

Contemporary Hypnosis (2005)
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ALTERED STATES OF CONSCIOUSNESS AND HYPNOSIS IN THE TWENTY-FIRST CENTURY

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Abstract

The contemporary perspective on altered states of conscious is surveyed as an introduction to commentaries on Kallio and Revensuo's lead article in *Contemporary Hypnosis* (2003). It is noted that the study of consciousness, unconscious processing, and altered states of consciousness are central issues in neuroscience, heralding fresh approaches to the neuroscientific understanding of hypnosis. These include attempts to bring together new neurophysiological methods with phenomenological report. The alteration in hypnosis of anterior brain processes including the anterior cingulate cortex and left dorsolateral prefrontal cortex are particularly productive areas of research. The lack of engagement with neuroscientific evidence from theorists with a purely social and cognitive orientation to hypnosis is noted, with examples provided from research on attention and relaxation. Unifying the field awaits active collaboration between scientists with neurophysiological and social orientations.

Key words: altered states of consciousness, attention, frontal lobe, hypnosis, relaxation

Introduction

In this issue commentaries are provided on Sakari Kallio and Antti Revensuo's theoretical article in *Contemporary Hypnosis* (2003) 'Hypnotic phenomena and altered states of consciousness: a multilevel framework of description and explanation'. The writer is in sympathy with a number of their viewpoints.

The contemporary perspective on Altered States of Consciousness (ASC)

The neuroscience of ASC has come in from the cold, largely because the scientific study of consciousness, per se, has for a decade moved centre stage in cognitive and affective neuroscience (Gazzaniga, 2000; Mesulam, 2000[**AQ Mesulam in refs - which is correct?**]; Zeman, 2001). Consciousness is inextricably bound to first person experience. Historically, though phenomenological report was the essential preoccupation of the foundational beginnings of psychology as a science, it was outlawed during much of the twentieth century when scientists were in the thrall of behaviourism and black box experimental psychology. While making a brief reappearance in the 1960s, phenomenological report is again heralded, this time in tandem with objective measures in fields such as psychophysics, metabolic brain imaging and psychophysiology. The EEG, once useful in times past in defining stages of sleep and in providing evidence of seizure

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discharge, is undergoing a renaissance and together with MEG is making insightful contributions in the underpinning of cognitive functions through the examination of the role of oscillations throughout the EEG spectrum, but notably the gamma rhythm with its potential for elucidating the binding problem in consciousness (Singer and Gray, 1995; Varela, 1995[**AQ: Varela in refs - which?**]). This takes us way beyond the foundational work that applied EEG to hypnosis and which measured levels of consciousness in a purely activational sense using as an analogy the alteration of consciousness in sleep.

In the light of these developments we can anticipate a new generation of electrophysiological studies of hypnosis. To provide one example, Croft, Williams, Haenschel and Gruzelier (2002) have shown that as the intensity of painful stimuli increased, so did the amplitude of gamma oscillations recorded over prefrontal cortex. Source localization procedures indicated that the generator was in the anterior cingulate cortex, historically regarded as limbic cortex. Following an hypnotic induction, however, this relation no longer held in hypnotically highly susceptible participants who experienced both hypnosis and analgesia, though it was unaltered in those with low susceptibility who did not experience hypnosis or analgesia. Importantly the hypnotic experience was paralleled by a dissociation between the brain's response to the painful stimuli and the subjective experience of pain.

In cognitive neuroscience strides have also been made through the realization of the importance of unconscious processing as a determinant of everyday behaviour.

Everyday conscious awareness is but the tip of an iceberg, underneath which there is a realm of relatively uncharted processes, which are likely to be just as complex as those of so-called altered states of consciousness (ASC) which have currently tended to defy systematic elucidation; the brain is functionally in a constant state of flux and alteration. (Vaitl, Birbaumer, Gruzelier, Jamieson, Kotchoubey, Kübler, Lehmann, Miltner, Ott, Pütz, Sammer, Strauch, Strehl, Wackermann, Weiss, in press)

Studies with a range of methods in healthy subjects have shown conclusive evidence of how information which is not processed consciously can determine future actions, and particularly motivation (Gazzaniga, 2000; Mesulem, 2000). The question is raised from this perspective as to whether a neurocognitive reframing of Eriksonian dynamics, recently embraced by social theorists (Lynn and Hallquist, 2004), is perhaps not too short a step away?

