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AUTHOR'S REPLY

THE HYPNOTIC STATE: SEMANTICS AND PRAGMATICS

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As I am in broad agreement with the comments made by Kirsch and Lynn and Fite, I will concentrate the bulk of this response on the commentaries of Spiegel and Woody and Sadler.

Hypnosis, trait and mental state

In his reply, Spiegel makes a number of statements that illustrate well many of the problems that I address in my paper.

For instance, as supposed evidence of physiological markers of a hypnotic state, Spiegel points to evidence that high hypnotizables show greater left frontal theta activity when in conditions defined as hypnosis (Sabourin et al., 1990). However, whilst this finding is very interesting, of itself it tells us nothing about an alleged 'altered state of hypnosis'; it simply invites the same questions I address in my paper, such as, what would happen if we kept the context as hypnosis, but used different task instructions; would this response disappear? What other sorts of non-hypnotic tasks give rise to similar rises in left frontal theta? Would 'high hypnotizables' (or low hypnotizables for that matter) manifest similar responses to similar task instructions in entirely different contexts not associated with hypnosis? The problem is familiar; without the prerequisite controls, the fact that high hypnotizables show certain physiological effects under certain defined task conditions tells us nothing at all about whether they need to be in some special 'state', or utilizing some special hypnotic process, in order to do this.*

Next, Spiegel objects that my definition fails to consider that hypnosis is a trait. Whether or not people respond to various measures of hypnotic susceptibility in a consistent way is the subject of an important and longstanding debate, but as I have stated on numerous occasions (see, for example, Wagstaff, 1991) the state/non-state debate and the trait/situation debate are conceptually different. As Sheehan and Perry (1976) note, 'Both Hilgard and Sarbin, for example, stress hypnotic ability and so embrace the notion of trait specifically, but only the former of the two theorists

* *With regard to evidence for my definition, Spiegel points out that the paper by Kirsch et al. (1985) has been criticized by Allison and Faith (1996). However, rather than debate the specific arguments here, suffice it to say that my case does not require a demonstration that it is only the label of hypnosis that accounts for the positive effects of hypnotic procedures; hypnotherapeutic and other therapeutic interventions might vary in many ways that might be efficacious besides the label that is applied to them (such as degree of rapport and therapist enthusiasm involved etc.). The fact, however, that the label of hypnosis alone can influence suggestibility, as Barber showed many years ago, is strongly supportive of my case (Barber, 1969).*

chooses to view hypnosis as an altered state' (p.52). Indeed, in principle, it is perfectly consistent to hold the view that hypnosis is associated with a situationally determined 'altered state' (like being drugged, or suffering from an acute illness), and that sociocognitive variables, such as dramaturgic and imaginative skills, may lead people to behave in a consistent way in situations defined as hypnosis (see Sarbin and Coe, 1972; even obedience to authority may have 'trait' properties; see, Blass, 1991). The fact that hypnotic subjects will often claim to experience the same thing, or behave in the same way, in the same or similar situations, is not very surprising. Moreover, the finding that people respond consistently to the suggestions in standardized scales, such as the Stanford scales, tells us only that they have responded consistently to the suggestions in these scales, not that they have entered a special mental state, or need to, in order to respond to the suggestions.

Spiegel goes on to argue that, in his experience, lows to do not fail to respond because of the social and cognitive factors I identify, and that I 'beg the question' on this issue. I beg to disagree; as I note in my paper, there is a variety of evidence supporting the view that lows fail to respond to hypnotic suggestions, or fail to interpret their experiences as hypnosis, for a number of reasons compatible with a sociocognitive viewpoint, including inappropriate attitudes, expectancies, motivation and role-enactment skills (see for example, Kidder, 1972; Sarbin and Coe, 1972; Jones and Spanos, 1982; Spanos, 1986, 1991; Jones and Flynn, 1989; Kirsch, 1991; Lynn and Rhue, 1991a; Jan and Wagstaff, 1994; Gorassini, 1997).

