

THE RHETORIC AND SCIENCE OF ‘NOTHING BUT’

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Like Wagstaff (1998), we believe there are some important logical problems in how psychologists define and conceptualize the notion of a hypnotic state. However, here we will advance a different view of the nature of these problems. In addition, we will argue that the approach proposed by Wagstaff, although intriguing, compounds these underlying problems rather than solving them.

Perhaps the crux of Wagstaff’s (1998) position is that psychologists who try to describe hypnosis in terms of an altered state may simply be committing a basic logical error. As an illustration of the sort of category error involved, he describes someone who, being shown a theatre, library and other buildings, illogically asks, ‘But where is the university?’. Wagstaff’s account of hypnosis mainly emphasizes compliance, along with some allusions to motivation and expectancies. Presumably, then, it might be similarly illogical for someone hearing this account to ask, ‘But where is the hypnosis?’. That is, Wagstaff argues that hypnosis is not necessarily ‘something other than’ compliance and so forth (p. 156).

Nonetheless, this argument seems to miss the main issue. Let us consider some other illustrative examples. Is it illogical to look at a pile of lumber, wallboard, shingles and so on, and ask, ‘But where’s the house?’. Is it illogical to point out that music is something other than a series of notes, or that a human is something other than a collection of a few kinds of atoms? The issue is that most phenomena have emergent properties: They only acquire their basic characteristics when their components are organized so as to yield new properties that they did not possess separately.

Most likely, hypnosis has such emergent properties, too, and one of them may be an altered state of consciousness. The involvement of, to use Wagstaff’s term, ‘mundane’ components does not mean that the emergent phenomenon, too, must be mundane in the same sorts of ways. Thus, we respectfully reject the implicit reductionism in Wagstaff’s characterization of an altered state in hypnosis as a category error. Whatever the strengths and weaknesses of the altered-state notion turn out to be, it is not simply an error of logic to entertain it.

Another argument that Wagstaff raises against an altered state of consciousness in hypnosis is basically empirical: Researchers have failed to find definitive indicators of the presence versus absence of a hypnotic state. Although Wagstaff’s emphasis is on psychophysiological markers, other sociocognitive theorists have also made this point more generally. For example, Kirsch and Lynn (1995) note the ‘consistent failure to find any reliable markers of the hypothesized state’ (p. 849). Thus, even if the altered-state notion is not inherently illogical, perhaps the quest for empirical indicators of it has been so unsuccessful that it is time to give up on it – there’s just no ‘smoking gun’.

Our objection to this line of reasoning is that it seems to entail peculiar and troubling logical problems. First, it would seem illogical to hold states to higher evidential standards than other psychological constructs. A state is a hypothetical or latent

variable, just as a trait is; the difference is simply that states have more modest temporal stability than traits do. Hence, it does not make sense to insist that states be measured with more infallible indicators than we accept for other psychological constructs, such as traits. For example, no one objects to constructs such as intelligence, the Big Five and hypnotizability on the grounds that, to use Wagstaff's (1998, p. 156) words, no 'definitive set of physiological markers' has been discovered for any of them; thus, why insist that a hypothesized state, such as that due to hypnosis, meet a higher standard? Indeed, other widely recognized states, such as moods and hunger, also lack any infallible indicator, but this does not stop researchers from researching them sensibly and productively. The issue, then, is not the lack of a nearly infallible measure for a state; instead, it should be whether a particular state construct has explanatory value and leads to testable, interesting hypotheses.

Second, that underlying states may be difficult to measure well does not mean they do not exist or are unimportant. The anti-state position on hypnosis is reminiscent of the well-known debate in the cognitive-dissonance literature over whether the behaviours obtained in dissonance studies are attributable to an underlying state of cognitive dissonance. Bem (1967) created quite a stir by arguing that no underlying state was needed to explain so-called 'dissonance-reducing' behaviours, and that the existence of the hypothesized intervening state of dissonance had never been demonstrated well. More recent thinking has reinstated dissonance as a mediating state, along with the recognition that although clearly present, it is quite difficult to measure well.

Third, there is considerable foginess about what the term 'altered state' is supposed to signify. Indeed, the irony is that after so extensively critiquing altered-state views, what Wagstaff offers as his own alternative definition of hypnosis is arguably itself a radical altered-state view! He defines hypnosis as any suggestion that one is 'entering a special state ... we call 'hypnosis' (Wagstaff, 1998, p. 159), such that 'if you can convince people that they have been 'hypnotized' then they have' (p. 161). Wagstaff does not equate the belief that one is hypnotized with an altered state of consciousness: 'whether they are actually in or out of, or have been in an "altered state of consciousness" ... is an irrelevance' (p. 161). But the belief that one is hypnotized is *itself* an altered state of consciousness – that is, a state of awareness that one clearly did not have prior to hypnosis. (People who *believe* they are in love, or hungry or hypnotized behave differently – why should we reject their belief as an inadequate indicator of an important state of consciousness?)

Indeed, Wagstaff's definition of hypnosis implies a close-to-infallible indicator of the hypnotic state: the subject, when asked what state he or she is in, should respond, 'hypnotized'. In addition, a person who is unaware of being hypnotized is, by Wagstaff's definition, not hypnotized; therefore, it is a state that is clearly differentiable from most other, everyday states – to wit, an 'altered' state. Hence, we have the paradox that Wagstaff rejects the 'traditional' connotations of a hypnotic state while at the same time embracing them as the only thing that seems to define what hypnosis is. Therefore, we do not find in his view any particularly consistent perspective on whether hypnosis is a state of consciousness, and whether it is 'altered' or not.