Coincidentally ASC are coming out of the wilderness of 'fringe' concerns. From 1998 the writer belonged to a German six-year funded consortium on Altered States of Consciousness, which in the author's case included research on hypnosis and schizotypal unreality experiences. The fruits of our consortium are published in the January issue 2005 of *Psychological Bulletin* (Vaitl, Birbaumer, Gruzelier, et al., in press). To give the contemporary flavour of ASC, the encompass of the review can best be conveyed by paraphrasing the following catalogue from the abstract of the article:

- (a) occurring spontaneously (drowsiness, daydreaming, hypnagogia, sleep, dreaming and near-death-experiences);
- (b) evoked by physical and physiological stimulation (pressure, temperature, starvation/diet, sexual activity/orgasm, and respiratory maneuvers),
- (c) induced by psychological means (sensory deprivation/homogenization/overload, drumming, dancing, relaxation, meditation, hypnosis, and biofeedback); and
- (d) caused by diseases (psychosis, coma, vegetative state, and epilepsy).

The emphasis was on psychological and neurobiological investigations while inclusion required in most cases consideration of more than one level of enquiry, with

levels ranging from the subjective to the biological. We specifically did not include pharmacologically induced ASC. As an aid to clarification and future research including hypnosis, our phenomenological analysis characterized four dimensions: activation, awareness span, self-awareness and sensory dynamics. The review disclosed that as well as arising from compromised brain structure, different states of consciousness mainly arose from transient changes in brain dynamics such as disconnectivity, and changes in neurochemical and metabolic processes. At a more subtle level of brain functioning, conscious experience could also be altered temporarily by environmental stimuli and their restriction, mental practices and techniques of self-control.

The reawakening of interest in ASC will offer new perspectives and will facilitate the revisiting of old considerations in a fresh way. As observed before, an example may be given by reconsidering the striking parallels between hypnosis and dreaming for the processes involved. In Llinas and Pare's model (1991) dissociations between specific and nonspecific thalamocortical systems underpin dreaming, which is conceptualized as a state of hyperattentiveness to intrinsic activity, without registering sensory input. Consider Fuster's (1995) description of cognitive features of dreaming which include the altered sense of time and absence of temporality, the lack of guiding reality and critical judgement, the anchoring in personal experience, affective colouring, dissociation from sensory input and context. 'The fragmented networks activated in the dream seem to lack the associative links to a time frame, anchored as they are in the present, without time tags and references. This could equally be a description of the hypnotic state as high susceptibles experience it' (Gruzelier, 1998: 18)

Evidence for an ASC in hypnosis.

Aside from neutral hypnosis, attempts are being made to map brain states evoked by specific challenges involving discrete phenomena that contribute to the domain of hypnosis, and unambiguously reside within ASC. These for example include hypnotic visual hallucinations and analgesia (e.g. Crawford et al., 1998[AQ: all authors needed]; Rainville et al., 1999[AQ: not in refs - 2002 only]; Wik, Fischer, Bragee, et al., 1999[AQ: all authors needed]; Faymonville, Laureys, Degueldre, et al., 2000[AQ: all authors needed]; De Pascalis, Magurano, Bellusci, et al., 2001[AQ: all authors]; Ray et al., 2002[AQ: not in refs]; Spiegel, 2003; Derbyshire, Whalley, Stenger, et al., 2004[AQ: all authors]).

In the case of neutral hypnosis, modest attempts have been offered (Gruzelier, 1998, 2004), to begin to capture that state of altered brain functional organization which represents neutral hypnosis when it is induced by classical hypnotic relaxation procedures, and to approach the temporal nature of the process (see Ray and De Pascalis, 2003, who tested an aspect of the model with affirmative results).