When it actually comes to identifying what is meant by an 'altered state of hypnosis', it seems that what Spiegel has in mind is the idea that hypnosis is rather like intense absorption in novels etc. As I mention in my paper, this familiar notion corresponds to one of the components in Theodore Barber's original 'non-state' formulation of hypnosis (see Barber et al., 1974). It also corresponds to what Kirsch terms the 'weaker version' of the altered state hypothesis. However, as Kirsch also points out, if this 'intense absorption' is supposed to be a fundamental defining quality of hypnosis, then the term 'hypnosis' is effectively rendered redundant. Research into hypnosis simply becomes research into attention, or imaginative involvement. Given this, it is perhaps no wonder some people feel surprised they have not 'entered hypnosis', if, expecting something more profound, all they experience is something they might experience whilst reading a book or driving.

Spiegel then argues that I misconstrue the design of his studies using event potentials. I do not think so, though I do offer an alternative interpretation of their implications. I did not mention the other study reported by Spiegel and Barabasz (1988) showing increases in P300 following a suggestion for anosmia, because these findings contradict other findings for anosmia and different negative hallucination suggestions, and it is not at all clear what the results mean. If instructions for negative hallucinations produce a surprise reaction for anosmia, subjects should presumably be equally surprised when they negatively hallucinate other stimuli; but this is not the case (for a discussion, see Perlini et al., 1996).

Finally, Spiegel's argues that, in the case study by Davies and Wagstaff, the fact that our client 'did well without any formal use of hypnosis does not prove whether or not hypnotic phenomena were involved'. I find it difficult to respond to this statement, and others made by Spiegel (such as 'lows' can be 'hypnotically susceptible', and that 'waking hypnosis' differs from 'true hypnosis'), because it is not clear in what sense he differentiates between 'hypnotic' and 'non-hypnotic' phenomena. His statement on our case study presumably assumes that hypnosis is some kind of altered state that requires no formal induction, and can occur even when the client does not accept its occurrence; in other words, our patient was 'hypnotized', but neither she nor we realized that she was. But this was the starting point for my paper, so I will not bore readers by reiterating the problems with this idea, and my proposed solution.

In sum, it is my opinion that the psychophysiological experiments conducted by Spiegel and his associates are of considerable psychological interest; problems of interpretation in such studies will continue, however, so long as researchers fail to define exactly what they mean by hypnotic as opposed to non-hypnotic responding, and give inadequate consideration to the fact that sociocognitive explanations also predict changes in brain activity.

Semantics and logic reconsidered

In their commentary, Woody and Sadler take a rather different tack, based more on criticisms of the semantics and logic of my arguments.

First, Woody and Sadler object to my use of the notion of the hypnotic state as a category error. However, they misconstrue the idea of a category error. Of course it is not illogical to examine a random pile of building materials and ask 'where is the house'? This is a perfectly reasonable question; until the materials are assembled by someone, there is no house. What is illogical, however, is to examine all of the components of an assembled house separately, and then ask 'where is the house'? (for further discussion of category errors, see Ryle, 1973). According to my view, therefore, when one person says she felt 'hypnotized', or another says he is 'hallucinating' following an induction procedure, the explanations lie in various combinations of the factors that sociocognitive theorists have put forward; we do not need to look for 'something else', another unique factor in addition to these. In this context, the idea of 'emergent properties' is not problematic. When Gestalt theorists argue that the 'whole is more than the sum of the parts', they do not mean that to explain the whole we need to look for some extra 'parts'. Indeed, if Woody and Sadler want to argue that all of the phenomena we associate with the term hypnosis can be understood in terms of the emergent properties of various combinations of the factors identified by sociocognitive theorists, then we are closer to agreement than they might think.

Second, Woody and Sadler argue that to define hypnosis as an altered state we do not need to come up with a definitive set of psychophysiological markers, nor do we have to assume that it is easy to measure. The arguments they attack here are straw men; nowhere do I stipulate such requirements. Rather my point is that some researchers claim to have found physiological correlates of a hypnotic state, and have argued that their results are not explicable in terms of the variables suggested by sociocognitive theorists. However, it is my opinion that, for the moment at least, these relationships are explicable in terms of, indeed, are predicted by, these sociocognitive factors.

