

COMMENTARY

Reply to Three Commentaries on “Hypnosis Reconsidered, Resituated, and Redefined”

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In responding to the comments (pp. 327–359) on my article (pp. 295–325) in this issue of the *JSE*, I will take up the issues raised by each of the three Commentaries separately. I take this approach because their concerns seem to be quite different, and by treating them individually I can best do justice to each.

Tart’s Comments

Tart’s comments are directed mainly to the first half of my article, where I call into question the way *hypnosis* has been defined, particularly over the past seventy-five years, in terms of arbitrarily compiled lists of hypnotic phenomena. He makes the point that the lack of sensitivity to implicit and cultural assumptions found in the case of hypnosis illustrates a problem that too often goes unrecognized in other areas of scientific inquiry. Tart calls attention to the fact that scientists can be vulnerable to becoming attached to a self-concept of objectivity that excludes the possibility of being influenced in their observations or theorizing by unrecognized biases. He points out that the recognition of the possibility of unacknowledged influences operating in hypnotic experimentation, formulated in the 1960s in terms of experimental bias and demand characteristics, was something that he himself researched, and he notes how quickly that kind of important scientific self-examination disappeared.

It is precisely the need for this kind of self-examination with regard to hypnosis that led me to write the article. I was confident that a call for the examination of unrecognized assumptions would be welcomed by those who appreciate the importance of hypnosis and want to make hypnotic research and clinical practice as effective and fruitful as possible. It was with some surprise, then, when I read the comments of Cardeña and Terhune, who not only disagree with my conclusions (which is their prerogative) but also convey the impression that all is well and in order in the field of

hypnotic research and that any discussion that presumes to re-evaluate basic assumptions is not welcome. For that reason, I would like to respond at some length to their comments.

Comments of Cardeña and Terhune

To begin I would like to say something about the comments of Cardeña and Terhune on the historical segment of my article. Their description of the experiment carried out by the Franklin commission set up to investigate animal magnetism is accurately outlined. It was known from the very first years that effects of animal magnetism could, at least in some cases, be attributed to suggestion, and this possibility is mentioned in the earliest magnetic literature. The observation of Cardeña and Terhune about my take on Orne's experiments, however, puzzles me. It must be obvious that Orne's experiments to see which responses were due to hypnosis and which to prior knowledge surely are geared to attempt to ascertain whether hypnosis is genuinely present in any particular responses.

Commenting on trance, Cardeña and Terhune misunderstand my view of the field of hypnosis at present. I do not say, as they seem to imply, that there has been no progress in the field of hypnosis. On the contrary, I consider the work on hypnosis, both experimental and clinical, that has been carried out over the past 200 years to be most impressive and to have contributed important benefits to human science. However, I do claim, along with Weitzenhoffer, that the field is presently in a state of disorder. Unfortunately, it appears that Cardeña and Terhune are so intent upon defending the reputation of experimental hypnosis as conducted up to this point (although I do not see the need for such a defense) that they seemed to miss the substance of what I was saying.

The view of Cardeña and Terhune that my account of trance is "somewhat novel" is puzzling (especially considering that later they will claim there is nothing new in my proposal). Of course associating hypnosis with trance is not novel at all. Many have done it—most notably Milton Erickson. What is somewhat novel is the combination of elements I include in my definition of *trance*. Also, I define *hypnosis* in a very specific way as a subspecies of trance: an inner-mind trance that includes rapport. Proposing this definition of *hypnosis*, situated within my very specific meaning of *trance*, is, I believe, indeed novel.

It seems that Cardeña and Terhune take my call for a "fresh start" in defining *hypnosis* to mean I am talking about starting from scratch. It must be obvious from my article that I do not take that attitude. My ideas arise from and depend upon the whole rich and fruitful tradition represented in the hypnotic literature.

I find it interesting that Cardeña and Terhune object to the use of the word *trance* on the grounds that, in addition to the ancient meaning of the word that I use as my starting point, many other meanings have, over the ages, been ascribed to it. I chose the word to provide a starting point for clarifying the state of affairs in the hypnotic literature that has led to so many differing and contradictory definitions of *hypnosis*, which are difficult for the intelligent reader to make sense of. Having made that decision, I clearly defined the meaning of *trance* that would be most useful to the task at hand. There is nothing vague in my statement of its meaning in this context.