The greater investment of research has involved investigation of altered and disconnected frontal functions with hypnosis. It would appear that Kallio and Revensuo (page xx, this volume) have some sympathy for the findings: 'So far we believe that the neural correlates of the ASC of hypnosis might be found in the changed activity of the frontal areas (e.g. Gruzelier, 1998, 2000)'. Evidence in support of frontal alterations will not be gone in to here (see Gruzelier, 1998, 2000, 2004 and for recent empirical evidence see Croft et al., 2002; Gruzelier, Gray and Horn, 2002; Egner, Jamieson and Gruzelier, 2005). But this is not a flash in the pan as exemplified by the range of measures used which included event related potentials (Jutai, Gruzelier, Golds and Thomas, 1993; Kaiser, Barker, Haenschel, Baldeweg and Gruzelier, 1997; Gruzelier et al., 2002); EEG

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coherence (Gruzelier, 1998; Egner et al., 2005); gamma oscillations (Croft et al., 2002); haptic sorting (Gruzelier et al., 1984; Cikurel and Gruzelier, 1990); word and design fluency tasks (Gruzelier and Warren, 1993; Kallio, Revonsuo, Hamalainen, Markela and Gruzelier, 2001) and fMRI (Egner et al., 2005).

Two regions frequently implicated have been the anterior cingulate cortex (ACC), which is of central relevance to hypnotic analgesia, and the left dorsolateral prefrontal cortex (DLPFC), with evidence of decoupling between the two (Egner et al., 2005). While there is more neurophysiological evidence to hypnotic analgesia than this (e.g. Gruzelier, 1998; Rainville, Hofbauer, Bushnell, et al., 2002[**AQ: all authors**]), one cannot turn a blind eye to the involvement of the ACC and DLPFC.

Engaging with the evidence

What is conveyed by Kallio and Revonsuo's lucid discourse, and the conflicting view points in the commentaries on their article in this issue, is an impasse in the field between state and nonstate hypnosis theorists. One of the primary reasons in the writer's opinion for the current impasse is one of lack of engagement with the neurocognitive evidence. This is due in turn to differences in pedagogical background and to the complexity of the more reductionist levels of explanation such as the neuroscientific. As has been spontaneously admitted by virtually all the nonstate proponents that I have met, the neuroscientific evidence is beyond their field. As one manifestation of this, carry out a citation search and this will disclose virtually no discussion by those representing the so-called sociocognitive school of the more than 30 empirical and theoretical articles I have published. Now, while it is one thing to make the admission of a lack of understanding, it is quite unscientific to opine that there is no evidence for an ASC perspective, and to go on and attribute hypnosis to purely psychological constructs. Especially when this flies in the face of evidence which contradicts such a restrictive attribution (Gruzelier, 2000).

Those who prefer a nonstate perspective commonly attribute background alterations in brain function in hypnosis to focussed attention and/or to relaxation. This is based on wishful thinking rather than on any scientific analysis of neurocognitive evidence. Some of this 'unacknowledged' ground has been covered before in *Contemporary Hypnosis* (Gruzelier, 2000: 57) and is paraphrased here. Examples of just two processes will be given.

Focussed attention

Our evidence contradicts the view that when responding to instructions of hypnosis the shifting and focussing of attention is no more than the ordinary processes of selective attention. This evidence comes from measuring electrodermal orienting responses and auditory event-related cortical potentials, both of which are not susceptible to self-regulation without extensive training. The attention of highly susceptible subjects when responding to instructions of hypnosis we found to differ from their attention when absorbed in a story and from when they were deeply relaxed. It also differed from simulation of hypnosis and it was different from the attention of subjects with low susceptibility who were responding to instructions of hypnosis (Gruzelier and Brow, 1985; Gruzelier, Allison and Conway, 1988; Jutai et al., 1993; Gruzelier, 1998).