Third, Woody and Sadler contend that most of the explanatory variables put forward by sociocognitive theorists could be construed as involving 'states'. But why this is supposed to be a criticism of my arguments I fail to see, as I make, indeed labour, precisely the same point in my paper! I even argue that compliance could be and has been construed as a 'state'. Of course it is logically consistent to say that the belief one is, or has been hypnotized (or baptized or married for that matter) could be construed as 'an altered state'. Indeed, as I point out, such a conceptualization avoids the category error, and enables a possible reconciliation between the state and sociocognitive perspectives. The problem remains, however, that this notion of 'an altered state' is certainly not what most state theorists have in mind when they search for the physiological correlates of 'the hypnotic trance'. For a start, it negates the view that a person can enter a hypnotic state yet not believe they have; neither does it necessitate any reference to notions such 'dissociated experience or control'. Moreover, as survey research shows, it is not what the general public have in mind when they view hypnosis as a 'trance' state. (For further discussion on this issue see Wagstaff, 1981.)

Fourth, Woody and Sadler seem to object to the idea of attempting to explain hypnosis in terms of a wide range of variables. I fail to see the problem. To explain all of the automobile accidents in the USA in a single year would require a very large range of explanatory variables (alcohol consumption, poor servicing of cars, manufacturing defects, speeding, road rage, heart failure, weather conditions, road markings, poor lighting etc.); but, as long as they are sensible, consistent and have predictive value, it does not follow that such variables are somehow less viable or meaningful as explanations than a single ill-defined explanation, such as 'driver dissociation'. Moreover, to argue that the phenomena we associate with hypnosis require a wide range of sociocognitive explanations is not to imply that the range of such explanations must include 'almost anything' (I have yet to see a sociocognitive theorist attempt to link hypnotic responding to hair colour, a unique brain state, or animal magnetism). If readers want to examine the kind and range of explanations put forward by sociocognitive theorists, the literature is there to consult (see, for example, Spanos and Chaves, 1989; Lynn and Rhue, 1991b).

Continuing the 'almost anything' critical theme, Woody and Sadler cite Barber's definition of 'thinking and imagining along with suggestions' (Barber et al., 1974) and then criticize it as inadequate. But again, I do not see why this is supposed to be a criticism of my paper. I too think Barber's definition is inadequate, which is why I do not adopt it. As I make clear in my comments on Nash's definition, the main problem with this type of definition (as well as those based on notions such as dissociation) is that it fails to clearly differentiate the hypnotic from the non-hypnotic. My sole reason for citing it was to show how similar it is to others offered by 'state' theorists.

When they reach my definition, Woody and Sadler, once again, argue that the definition could refer to 'almost anything'. In an attempt to illustrate this, they substitute the word 'hypnosis' with the word 'learning', and argue that the definition still makes sense. Thus they consider it meaningful to say that 'learning' is 'any suggestion that we are, will be, or have been, in a special condition we call learning'. But surely this statement does not make sense. Unlike hypnosis, the concept of 'learning', or the acquisition of knowledge, clearly has an existence outside that of suggestion. Of course one can suggest to someone that he or she is learning something, but this does not mean that he or she is actually acquiring any knowledge. Similarly earthworms and rats can 'learn' or 'acquire knowledge', but surely Woody and Sadler would not want to argue that 'learning is a term that refers to any instruction or procedure that either explicitly or implicitly conveys to an organism, including an earthworm or rat, that it is about to enter, or has entered, a special state we call learning'. In contrast, my definition is based on the premise that hypnosis has no literal existence outside that of a suggestive procedure.

Significantly, from claiming that the range of explanations I provide covers 'almost anything', Woody and Sadler then switch to the idea that kinds of sociocognitive variables I put forward (such as 'expectancies') are insufficient to explain hypnotic phenomena. To me this smacks of an attempt to have one's proverbial cake and eat it. They can criticize my position for being either overinclusive ('almost anything') or underinclusive ('nothing but'), but not both.