Cardeña and Terhune ask whether hypnosis, as I define it, only involves a narrow internal focus and specifically whether this means that focus is all there is to hypnosis. I attempted to clarify this issue in my treatment of the crucial role of “rapport” in my definition of *hypnosis*. I believe that rapport involves, in an essential way, the hypnotic subject’s incorporation of the hypnotist into the focus, so that the hypnotist becomes an internal presence. From this it naturally follows that suggestions given by the hypnotist are experienced as coming from within and are, therefore, very influential. I must disagree with Cardeña and Terhune that this approach excludes external stimuli from the hypnotic experience. The hypnotist may, and often does, draw external stimuli to the attention of the subject. In so doing, these too are incorporated into the subject’s internal focus. Later in their comments, Cardeña and Terhune say,

Participants experience the suggestions as coming from the hypnotist; this, in turn, produces the extra-volitional phenomenology of hypnotic responding—the experience that the responses are controlled by an external agent rather than by the person him/herself. (Cardeña & Terhune 2012:334)

I agree with this statement, for subjects do experience suggestions as extra-voluntary. However, this evaluation on the part of the subject is in terms of his or her conscious understanding. This does not rule out the possibility of a subconscious evaluation of a different kind, as Pierre Janet’s experiments with hysterics demonstrated (Janet 1889).

Cardeña and Terhune claim that I confound spontaneous and suggested responses, and that I do this throughout my article. This is a reading that I find difficult to respond to, since a substantial part of my explanation deals with the broader category of *trance* rather than the more restricted one of *hypnosis*. However, I would say that in my approach to *trance* states (one type of which I define as *hypnosis*), I indicate that both spontaneous and suggested responses occur. To say they both occur is not to say there is no difference between them, as Cardeña and Terhune seem to imply.

When considering what Cardeña and Terhune say in their comments

about suggestion, I am reminded of why I wrote this article. In these paragraphs they illustrate precisely what I think is confusing and misleading in discussions that take place around hypnosis. They ask the question, “Why are high hypnotizables also highly responsive to suggestions outside of a hypnotic context?” First of all, I find problems in the use of the term *high hypnotizables*. The notion of high hypnotizables is derived from and determined by a way of defining *hypnosis* that in my opinion simply does not work, for very good reasons. I find it surprising that Cardeña and Terhune say little about one of the main contentions in my article: that “hypnosis” simply cannot be defined in terms of lists of hypnotic phenomena. In contemporary literature, every attempt to experimentally determine the presence of the hypnotic state in the experimental subject is based on some version of a list of phenomena which the subject is supposed to manifest. And it is from this definition, and from the identification of the chosen phenomena in the behavior of the experimental subject, that the experiment is said to be about hypnosis. Without attempting to recapitulate my treatment of this important issue, let me here point out that when one considers the history of hypnotic literature, it is clear that these lists are made up of phenomena that are arbitrarily selected from among many that could have been chosen. As such, they do not and cannot constitute a dependable basis for identifying the presence of hypnosis. When “high hypnotizables” are identified, it is on the basis of these criteria. For that reason, when statements are made about the characteristics of “high hypnotizables,” I do not experience great confidence in the claims being made about them. To respond to their question thus formed, let me also ask: How does one determine what a “hypnotic context” is, except through the application of the same questionable phenomena-list criterion already mentioned? So I would respond that, given my objections, their question simply cannot be answered in its present form.

The query of Cardeña and Terhune about the role of individual differences in hypnotic experimentation is subject to the same criticism. I must ask how does one reliably determine responses based on individual differences resulting from “hypnotic induction” when there is no reliable way to know that a hypnotic induction has taken place? Again, I must express my dismay that Cardeña and Terhune have not in any way responded to this central point of the article: that the definition of *hypnosis* that is current in experimental hypnotic work is confusing and impossible to fruitfully employ in precise discussions of hypnotic experiences. Instead their criticisms are largely from a point of view apparently based on full acceptance of what I find unworkable. I would like to have heard some plausible reasons for denying my position. Instead, it seems to me, it is dealt with by ignoring it.

With regard to “variability across contexts,” I am surprised to see that Cardeña and Terhune seem to be unaware of the context in which I said “what is true of the clinical setting must be equally true of the experimental.” It is obvious that the “what is true” phrase referred to the fact that an expanded notion of demand characteristic must be applied in both areas, and that this was not a general insistence on the equivalency of clinical and experimental situations.

I am not surprised that Cardeña and Terhune may have misunderstood my statement about state-dependent memory, for I did not elaborate my views about post-hypnotic amnesia in this article. In establishing my position I was not relying on intuition, but the literature of state-dependent memory (e.g., Overton 1964, Fischer 1971, Eich, Macauley, & Ryan 1994, Woike, Bender, & Besner 2009). My particular view is that, in contrast to the almost ubiquitous presence of post-hypnotic amnesia in the first decades of magnetic somnambulism, the modern incidence is extremely low, and I fully concur with this finding of Hilgard and Cooper. However, I believe there are sociological reasons for this decline of post-hypnotic amnesia over the past two centuries. This does not affect the validity of the notion of state-related memory or militate against my belief that this phenomenon operates in many situations in ordinary life.