Relaxation

Alterations of brain function resulting from hypnosis in highly susceptible subjects have been distinguished from relaxation in the following ways:

- 1) Electrodermal orienting responses of fronto-limbic origin differentiated hypnosis from a relaxation control condition which had been experienced a month earlier or later than the hypnosis session, and there were no differences in levels of tonic arousal indexing sympathetic autonomic activity (Gruzelier and Brow, 1985).
- 2) The pattern of response during hypnosis also differentiated subjects who simulated hypnosis (Gruzelier et al., 1988).
- 3) A left to right anterior hemispheric shift in hypnosis disclosed with a haptic sorting task requiring the identification of objects through active touch while blind folded (Gruzelier, Brow, Perry, Rhonder, Thomas, et al., 1984[AQ: all authors]) withstood an *active-alert* induction whereby subjects pedalled a stationary bicycle while following instructions of hypnosis with suggestions of mental invigoration (Cikurel and Gruzelier, 1990).
- 4) The lateral shift with hypnosis in the haptic task was also differentiated from relaxation by comparing performance with relaxation in a floatation tank (Raab and Gruzelier, 1994). Whereas with hypnosis there was a decline (inhibition) of left hemispheric processing, the degree of which correlated with the scale of hypnotic depth obtained during the task (Gruzelier et al., 1984), this left anterior relation was absent with floatation, yet floatation shared with hypnosis a right hemispheric enhancement in haptic processing. These dynamics with floatation were also mirrored in lateralised recognition memory tasks (Raab and Gruzelier, 1994).
- 5) Alpha and theta activity have also distinguished hypnosis from the effects of relaxation, not only during hypnosis but after 'dehypnosis' (Williams and Gruzelier, 2000). (Gruzelier, 2000: 57)

Unification of the hypnosis field and understanding

Consideration of levels of explanation by Kallio and Revensuo (2003), also entertained by Hasegawa and Jamieson (2003) from my laboratory, is helpful in this regard. However, just as it is clear that the phenomenological level is of fundamental importance to hypnosis as it is to ASC in general, and the methodological innovations of McConkey (McConkey, Wende, Barnier, 1999) are welcomed, it is also clear that ASC require biological changes that are different from ordinary SC. Theoretical differences between social and physiological theories of hypnosis are not simply a difference in levels of explanation.

In fact an integrated multilevel approach has been the underlying ethos of our research exemplified by a neurocognitive translation of the psychophysiological and neuropsychological results (Gruzelier et al., 1984; Gruzelier, 1998). This has been an attempt to gain an understanding of the hypnotic induction process, and in so doing tackle the question of why we induce hypnosis the way we do from a neurocognitive perspective.

As indicated earlier, and echoed here by Lynn, much, much more can be gained towards the goal of unification. Perhaps this may be accomplished not only through open-minded discussion and interaction, but best of all by formulating investigations by neurophysiologists, cognitive and social psychologists collaboratively.

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ON THE VIRTUES OF VIRTUOSOS

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Abstract

We contrast the proposals of Kallio and Revonsuo (2003) with some of Ernest Hilgard's classic innovations in hypnosis research. In particular, we note some empirical difficulties with the reliance on hypnotic virtuosos as the basis of hypnosis research, rather than Hilgard's dimension-based individual-differences method. We also discuss the idea that deceptive hypnotic suggestions may have important properties that the more usual, invitational hypnotic suggestions lack.

Keywords: hallucination, hypnosis, hypnosis scales, hypnotic virtuoso

At the heart of the target article by Kallio and Revonsuo (2003) is a call for revolution. To appreciate this, it is useful to provide a brief historical context.

As is well known, Jack Hilgard and his colleagues at Stanford University in the 1950s, 60s, and 70s virtually invented modern hypnosis research. One of their seminal contributions was methodological: they developed standardized scales for measuring hypnotic responsiveness, whereby a person's standing on an underlying, reasonably normally distributed trait is indexed by the total number of suggestions passed (Hilgard, 1965). With this development, the overwhelmingly prevailing paradigm in hypnosis research became to pretest people on such scales and select relatively high scorers and relatively low scorers to contrast in studies of hypnotic phenomena. This approach 'domesticated' hypnotic responsiveness, so to speak: rather than seeming strange and intractable, it could be studied just like any other psychological trait such as intelligence.

A second seminal contribution of Hilgard was theoretical. His neodissociation theory proposed that in hypnosis the executive system becomes divided into two parts, with parallel, dissociated streams of consciousness (Hilgard, 1977). This conceptualization not only offered an explanation of many otherwise odd and singular phenomena in hypnosis (such as the 'hidden observer'), but also it connected these phenomena with everyday, nonhypnotic events, such as action slips, which were proposed to be relatively minor instances of the same mental mechanisms. Like the invention of the standardized scales, neodissociation theory helped to make hypnosis a more mainstream topic in psychology, in particular by embedding it in the broader landscape of everyday, nonhypnotic processes of cognition and attention.

