

DISCUSSION COMMENTARY

HYPNOSIS: REINSTATING THE STATE

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Theodore Barber's paper 'Hypnosis: a mature view' is important for a number of reasons, not least because it reveals just how far theories of hypnosis have evolved since the days when he could reasonably have been called the *enfant terrible* of the hypnosis world. Then he was of course strongly 'non-state' in his perspective, and 'hypnotic' appeared only in inverted commas (see, for example, Barber, Spanos and Chaves, 1974). Now he offers an account which can embrace 'trance', 'state' and 'neodissociation'. Indeed, he is prepared to write 'The "trance" view more or less describes the behavior of some amnesia-prone subjects ... the ability to 'block out' or isolate stimuli, memories and experiences underlies their hypnotic talents, [including] hypnotic analgesia, anesthesia ... and amnesia' (Barber, 1999: 37, 38). The quotation is from the chapter on which the current target article is based, and it is unfortunate that here he has had to distil his ideas into so short a paper. I shall refer to material in the original chapter, since it gives a fuller account of his 'three-dimensional' theory of hypnosis.

In some ways Barber's ideas are not entirely new, and I am not convinced that they really qualify to be treated as a paradigm shift. Thus, Sheehan has for a long time argued the case for addressing the different cognitive styles which make possible 'the essential richness and variety of hypnotic experience' (Sheehan, 1986: 159). Interestingly, he also proposed the existence of *three* cognitive styles (Sheehan and McConkey, 1982; Sheehan, 1986). Barber's three styles do not map very closely on to those of Sheehan, and one wonders whether the choice of three has a certain arbitrariness. One accepts that the Pekala studies (Pekala, 1991; Pekala, Kumar and Marcano, 1995) lend statistical support to the notion of three broad styles, but again the match with Barber's types is not exact. As Barber indicates, his basic categories have the potential to be divided into subtypes, and it seems almost as reasonable to suggest that the three could be collapsed into one or two superordinate categories. Barber himself seems to see nothing sacrosanct about the number three, since he suggests (p.41) that future research will 'delineate the characteristics of additional types and subtypes'. In other words, there is perhaps something of a continuum, and dividing it into different sections is a matter of convenience. Certainly, the distinction between the fantasy-prone and the amnesia-prone types is somewhat blurred, both in some of the characteristics associated with their response to suggestions, and in the antecedents alleged to produce the type. Thus, both the fantasy-prone and amnesic subjects were said to exhibit 'psychosomatic plasticity', and many women of each type had experienced false pregnancies. One of the causes for the cognitions of both types is said to be that they were acquired as an escape from an undesirable childhood environment. The major differences cited for these groups are in fantasizing and memory. The amnesia class were described as not having 'real-as-real' fantasies, and here there may be a genuine difference, since outside the hypnosis context

people do vary in their ability to generate convincing mental images. Barber states that many fantasizers report memories from before the age of 2, whereas amnesics seldom had memories for earlier than 5. However, this difference in remembering abilities is possibly not what it seems, for the ability to recall genuine memories from before the age of 2 seems implausible. That age is well within the generally accepted period of infantile amnesia (Campbell and Spear, 1972). (For a wider consideration of early memories see Brandon et al., 1998.) Since the fantasy-prone subjects are given that name for a very good reason, it would appear likely that the reported memories are also fantasies; certainly Barber cites no research attempting to test the veracity of the alleged recollections.

In summary, it seems possible that the fantasizers and amnesics are relatively close in their behaviour, but have differences imposed by the inability of the latter to generate convincing mental experiences. In the absence of vivid sensations, the amnesia-prone would not establish clear episodic memories of their hypnotic experiences, and hence tend to be amnesic for them.

What of Barber's third class of good hypnotic subject, the 'positively set person'? These are people with positive attitudes and expectations regarding hypnosis, which allow them 'to think with and imagine the suggested ideas while letting go of extraneous and contrary thoughts' (p.28). Barber says little of the experiences that result from this thinking and imagining, but he does quote Kirsch (1991): 'they may devise and implement various cognitive strategies aimed at the goal of experiencing hypnotic suggestions'. Thus, the end product for this class of subject may be rather similar to that of the other subjects, the difference being that the experience is unfamiliar to them (no unhappy childhood to escape?), and hence they require some guidance to achieve the desired goal.

In view of the preceding considerations, I question whether the differences between the three types of subject are as great, or as potentially informative, as Barber's paper implies. To me, the paper's importance lies more in the change of approach to hypnosis that it brings to the centre of the hypnosis debate.