Cardeña and Terhune rightly point out that a great deal of research has been done concerning various important factors that affect the form of hypnotic phenomena, including conscious and unconscious personal and cultural expectations. My emphasis on the importance of these factors was not meant to imply that no one had taken notice of them over the long and winding road of hypnotic history. My concern was that although such elements have been here and there acknowledged, I do not believe that they have been adequately taken into account in the procedures of hypnotic research in general.

I must take exception to the claim of Cardeña and Terhune that I deny that “there are substantial behavioral, experimental, and physiological differences in the hypnotizability of individuals.” This is simply not true. That was not said in my article. Such a position is both contrary to what I believe and is in no way implied in my redefinition of *hypnosis*.

Although Cardeña and Terhune do not care for my use of the term *subliminal resources*, I must point out that this term is not new and the reality to which it refers, as expressed in the article, was often discussed by Frederic Myers, William James, Pierre Janet, Morton Prince, and many other experimenters in the era in which psychodynamic psychology was taking form. The fact that recent hypnotic research has not followed through with these important early insights is not a reason to deny their usefulness.

When Cardeña and Terhune take issue with my extending the notion of trance to everyday phenomena, they say that this dissolves hypnotic phenomena into irrelevance. They say that “if they are an aspect of every experience, there is no reason to even suppose that there is a distinct domain of hypnotic phenomena, and there is nothing special about them.” In expressing surprise that anyone might hold such a position, Cardeña and Terhune show that they have missed the whole tenor and central argument of the article. There I took pains to point out that the way the domain of hypnosis has been determined over the past decades has been through applying canonical lists of phenomena that supposedly set hypnosis off from other conditions. But this way of determining the domain of hypnosis simply cannot work. I will not repeat here the discussion in which I give reasons for this position. But in taking this position I did not intend to say that therefore hypnosis has nothing distinct about it. On the contrary, I gave *hypnosis* a definition that makes it fully distinct from every other human state or condition. It is my belief that this is the first definition that actually accomplishes this task.

As to the belief of Cardeña and Terhune that my explanatory approach is unfalsifiable because “one could always envision some type of ‘unconscious rationale’” that would account for the hypnotic response, this would be true only if there were no possibility of determining which specific psychological motivations may be operating subconsciously in individual subjects. I have to admit that I am more sanguine about making such a determination than Cardeña and Terhune may be.

Cardeña and Terhune contend that I make a “categorical mistake” in the way I use *subconscious* and *unconscious*. Much psychological and philosophical ink has been spilled over this complex problem, and I do not believe it is possible to do justice to that difficult discussion here.

It can come as no surprise that I disagree with the Conclusion of Cardeña and Terhune. To evaluate my position without even acknowledging, much less responding to, my concern that conventionally accepted definitions and domain identifications of hypnosis are fatally flawed, leaves me perplexed. It would have been of great relief to me had they provided me with good reasons for believing that my worries were misplaced, but I am afraid they have denied me that comfort. Rather, it is as if I had never spoken; so I can only conclude their counsel to be that if one pretends the disease is not there, perhaps one will be cured of it.

With regard to their referring to my definition of *hypnosis* as “vague” and “simplistic,” I can see no justification for such a view. My definition is just the opposite of vague; it is clear, unambiguous, and definite. On the other hand, I cannot imagine anything more vague by way of definition

than the one I call into question in my article: that hypnosis is a condition of some uncertain type that is defined by its manifesting an artificially limited number of arbitrarily chosen and canonized hypnotic phenomena which have been selected from a vast array of phenomena which show up not only in the hypnotic literature, but also in non-hypnotic states. This, it seems to me, makes a rather shaky foundation for experimental work, while at the same time providing little help to the clinical worker, especially those engaged in psychotherapy. As to the statement that my definition is simplistic, it is worth pointing out what that word means in this context. *Simplistic*, when applied to a scientific theory, means “unable to deal with the full richness of the data involved in the field in question.” That certainly does not apply to my theory. I might add, however, that it is indeed “simpler” than that one that has been in vogue. But simplicity should not be a mark against a theory. The criticism of a theory should, on the contrary, be based on its inadequacy to the data. I believe that simplicity is in fact an advantage, particularly if it helps to bring clarity to a field of endeavor that finds itself in some disarray. I might add that it is precisely such simplicity that, when developed with a view to experimental confirmation, should facilitate devising means for empirically deciding its worth.

Cardeña and Terhune worry that non-specialist readers may assume that my account is a fair description of the field as it currently stands. The great American philosopher and scientist Charles Sanders Peirce wrote,

We may as well acknowledge it, [scientific men] are, as such, mere specialists. . . . We are blind to our own blindness; but the world seems to declare us simply incapable of rising from narrowness and specialism to take broad view of any facts whatsoever. (Peirce 1935:376)

Cardeña and Terhune seem to take the position that only those who are themselves engaged in hypnotic experimentation (the specialists) are in a position to make a judgment about what is happening in their field, and that therefore those who are not (the non-specialists) must be content to let their opinion in the matter be formed by those who see themselves as spokespersons for their specialty. This is a kind of scientific puffery that is, unfortunately, more common among scientific writers than one might hope (Lewontin 1997). I believe it is important for specialists to have some confidence in the intelligence of non-specialists. My position, expressed in this article, is that although hypnosis research has provided valuable insights into the nature of human experience, it has at its core a problem which, while not totally invalidating what has been done so far, urgently needs to be corrected. I do not expect this to be too great a shock for intelligent non-

specialist readers, nor would I think it beyond their capacity to treat this discussion as a stimulus to expand their readings to include other relevant literature.