In effect, Kallio and Revonsuo (2003) urge us to overturn much of this legacy of Hilgard, both the theory and the methodology. Concerning neodissociation theory, they

state that ‘we are suspicious about any use of the concept which implies that our normal integrated phenomenal consciousness could somehow be divided into two isolated but simultaneous streams by a hypnotic induction’ (p. 144). Instead, they cite the dissociated control theory of Bowers (1992) and Woody and Bowers (1994) as a more plausible and promising alternative. Although Bowers originally proposed this theory to rescue some parts of Hilgard’s neodissociation theory, its main ideas are actually much older. For example, consider the views of Sidis in 1899 (pp. 68–9):

The superior or the highest nervous centres ... possess the function of *choice* and *will*. ... These superior choice and will-centres, localized ... in the frontal lobes ... on account of their selective and inhibitory function, may be characterized as inhibitory centres *par excellence*[AQ: **original emphasis or yours?**].

In hypnosis the two systems of nervous centres are dissociated, the superior centres and the upper consciousness are inhibited, or, better, cut off, split off from the rest of the nervous system ..., which is thus laid bare, open to the influence of external stimuli or suggestions.

Thus, this part of Kallio and Revonsuo’s argument may, in effect, represent a cycling back to earlier ideas, which antedated Hilgard’s theory.

However, the revolution that Kallio and Revonsuo (2003) propose goes far beyond the rejection of neodissociation theory: they propose that Hilgard’s dimension-based individual-differences methodology for hypnosis research should be overturned. In particular, they argue that what standardized hypnosis scales measure in most people has little to do with the crucial phenomena of interest, which are unique to certain rare individuals, termed ‘hypnotic virtuosos’. Hence, the prevailing research paradigm, which contrasts relatively high versus relatively low scorers on standardized hypnosis scales, cannot illuminate the classic, true hypnotic phenomena, such as hallucinations. They propose, therefore, that we should abandon this paradigm and intensively study individual virtuosos instead. As in the theoretical domain, this suggestion would turn back the clock to a pre-Hilgard era, in which the study of hypnosis was much more a matter of case studies (e.g. James, 1890).

In a sense, Kallio and Revonsuo (2003) are suggesting that the current impasse in hypnosis research is iatrogenic. Instead of studying ‘true’ hallucinations in those capable of them, namely hypnotic virtuosos, we have conflated these phenomena with qualitatively different, easier ones in a broader spectrum of people. Kallio and Revonsuo argue that unlike true hallucinations, which may require a genuinely altered state of consciousness, direct motor and motor challenge suggestions require no special state and fall in the same category as ‘ordinary mental imagery’ (p. 129). In short, the dimensional, standard-scales view of hypnosis research inadvertently substitutes phenomena that only require ‘vividly imagining an experience’ for the real thing, which is ‘truly hallucinating an experience’ (p. 130).

Kallio and Revonsuo’s (2003) abandonment of Hilgard’s idea that hypnosis lies on some kind of continuum with certain everyday phenomena may be viewed as a bid, in effect, to restore some of the strangeness and uniqueness of hypnotic phenomena. In this way, it is somewhat comparable to Barber’s (1999) recent proposal that there may be three qualitatively different, alternative types of high responder to hypnosis: those with a positive set, those with high imagery capacity, and a small minority with unusual dissociative/amnestic capacities. Kallio and Revonsuo are clearly zeroing in on the last of these possibilities, in contrast to the first two.

Some uncomfortable evidence

Although Kallio and Revonsuo's methodological proposal is intriguing, it does not yet deal with certain lines of evidence. Interestingly, Hilgard carefully considered several of these lines of evidence in formulating his own, quite different views.