Fourth, the term 'altered state' as applied to hypnosis seems to have a history of unproductive surplus meaning, akin to magic crystals and tarot cards. In contrast, taken literally, an altered state is simply a changed or modified mental condition. It is bewildering that sociocognitive theorists tend to attack the very idea of an altered state in hypnosis. Their explanatory variables, like motivation and expectancy, *are* state variables, and the way Wagstaff and others refer to them implies that the

relevant motivations and expectancies are hypnosis *specific*. Furthermore, in studies these state variables are typically modified or 'altered' with experimental manipulations, and the assumption is that when different behaviour ensues, it was mediated by some intervening state (often measured with a manipulation check). Special trait theorists, likewise, typically infer the existence of mediating states of some sort, and usually admit at least some influence of the situation on such states. Thus, the question of whether there is a 'state' of hypnosis is not, in our view, where the actual underlying disagreement among various theorists lies, as indeed Wagstaff suggests at various points in his article. Almost all theories imply some important mediating state variables – where they differ is on the *nature* of such variables.

So, to what sort of theoretical variables does Wagstaff appeal in explaining hypnotic phenomena? He provides a list, not meant to be exhaustive, of no less than 14 various sociocognitive processes that are probably involved, including motivation, relaxation, imagination and so forth. It is difficult to see how this sociocognitive umbrella is simpler or more parsimonious than the dissociative one that he criticizes. However, even the 14 sociocognitive variables are modest in comparison with Wagstaff's (1998) final position, which is wide open in the range of explanatory variables it embraces, even taking in opposites:

Hypnosis can be a state of alertness, or relaxation and drowsiness; a state of focused concentration or a state of diffused attention; a state of decreased suggestibility or a state of increased suggestibility; a state of uninhibited, uncritical imaginative involvement, or one of critical, analytical, convergent thought, and so on. It can be whatever is most suitable for the client. (p. 162)

Unfortunately, saying that something is almost everything is not saying much. Rather than allowing more agreement among researchers, as Wagstaff claims, this unconstrained view means that little or no meaningful agreement is even possible.

Let us look at two statements of the sociocognitive position more closely. Consider the position statement Wagstaff quotes approvingly from Barber (1974, p. 4), but with just one change – the term 'problem-solving' substituted for the word 'hypnotic': 'Subjects carry out so-called "problem-solving" behaviours when they have positive attitudes, motivations, and expectations toward the test situation which lead to a willingness to think and imagine with the themes that are suggested.' This change results in a statement that almost no one would disagree with, but which, likewise, almost no one would find particularly insightful. There is hardly any behaviour for which positive attitudes, motivations and expectations would not be somewhat relevant, but there is also hardly any behaviour for which they would be sufficient.

Similarly, consider Wagstaff's (1998, p. 160) definition of hypnosis, but with the word 'learning' substituted for 'hypnosis': Learning is 'any suggestion that we are, will be, or have been, in a special condition we call learning'. This statement would not contribute to a better understanding of learning. Why, then, are such position statements taken as insights into the nature of hypnosis?

Such statements seem to derive their rhetorical force from the implication that hypnosis is *nothing but* positive attitudes, motivations and expectations, or that hypnosis is *nothing but* the suggestion of a special condition called 'hypnosis'. But to show, for example, that hypnosis is affected by expectancies falls far short of showing that hypnosis is nothing but expectancies. The set of 14 sociocognitive variables, even if they could all be measured well, would be likely to explain only a fairly modest proportion of the variance in hypnotic responses. There is a parallel here with research

on alcohol intoxication: social variables such as expectancies are clearly predictive of intoxicated behaviour, but such behaviour is not the sole result of such variables. In short, the implied 'nothing but' position sounds impressive, but does not deliver.

Finally, Wagstaff repeatedly pits 'mundane' or 'ordinary' processes against 'special' ones. We believe this is an unfortunate and unproductive dichotomy. It is indeed likely that, some day, hypnotic phenomena will become fully explainable in terms of widely accepted, general psychological knowledge. In effect, Wagstaff proposes that this has already happened. In contrast, we, along with many other hypnosis researchers, believe that hypnotic phenomena are still puzzling, and that the puzzles they pose serve as a valuable window on some general psychological processes that are not yet well understood.

For example, in recent research (Szechtman et al., 1998), we studied the PET scans of hypnotized highs who were hallucinating an auditory message (when they expected to hear it again but it was not actually played). The resulting patterns of brain activation (that is, underlying states) turned out to be quite distinct from those obtained when the same participants were hearing the message or imagining it, and also clearly distinct from the patterns obtained from equally highly hypnotizable participants who failed to experience the hallucination. These results, we argued, potentially shed light not only on hallucinations in schizophrenia (which appear to be associated with somewhat similar patterns of brain activation), but also on the underlying processes that normally mediate reality monitoring. In such ways, the special and the mundane are two sides of the same coin, and the 'special' qualities of hypnosis will, we hope, illuminate general principles that underlie the 'mundane'.

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