I must agree with Cardeña and Terhune that this proposal could have been written in the 1950s; unfortunately, it was not. The problem was already evident at that time, but it seems that my particular dissatisfaction and my specific solution were not yet in evidence.

I would like to make one final comment on the response of Cardeña and Terhune. It seems to me that knowledge of the history of hypnosis includes knowledge of the research that has occurred during its course. Although Cardeña and Terhune may not agree with my identification of problems relating to certain aspects of more recent hypnotic experimentation, sometimes the wider view of the historian and clinician can provide a perspective on the key issues that may be denied one with too great proximity.

Beere's Comments

In contrast to the approach of Cardeña and Terhune, Beere's comments respond directly to my concerns about the understanding of hypnosis currently in vogue and also to the substance of my proposed alternative approach. His comments and questions are stimulating and are the kind of thoughtful reflection that is needed to create a dialogue which can, in his words, "assist in furthering his [Crabtree's] theory, a task well worth the endeavor." It is with a sense of that dialogue that I respond to his comments in some detail.

Terms and Definitions

The most important terms to be defined are *trance* and *hypnosis*. My intention in the article was to define these terms as clearly as possible so that the ensuing discussion could be as free of ambiguity as can be reasonably expected.

My definition of *trance* is: a state of intense focus on something, accompanied by a diminished awareness of everything else, which evokes appropriate subliminal resources. I have been developing this definition for fifteen years and it has undergone several revisions. But from the first it was inspired by a definition of *trance* from *Webster's Collegiate Dictionary*: a profound state of absorption or abstraction. Since I considered *absorption* and abstraction to be complementary terms, my first definition was "a state of absorption and abstraction," in which my meaning of *absorption* was "focus" and *abstraction* "diminished awareness." I realize that there are

many meanings of *trance* that have developed over the past five centuries or so. My intention in working out this definition is to distinguish what I mean from all other meanings of the word. For that reason, when Beere asks how *trance* as defined in other places is included in my definition, I have to say that those other meanings are not what I intend, and I certainly do not mean to say that my definition includes them. So when I use the word *trance* in this article, I mean it strictly according to my specific definition. This will, I hope, prevent the reader from thinking that I am going to discuss any other usage.

My definition of *hypnosis* is: an inner-mind trance characterized by rapport. Whenever I talk about my usage of that word in my proposed framework it always has that meaning and that meaning only. Others use the word with many other meanings. My explanations concern only what falls within my specific definition. Trance is the broader category; hypnosis is only one type of trance. I am concerned that in several places Beere seems to believe that I am saying that all *trance states* are a type of *hypnosis*. That is certainly not my meaning. Trance includes all instances of hypnosis, but hypnosis does not include all instances of trance. I also want to emphasize the crucial importance of the use of the word *rapport* in my definition. Any inner-mind trance that does not involve rapport is not hypnosis, in my meaning of the term.

Clarifications

Beere raises an important issue when he points out the problem of establishing the domain of hypnosis. We must be critical in accepting which reported cases of hypnosis in the literature of the last two centuries were indeed hypnosis and in deciding whether the phenomena reported actually occurred, as opposed to being the result of bad observation, fraud, etc. In this I fully agree with Beere. Moreover, the difficulty in establishing the domain of hypnosis is precisely what led me to write my article. I did not state that all the reports of hypnosis were genuine, but, quite the contrary, that the list of the phenomena connected with hypnosis has evolved over time and that no particular list can be considered canonical in deciding what hypnosis actually is. So there is no way that I or anyone else can be assured that the phenomena reported over the past two hundred years are in fact hypnotic. The approach that I use in my paper does not depend on making such a judgment, but emphasizes that fact that many researchers *do* make such judgments, without realizing how arbitrary those judgments are.

Beere is correct in pointing out that I did not develop my article in the direction of explaining differences in hypnotizability, but I will say something about that here. Differences in hypnotizability in my theory are due to differences in the ability of subjects to achieve a state of inner focus

while in rapport with a hypnotist. Rapport is a key element of hypnosis, and there can be many reasons why that rapport may vary from one hypnotic situation to another. These reasons include, among others, the possibility that the subject cannot achieve rapport because a history of abuse, for example, makes openness to that kind of connecting (which involves a degree of trust) difficult, or the possibility that the hypnotizer, for various subjective reasons, is better at establishing rapport with some types of hypnotic subjects than others. I believe such factors may be so inhibiting that some individuals may not be hypnotizable at all. But this in no way means that those same individuals will not be subject to *trances* in my definition of the term, particularly those trances we experience in everyday life. It is important to distinguish between hypnotizability and the capacity to go into non-hypnotic kinds of trances.

