*, *29*, 116–127.

Moore, R. K. (1964). Susceptibility to hypnosis and susceptibility to social influence. *Journal of Abnormal and Social Psychology, 68*, 282–294.

Nadon, R., Hoyt, I. P., Register, P. A., & Kihlstrom, J. F. (1991). Absorption and hypnotizability: Context effects re-examined. *Journal of Personality and Social Psychology, 60,* 144–153.

Oakley, D. A., & Halligan, P. W. (2010). Psychophysiological foundations of hypnosis and suggestion. In Rhue, J., Lynn, S. J. and Kirsch, I. (Eds.), *Handbook of Clinical Hypnosis* (pp. 79–118). Washington, DC: American Psychological Association.

O'Neil, J. A. (2014). Hazards of 'final' definition. *Journal of Mind–Body Regulation, 2*(2), 129–131.

Polito, V., Barnier, A. J., & McConkey, K. M. (2014). Defining hypnosis: Process, product, and the value of tolerating ambiguity. *Journal of Mind–Body Regulation, 2*(2), 118–120.

Sheehan, P. W., & Perry, C. W. (1976). *Methodologies of hypnosis: A critical appraisal of contemporary paradigms of hypnosis.* Hillsdale, NJ: Laurence Erlbaum.

Sheehan, P. W., & McConkey, K. M. (1982). *Hypnosis and experience: The exploration of phenomena and process*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Silva, C. E. & Kirsch, I. (1987). Breaching hypnotic amnesia by manipulating expectancy. *Journal of Abnormal Psychology, 96*, 325–329.

Skemp, R. R. (1972). Hypnosis and hypnotherapy considered as cybernetic processes. *British Journal of Clinical Hypnosis*, *3*, 97–107.

Spanos, N. P. (1982). A social psychological approach to hypnotic behavior. In G. Weary, & H. L. Mirels (Eds.), *Integrations of clinical and social psychology* (pp. 231–271). Oxford, UK: Oxford University Press.

Spanos, N. P. (1986). Hypnotic behavior: A social psychological interpretation of amnesia, analgesia, and "trance logic". *The Behavioral and Brain Sciences, 9*, 449–502.

Spanos, N. P. and Chaves, J. F. (Eds.). (1989). *Hypnosis: The cognitive behavioral perspective.* Buffalo, NY: Prometheus.

Spanos, N. P., Cobb, P. C., & Gorassini, D. R. (1985). Failing to resist test suggestions: A strategy for self presenting as deeply hypnotized. *Psychiatry*, *48,* 282–292.

Stukat, K. G. (1958). *Suggestibility: A factorial and experimental analysis*. Stockholm, Sweden: Almquist and Wiksell.

Tart, C. T. (1966). Types of hypnotic dreams and their relation to hypnotic depth. *Journal of Abnormal Psychology, 71,* 377–382.

Tart, C. T. (1970). Self-report scales of hypnotic depth. *International Journal of Clinical and Experimental Hypnosis*, *18*, 105–125.

Terhune, D. B. (2014). Defining hypnosis: The pitfalls of prioritizing spontaneous experience over response to suggestion. *Journal of Mind–Body Regulation, 2*(2), 115–117.

Thornton, E. M. (1976). *Hypnotism, hysteria and epilepsy: An historical synthesis*. London, UK: Heinemann.

Wagstaff, G. F. (1981). *Hypnosis, compliance, and belief.* Brighton, UK: Harvester / New York, NY: St Martin's Press.

Wagstaff, G. F. (1998). The semantics and physiology of hypnosis as an altered state. *Contemporary Hypnosis*, *15,* 149–164.

Wagstaff, G. F. (2004). High hypnotizability in a sociocognitive framework. In M. Heap, D. Oakley & R. Brown (Eds.), *The highly hypnotizable person* (pp. 85–114). London, UK: Brunner-Routledge.

Wagstaff, G. F., Cole, J., & Brunas-Wagstaff, J. (2008). Measuring hypnotizability: the case for self-report scales and normative data for the Long Stanford Scale. *International Journal of Clinical and Experimental Hypnosis, 7*, 27–40.

Wagstaff, G. F., Daniel, D., Lynn, S. J. & Kirsch, I. (2009). The cognitive behavioral model of hypnotherapy. In J. Rhue, S. J. Lynn, and I. Kirsch (Eds.), *Handbook of Clinical Hypnosis* (pp. 179–208). Washington , DC: American Psychological Association.

Wagstaff, G. F., & Davies, A. D. M. (1991). When hysterical ataxia is a role-enactment. *Contemporary Hypnosis*, *8*, 157–160.

Weitzenhoffer, A. M. (1980). Hypnotic susceptibility revisited. *American Journal of Clinical Hypnosis*, *22*, 130–146.

Woody, E., Sadler, P. (2014). A Somewhat Altered Debate about the Hypnotic State. *Journal of Mind–Body Regulation, 2*(2), 112–114.

Yeti (2013). *Cambridge English Dictionaries*. Retrieved from http://dictionary.cambridge.org/dictionary/british/yeti

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