Barber's new emphasis is perhaps encapsulated by the term he consistently employs to describe the non-state position: 'cognitive-behavioural-social-psychological'. I like this term, since it reverses the commonly used 'sociocognitive'. The latter seems to me to be an inappropriate hangover from the days when there was a powerful (and justifiable) swing away from overly gullible state-like theories. At that time, researchers spent a great deal of effort in showing that hypnotized individuals did not do anything that simulators could not do, and it seemed reasonable to look to 'interpersonal' accounts for the hypnotic behaviour; in a sense, Milgram (1974) was the metaphor. However, the equivalence of the hypnotic and simulated behaviour should have been used only to emphasize that hypnotic subjects knew what was expected; the results did nothing to explain *why* the behaviour was exhibited, and it was not a foregone conclusion that the behaviour was a simple product of compliance. Indeed, for a long time that account has been difficult to maintain logically; for example, I have pointed out that in employing a battery of susceptibility tests researchers clearly did *not* want their subjects to be compliant to every test (Naish, 1986a). More recently, Wagstaff (1998) has made similar points, and he, like Barber, can be seen to have shifted from the earlier 'fiercely rational position' (Naish, 1986b: 162). However, Wagstaff is still very ready to place emphasis on the 'social' component of the behaviour and would seem to be a member of the group described by Barber as 'Investigators ... of hypnosis [who] have focused their attention upon the positively-

set subject' (p.31). It seems unsurprising, and ultimately uninteresting, that a subject about to submit to what is expected to be an unusual, perhaps 'scary' experience, will do better if positively motivated, at ease with the experimenter and so on. When we are faced with all the fascinating behaviour and experiences reported by susceptible subjects, it seems strange to focus interest on the social dynamics that prepare them to undergo hypnosis. As I have argued for a long time (for example, Naish, 1985), it is the behaviour and the experience that are deserving of explanation. Now Barber has relegated the 'social' part of the phenomenon to third place, which seems far more appropriate; of course social psychological processes come into play in hypnosis, but so they do in many other aspects of human behaviour, without requiring that those processes should always take centre stage when developing a theory for the behaviour.

What is Barber's theory for the behaviour? Unfortunately, he does not present a well-worked-out explanation for the behaviour, even in his longer chapter, being content simply to show that three sorts of people can exhibit different aspects of it. However, most significantly, he does address the question (p.37): 'Is hypnosis an altered state of consciousness?' To this he responds that the three hypnoses he has postulated each has its own state of consciousness. This is a remarkable statement to come from a founding father of the non-state position. Could he just be invoking the idea of a multitude of 'states', rather as Wagstaff (1998) does, simply to show that the term has no real utility? It would seem not: for example, we read (p.37) 'the hypnosis of the amnesia-prone, which has sleep-like characteristics with apparent automaticity followed by amnesia'. The quotation with which I began this paper also dealt with the amnesic subjects, referring to their ability to isolate stimuli so as to achieve experiences such as analgesia; clearly, Barber sees these as real changes in consciousness, and implies a cognitive underpinning.

Surprisingly, Barber makes no mention of recent 'brain-mapping' techniques, which are offering a different insight into the hypnotic process. Perhaps he has avoided such material because it has not as yet revealed three types of neural activity, to correspond with his types; it will be interesting to await future developments in that regard. What has been demonstrated is that susceptible subjects are able to modify patterns of activity in exactly the ways one would expect, if they were genuinely having the experiences they claim. Thus, using functional magnetic resonance imaging (fMRI), Crawford et al. (1998) showed shifts in activity in the anterior cingulate region during suggestions for analgesia. They believe that the region forms part of a circuit involving the thalamus and other attentional pain control systems. In the past it has been common to ascribe hypnotic pain control, in part, to distraction (for example, Wagstaff, 1986), but De Pascalis, Magurano and Bellusci (1998) reported that the greatest reductions in pain reports were achieved when subjects focused attention on the area to be made analgesic. Furthermore, it was in this condition that larger EEG effects were detected. It would seem that the neural underpinnings of Barber's states of consciousness might be beginning to emerge.

Should neural activity be used to justify the concept of a 'state of consciousness'? Wagstaff (1998) would say not, since there is no evidence for a unitary neurophysiological condition, uniquely associated with hypnosis. However, this level of scepticism can be seen as unnecessarily restrictive, resulting perhaps from Wagstaff's use of 'state'. The title of his 1998 paper is *The semantics and physiology of hypnosis as an altered state: Towards a definition of hypnosis*. Certainly, it would be hard to argue for a single altered state, if there were a number of different neural states. Barber helps

us in this matter by using the longer term 'state of consciousness'. When a person has achieved the necessary pattern of neural activity to escape the distress of pain, it seems reasonable to describe their consciousness as having been altered. A common rejoinder to this is that we could then feel free to describe every nuance of 'mental state' as yet another state of consciousness. Indeed we could, but since we are only beginning to find tools to research these 'states' it seems particularly unhelpful to try to be so fine-grained in separating them. There is nothing fine-grained in what can be detected by EEG, and even the much higher-resolution fMRI is a relatively coarse tool. Similarly, there was nothing subtle in the change of consciousness of a pain sufferer whom I saw, after she had exhausted everything a pain clinic had to offer. Following hypnosis she reported that the pain had completely left her. These are the sorts of clear change of consciousness that we can observe, and have a hope of following neurophysiologically. If we accept that there has been a change, then it seems reasonable to accept also that it had a start state and an end state. We have an altered state of consciousness.

I will try to draw all this together. Theodore Barber may or may not turn out to be right in identifying three kinds of excellent hypnotic subject, but to me that is relatively unimportant. The stimulating aspect of his ideas lies in the way he presents them. Who would have expected the Barber of 20 years ago to offer a mature view that 'harmoniously encompasses' trance, state and neodissociation? He has shown that it is reasonable to focus on the phenomena of *hypnosis* (rather than in simulators or other non-hypnotic situations), and to consider what sorts of people can adopt these altered states of consciousness. It does not matter that the states may not be unique to the hypnosis situation; what is of interest is how they are achieved.

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