I agree with Beere that during induction there is a transition from a non-hypnotic state to a hypnotic one. That transition occurs when the everyday trance with which the person begins (the non-hypnotic state) is disrupted and replaced by another focus that engages the person in his or her inner world (in this I agree with Charles Tart's description of the hypnotic induction process, Tart 2008). A further step then occurs in which that new focus is replaced by a more or less passive state that awaits automatisms from the subconscious mind, which provide a new, engaging focus. For a psychotherapist, that is the moment at which hypnotherapeutic work begins.

I must comment on Beere's concern when I refer to Erickson's use of the word *trance* as a synonym for *hypnosis*. I have learned a lot from Erickson, but my usage of the terms *trance* and *hypnosis* are not identical to his. He sees these terms as synonyms. I certainly do not. That is why I have taken pains to make their respective definitions as clear as possible. Beere seems to mistakenly think that I agree that they are synonyms and from this misunderstanding he wonders how *hypnosis* can be applied to the many possible meanings of *trance* that he cites from his online dictionary. The term *hypnosis* cannot be applied to any of many other kinds of trance that may exist. It can only be applied to one kind of trance—that which I have spelled out in my definition of *hypnosis*. For that reason, he is also mistaken when he says that I see “‘trance’ as a core definition of ‘hypnosis’.” He continues, “In this regard, I found myself asking, ‘How is trance different from hypnosis?’” These statements show that he continues to confuse the two and is not mindful of the clear definitions I have given them. I see trance as the broad category which has many subspecies, only one of which is hypnosis. So Beere is correct in saying that trance includes hypnosis and also many other kinds of mental states. But hypnosis is not an example of those other kinds of states.

In this context, I must disagree with Beere that what is called *zoning out* does not involve focus. The unresponsiveness to the environment that characterizes that state is due to some real, although perhaps not verbally expressible, inner focus. I believe that the same would apply to those states Beere refers to with the terms *dazed* and *half-conscious*. I would add that I do not apply the word *trance* to states of total unconsciousness, as Beere seems to think I do.

I believe it is because of these misunderstandings of my use of the terms *trance* and *hypnosis*, that Beere has arrived at some mistaken conclusions about the implications of my proposal. He asks whether my definitions of *trance* and *hypnosis* add clarity to the discussion. If his understanding of my proposal were correct, they probably would not. But it is not correct, and given my actual usage of those terms, I would have to say they do add clarity. In my proposal it should now be clear precisely what hypnosis consists of and how to recognize it not only through its observable phenomena, but also, and most importantly, through its subjective experience.

With regard to my phrase “the evocation of appropriate subliminal resources,” Beere wonders whether this is the equivalent of the commonly used “activating unconscious responses via hypnotic procedures.” In my proposal, these are not equatable because my phrase refers to the definition of “trance” in general, whereas the latter phrase refers only to hypnotic trance.

Beere presents a very thoughtful examination of my notion that trances are everyday phenomena. He wonders whether my definition of trance accurately describes day-to-day experience. His first question has to do with my use of the word *intense*. He asks whether, as he goes about his daily activities, his focus on various things could be described as intense. Beere’s question gives me the opportunity to clarify this matter here. I conceive of “intense” as admitting of degrees, of greater and lesser intensity. I wonder, however, about Beere’s description of the experience in which his thoughts are characterized by a “generalized wondering.” He describes shifting from inner thoughts occurring as he stares at the wall and furniture, ending with a plate resting on a stand, saying that the room was present all along and only now does he really notice it. He says that, according to me, he was not in a trance until he actually focussed on the plate. I would, in fact, not say that. I would say he was already in a trance, but the focus of that trance was his inner thoughts. His trance shifted when he began to notice the plate, and left his inner mulling aside. This is the kind of shifting from trance to trance that I describe at some length in the article. When Beere talks about being aware of something in a “diffused” way, he seems to be using that word in two senses. In one sense he seems to mean that what he is diffusedly aware

of is what I could call something on the “fringe” of attention, in precisely the way *fringe* and *focus* were used by William James (James 1890). But “diffused” attention for Beere also seems to be used to denote an awareness in which the focus is of very low intensity. In my way of looking at things, a “diffused” or “floating” awareness, when used in this latter sense, does not refer to having *no* focus, but a low degree of focus which moves rapidly from object to object until something really grabs the attention, at which point the focus is more intense.