Woody and Sadler finish their commentary by citing a paper by Szechtman et al. (1998) which they allege illustrates the 'special' qualities of hypnosis. Basically, in this study the researchers found that in a context defined as hypnosis, high susceptibles who reported that they hallucinated an auditory message showed different patterns of brain activation from highs who did not report such hallucinations. Moreover, these patterns were different from those produced when the same subjects were asked to imagine the same message. Again, this is all very interesting, but given we do not know how the individuals concerned, as well us others, would respond given the same range of expectancy manipulations and imagination tasks in an entirely different context, it tells us nothing whatsoever about the influence of 'hypnosis'. For instance, even if we ignore the possible influence of methodological problems, such as the 'holding back effect' in within-subjects designs (Sheehan and Perry, 1976; Wagstaff, 1981), what these findings would seem to indicate is that some people who score high on suggestibility tests, when they expect to hear a stimulus, are able to hallucinate the stimulus to some degree, and an expectancy manipulation for repetition of the stimulus is more effective in producing this response than a simple imagination instruction. I am sure that a number of sociocognitive theorists would have no difficulty in accepting such a proposition (see, for example, Perugini et al., 1998).

In sum, though Woody and Sadler disagree with me on a number of issues, some of their supposed criticisms are simply restatements of points I have made; indeed, their idea of perceiving the concept of state as a label for the emergent properties of various combinations of sociocognitive factors points to some promising possibilities for future agreement.

Measuring hypnotic susceptibility

I endorse entirely Kirsch's cogent comments on various conceptions of the altered state position. However Kirsch, and Lynn and Fite also, draw attention to the problem of how acceptance of the suggestion that one is or has been hypnotized is to be measured.

Kirsch is obviously rather more optimistic than I am about the possibility of measuring hypnotic susceptibility by looking at increases in outcome measures following hypnotic procedures. However, whilst I agree with Kirsch that the most interesting and important aspect of hypnosis is that it can elevate responsiveness to suggestion and thereby affect treatment outcome, it does not necessarily follow that such difference effects are thereby the best, or most accurate, way to measure the construct.

Having said this, I am by no means entirely happy to rely on depth reports as measures of hypnotic susceptibility, though they do avoid the problems of plateau or ceiling suggestibility effects. Perhaps the answer lies in some combination of depth measures and scales with a range of more difficult suggestions.

The future

In their commentary, Lynn and Fite concentrate on the future of different conceptions of hypnosis, and draw attention to the vacuous nature of the 'altered state' or 'trance' concept as an explanatory variable in real-life events, or a way of developing and promoting hypnotherapeutic procedures. Once again, I endorse their important observations and thought-provoking analysis.

It is my experience also that when it comes to legal debates about the status of hypnosis as an interrogative tool, or a source of pseudomemories, the only constructs that are ultimately of practical significance are those derived from mainstream social and cognitive psychology. For example, one can have a sensible debate about whether, in a particular instance of investigative hypnosis, a witness was pressured, or misled through leading questions asked within a context that encouraged unrealistic expectancies; one can also examine whether hypnotic procedures, through context reinstatement, and the provision of reassurance, might have helped a witness remember and report more. In contrast, debates about whether the 'hypnotic state' is a beneficent, benign or malignant force in memory recovery turn out to be totally futile.

Similar problems have cropped up in relation to the alleged 'dangers of hypnosis'. If we want to establish whether or not a person was 'damaged' by a session with a hypnotist, we can get somewhere by looking at the sociocognitive variables concerned (such as, was the injured party a psychologically vulnerable person who was socially pressurized, manipulated, tricked, given anxiety-provoking expectancies, or socially humiliated by the particular social context?) Such discussion is again eminently more fruitful than those I have witnessed about whether 'the hypnotic state' is benign or malignant and whether the injured party was 'in' or 'out' of 'trance', or in a 'shallow' or 'deep' trance, at the time he/she was being stressed, humiliated, manipulated etc.

However, perhaps the most important feature of Lynn and Fite's analysis is their emphasis on the positive role that sociocognitive models can play in promoting hypnotherapeutic procedures. It seems to me that so long as it is assumed that the capacity to benefit from hypnotherapeutic procedures relies principally on some relatively unmodifiable capacity to experience 'trance', whatever that is, then it is difficult to see how the benefits that can derive from such procedures can be significantly advanced. In contrast, as Kirsch has consistently argued, an approach that stresses the importance of developing positive attitudes and expectancies would seem to have considerably greater potential.

What so far has been lacking from all sides in these debates, however, is a definition of hypnosis that will successfully distinguish the 'hypnotic' from the 'non-hypnotic'. I hope that my definition provides a small step in the appropriate direction.

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