Just what might this focus/fringe experience look like? Let us say I am staying at an old inn on a lake. In the morning I look at myself in the antique bathroom mirror. I wonder if my beard needs a trim. I examine it closely and decide it does. I turn and reach for my trimmer, and when I look back at my reflection in the mirror, I notice that the mirror has a number of small spots where the silvering has disappeared—a clear sign of age. I had not noticed the spots before; all I saw was my face and beard. If I had been called out of the room as I reached for my trimmer and asked whether the mirror was suitable and clear, I would have responded, yes. But now, for the first time I notice the spots and turn my attention to them. I look closely at them and note their positions, their shapes, their color. I see that they form a peculiar pattern—an arrangement of distinct triangles—that interests me. As I focus on the spots and the patterns that they form for me, I become more and more absorbed in them. For a moment I become so focussed on the mirror and its defects, I have almost no awareness of my beard and my face reflected in the mirror. I continue to concentrate on those odd spots and speculate how old the mirror is, whether this kind of defect occurs in all older mirrors, and what are the chances that this fascinating pattern of spots would eventually repeat itself in a mirror of the same manufacture. In the meantime, not only have I lost awareness of my beard and face, I do not notice the passage of time, and even momentarily forget who I am—I am totally focused elsewhere. Now I began to realize that I have totally lost awareness of my face, even though I am staring at its reflection. I have also lost awareness of the trimming project I had set for myself. My initial scrutiny of myself in the mirror has shifted to a fascinated examination of the spots on the mirror I am looking into. My focus has shifted, and my face and beard now form the fringe of my awareness. Now I am intrigued by what has just happened and reflect on the general fact that what I concentrate on at any moment is my focus, and what I am not concentrating on becomes the fringe of my awareness. These reflections become a new center of focus and now both beard and mirror are pushed to the fringe of my awareness. I can shift at will the focus/fringe structure of my experience: Now beard as focus with the mirror (as such) as fringe; then it is the mirror with its spots as focus with

my beard as fringe; and finally, my thoughts about *focus* and *fringe* take over as the center of attention. With each shift of focus the previous focus is forced to the periphery of my awareness and my awareness of it diminishes. Each new center of attention creates a new set of fringe elements. Expressed in terms of my specific definition of *trance*, I would say that each new focus creates a new set of things of which I have a diminished awareness.

Beere thinks that I “vacillate” about whether everyday experience is mostly trance and only partly so. This impression apparently arises from my statement: “Trances are part of everyday life.” My meaning was that one aspect of all everyday experience is trance. I thought my meaning would be clear, but I am glad to have the chance to remove any ambiguity.

I am puzzled by Beere’s reading of my text when he says, “Let us grant, for the sake of argument, that hypnosis is trance and that hypnosis is an inner-mind trance that can include all possible everyday experience.” True, hypnosis is a sub-species of trance, but it in no way follows from this that hypnosis can include all possible everyday experience. Hypnosis is only one, very specific kind of trance, one rarely experienced in ordinary everyday living. The conclusions Beere draws from this misreading of what I wrote cannot stand.

Beere then suggests that hypnosis can be trance without the requirement or implication that everyday experience also is trance. That might be hypothetically true, but it is not consistent with my proposal. Given my definition of *trance*, everyday experience must also be instances of trance.

When I point out that all hypnotic phenomena occur in some form or other in ordinary life, I am simply echoing the problem, recognized for many years now, that there is no phenomenon that is attributable to hypnosis and hypnosis alone. I did not mean to make a judgment about how *frequently* they occur in everyday life, as Beere seems to think, only that they do occur in everyday life. Beere then says, “This suggests to me that there must be something in the everyday circumstances that evoke ‘the hypnotic response’.” I am afraid that once again Beere attributes to me assertions that I have never made and with which, in fact, I disagree. I do not believe that everyday experiences evoke the “hypnotic response.” Again Beere seems to think I am saying that all everyday experiences are hypnotic. This is contrary to what I hold and contradicts what I have written. My position is that everyday circumstances evoke “trance” responses, very few of which will be “hypnosis” (unless, for example, the individual involved is in hypnotherapy). For that reason, Beere’s subsequent “reconstruction of the underlying logic” is incorrect.

Beere says that in reading my article he seems to “find himself caught in definitions that point back and forth to themselves such that they explain

the phenomena by fiat and are exempt from further consideration.” I can only conclude that this feeling arises from his misreadings of what I have written, which I have pointed out above. There is no circular logic in my actual exposition.

Beere’s discussion of “absorption” as I have used it with regard to trance is very helpful. In my earlier writings, I used “absorption” as an element of my definition of trance. I no longer do. In psychological writings the term *absorption* has become a technical term. I think it is problematic to use that term in its technical sense as a defining element. For that reason, I think that Beere’s criticism of my use of the term in this context—even though it is not part of my definition—is justified, and for that reason I will no longer employ it in my discussions of the nature of trance.

I also find Beere’s discussion of my use of the term *inner mind* useful. Following his suggestion, I would like to clarify my meaning of *inner mind* and *outer mind*. I use *inner mind* as a phenomenological term. The inner mind operates in the world of interior impressions. Its meaningful reality consists of the thoughts, imaginations, fantasies, memories, feelings, and emotions that we experience as occurring in the mental world we describe as private. We experience them both when awake and when dreaming. When we focus on any of these things, we establish an inner-mind trance. There are many other kinds of inner-mind trance besides hypnosis, such as meditation, daydreaming, and worrying. We experience *conscious* awareness of our inner world, but we also discover that there are mental dynamics operating outside our normal awareness. This subconscious aspect of life operates dynamically to reveal itself in conscious awareness in various ways, and the boundary between the contents of subconscious and conscious awareness continually shifts. Insights about these interactions make up the foundation of what Ellenberger called dynamic psychiatry (Ellenberger 1970), and the history of psychodynamic psychotherapy reveals how these insights evolved (Crabtree 1993, 2003).

The outer mind, phenomenologically speaking, is the aspect of our mentality that experiences the world as publicly available and is largely active in the practical aspects of daily living. It is “in its element” in the physical, social, interactive environment of our lives. The public world is its home, its theatre of operations, the place where it is active. The job of the outer mind is to find the best way to deal with worldly affairs. The outer mind’s meaningful reality is not just the physical world and its occupants, but also the expectations, rules, and protocols that operate there.

Beere reaches a false conclusion when he presumes my views are based on a mind–body dichotomy. My phenomenological approach to “inner mind” and “outer mind” experiences makes no such metaphysical presumptions.

I have no problem with Beere's belief that hypnotic induction can begin with an outside focus. However, at some point that focus must shift to the inner world, otherwise hypnosis will not take place.

Beere's discussion of suggestion is also helpful. I agree that suggestions result in phenomena that are experienced as occurring without conscious intention on the part of the subject. Suggestions are effective in hypnosis. But as a matter of fact, as Hippolyte Bernheim said, they also occur in waking life and phenomena such as paralysis and anesthesia, and hallucinations can be obtained through suggestion without hypnotism (Bernheim 1884). There is controversy about whether a person is more suggestible in hypnosis than in waking life, but in this I concur with Beere that suggestion is more effective in hypnosis.

Beere makes a good point in saying that, in the example of the railway worker, his experience of having no pain when his toes were amputated would, on subsequent reflection, be considered alien or strange. However, his comment that "these observations make clear that hypnotic phenomena are not everyday experiences at all," is another example of his misreading of my statements about the matter. I have never made the point that *hypnotic* experiences are everyday experiences, only that *trance* experiences are everyday experiences.

To a certain extent I agree with Beere that the flow of experience presumes some kind of "meta-state" or context that is more inclusive. I see that meta-state as required to provide our experiences with a unity. The work of Janet with hypnosis in the late 1800s led to the positing of some rock-bottom, fully inclusive awareness that brings all the piecemeal experiences of various hypnotic states together, and he believed that it would theoretically be possible to reach a perfect subterranean stream of consciousness which would embrace the whole conscious life of the individual (Janet 1889:335).

In my article, I discuss the fact that in trances of ordinary life we direct our attention to things that become the object of focus—that conscious intention is involved. Beere seems to think I said that is what happens in hypnotic induction. That is not what I wrote. The place of automatisms in hypnosis (and in everyday trances) is central. I have discussed elsewhere the matter of automatic responses in hypnosis and other altered states (Crabtree 2007).

Beere describes his experience of going into a "hypnotic state" without any rapport being involved. I have no doubt he went into a self-induced inner-mind trance state. But it is not what I would call *hypnosis*.

My notion of the importance of rapport in hypnosis was not conceived in an attempt to explain why suggestion works, as Beere surmises. Rather

it derived from my study of the history of animal magnetism/hypnosis. Rapport was first mentioned and researched by Puységur in 1784, and it has remained a central feature of the history of hypnotism ever since. The fact that rapport can help make sense of the effectiveness of hypnotic suggestion is something I became aware of only in recent years.

Beere is right in describing Erickson's view of suggestion in terms of receptivity. In fact, when Erickson observed that a person was a good listener and attentive to others, he knew that person would be a good hypnotic subject. When a person is in a hypnotic state, he or she will experience automatisms, some of which may come as the result of suggestion. But I do not believe, as Beere thinks I might, that for suggestion to work rapport is necessary. I do recognize the power of suggestions elicited in the non-hypnotic or "waking" state.

To answer Beere's question, when I talk about the hypnotic subject incorporating the hypnotizer into his inner focus, I do not mean to say that this occurs bodily. Neither do I believe that the hypnotizer is experienced as an extension of self. Rather there is a sense of intimate presence in the same inner space. Beere seems to think that I have said that, because of the special sense of connection, suggestions should infallibly work. My only contention is that suggestions are *more likely* to be successful.

Commenting on my notion of group-mind trance, Beere again shows that he confounds trance and hypnosis. I do not talk about group-mind hypnosis; that notion does not make any sense to me. Rather I talk about group-mind trance.

When Beere describes his experience of sensing what things would be like for a child between three and four, he says that he diffusedly focussed on his body, discovered various sensations and other processes arising, he became aware of pictures that seemed to fit that age span, and experienced emotions and sensations that fit that age. He says that this experience showed that one did not have to be in a trance state to carry out this exercise. My conclusion is the opposite. What he describes is focus, followed by the coming forward of various sensory and motor automatisms in response to that focus. This is precisely what I mean by the evocation of appropriate subliminal responses in the trance state. Similarly, EMDR seems to me to involve a trance state. Clients focus briefly on the issue they are concerned with and then let it go and allow to happen whatever happens. To my way of looking at things, this is an example of what I define as a trance state, complete with evoked subliminal resources.

Beere's discussion of figure-ground distinctions in perception is intriguing. He finds this way of describing perception helpful and says that he notes similarities between this approach and William James' focus-

fringe idea. Beere takes this understanding of perception as the basis for his theory of dissociation. In his theory, dissociation “arises when someone attends with such intensity that background features are blocked out and thus experienced dissociatively.” I find his ideas about dissociation intriguing and consonant with my own therapeutic work with dissociative disorders. He states that the experiences of dissociation are also those that occur in hypnosis. He also says that Dell states that dissociation is involved in every kind of human experience, and notes that this is “an observation Crabtree makes about hypnosis.” Unfortunately, once again Beere attributes to me a position that I have never taken. If he is to accurately express my position, it should be phrased, “an observation Crabtree makes about *trance*.”

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I would like to conclude my responses with a comment on Beere’s statement, “Unfortunately, as I considered various elements of his theory, almost every one had a practical, theoretical, or logical flaw.” I believe that most of what Beere considers flaws in the theory are in fact due to his misreading of the text. I have pointed out many instances where this misreading occurred. In fact, in reading Beere’s comments I have formed the impression that we actually agree on many things with regard to the phenomenology of hypnosis, but that certain key misreadings of my text have obscured that basic agreement. It is my hope that we may some day have the chance to discuss these things more thoroughly. For now I will be content if our exchange helps to clarify my views about hypnosis for the reader.

References

- Bernheim, H. (1884). *De la Suggestion dans l'État Hypnotique et dans l'État Veille*. Paris: Octave Doin.
- Cardena, E., & Terhune, D. B. (2012). A proposal that does not advance our understanding of hypnosis. *Journal of Scientific Exploration*, 26(2), 329–339.
- Crabtree, A. (1993). *From Mesmer to Freud: Magnetic Sleep and the Roots of Psychological Healing*. New Haven: Yale University Press.
- Crabtree, A. (2003). “Automatism” and the emergence of dynamic psychiatry. *Journal of the History of the Behavioral Sciences*, 39, 51–70.
- Crabtree, A. (2007). Automatism and Secondary Centers of Consciousness. In E. Kelly, E. Kelly, A. Crabtree, A. Gauld, M. Grosso, & B. Greyson (Eds.), *Irreducible Mind: Toward a Psychology for the 21st Century*, Lanham, MD: Rowman & Littlefield.
- Ellenberger, H. F. (1970). *The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry*. New York: Basic Books.
- Eich, E., Macaulay, D., & Ryan, L. (1994). Mood dependent memory for events of the personal past. *Journal of Experimental Psychology*, 123, 201–215.
- Fischer, R. (1971). The “flashback”: Arousal-statebound recall of experience. *Journal of Psychedelic Drugs*, 3, 31–39.
- James, W. (1890). *Principles of Psychology*, two volumes. New York: Henry Holt.

- Janet, P. (1889). *L'Automatisme Psychologique. Essai de Psychologie Expérimentale sur les Forms Inférieures Humaine*. Paris: Félix Alcan.
- Lewontin, R. (1997). Billions and Billions of Demons. *The New York Review of Books*, Jan. 9, 1997.
- Overton, D. A. (1964). State-dependent learning or "dissociated" learning produced with pentobarbital. *Journal of Comparative and Physiological Psychology*, 57, 3–12.
- Peirce, C. S. (1935). *Collected Papers of Charles Sanders Peirce*, Volume 6, edited by C. Hartshorne, P. Weiss, & A. W. Burks. Cambridge, MA: Harvard University Press.
- Tart, C. (2008). Accessing state-specific transpersonal knowledge: Inducing altered states. *Journal of Transpersonal Psychology*, 40, 137–154.
- Woike, B., Bender, M., & Besner, N. (2009). Implicit motivational states influence memory: Evidence for state-dependent learning in personality. *Journal of Research in Personality*, 43, 39–48.