Of chief importance, the available evidence does not strongly support the hypothesis that there is a rare type of highly responsive person who differs in kind, rather than degree, from the rest of the population. Hilgard (1965) was himself intrigued with the idea that bimodality in the distribution of scores on hypnosis scales might possibly suggest the presence of two types, including a rarer, more capable one; however, bimodality is probably a problematic basis for inferring underlying types (Balthazard and Woody, 1989). In the most comprehensive approach to the typology issue to date, Oakman and Woody (1996) found some evidence in favour of two latent types in response to hypnosis scales; however, their results indicated that the 'high' type was not rare.

Likewise, the evidence indicates that even among the unusual people who are the very most responsive to hypnosis, there is considerable heterogeneity (McConkey and Woody, 2003). There may be several quite different ways to be a 'virtuoso', or several quite different profiles among 'virtuosos'. Hilgard's interest in this problem led to the development of the Stanford Profile Scales, designed to map different patterns of hypnotic aptitude among the most highly hypnotizable individuals (Weitzenhoffer and Hilgard, 1963, 1967). In addition to having different patterns of ability, the underlying strategies used by different virtuosos may be quite different as well. For example, an intensive case study of two virtuosos by McConkey, Glisky and Kihlstrom (1989) found that one of them was very passive cognitively, whereas the other was very active cognitively. Taken together, such lines of evidence suggest that there may be no unitary entity of hypnotic virtuoso.

Another important issue is that the available evidence does not support the strong qualitative distinction that Kallio and Revonsuo describe between hypnotic hallucinations and other classic, motor-based hypnotic suggestions. They ask, 'Why should we assume that such very different subjective experiences as a visual hallucination of an object and the feeling of heaviness in the hand should have a common underlying mechanism?' (p. 118). The answer is that the two kinds of responses are quite strongly correlated, suggesting a common mechanism to at least some extent (Hilgard, 1965). In very recent factor-analytic work on the combined item pool of the two most widely used hypnosis scales – the Harvard Group Scale of Hypnotic Susceptibility, Form A (HGSHS:A; Shor and Orne, 1962[**AQ: not in the references - please supply details**]), and the Stanford Hypnotic Susceptibility Scale, Form C (SHSS:C; Weitzenhoffer and Hilgard, 1962[**AQ: not in refs**]) – Woody, Barnier, and McConkey (2004) found that the Perceptual-Cognitive (or hallucination) factor correlated 0.69 with the Direct Motor factor and 0.67 with the Motor Challenge factor. Given these substantial correlations, it is difficult to see how expectancies and other sociocognitive variables could be fully explanatory for response to motor suggestions but irrelevant for hallucinations, as Kallio and Revonsuo argue. Instead, we believe that Kallio and Revonsuo's tendency to ascribe all motor-suggestion responsiveness to purely sociocognitive factors is premature and probably incorrect (e.g. see Woody, Drugovic and Oakman, 1997; Woody and Sadler, 1998).

Invitation versus deception

Kallio and Revonsuo (2003: 129) draw an interesting distinction between hypnotic suggestions that function as *invitations* versus ones that function as *deceptions*:

When given a suggestion, the subject is invited to imagine some alternative state of affairs ... This view implies that the subject is not being deceived or led to believe that such a state of affairs really is the case. By contrast, a deceptive suggestion would aim at convincing the person that the state of affairs described in the suggestion actually holds, i.e. that the world is different from what it actually is. A deceptive suggestion aims at bringing about a true hallucination rather than just ordinary mental imagery.

In a somewhat similar vein, Tellegen (1978/1979) argued that acts of imagining that are not experienced as real do not meet the minimal definition of hypnotizability; instead, 'it is the *act* of positing something imagined as real that characterizes a response as hypnotic rather than the *content* of the imagined event' (p. 220, [AQ: original emphasis or yours?]). However, the invitational quality of most hypnotic suggestions is not usually viewed as an impediment to this experience of reality. Indeed, some hypnosis experts (e.g. McConkey and Barnier, in press) would argue that even very difficult hypnotic suggestions function essentially as invitations, to which the participant responds by actively co-constructing the suggested experience.

Nonetheless, what is intriguing about the idea of deceptive suggestions is they may obviate certain kinds of competing explanations for a hypnotic response. If subjects cannot anticipate what kind of response is expected of them, then presumably they would have difficulty bringing into play processes such as 'voluntary intention, subjective effort, and focused attention' (Kallio and Revonsuo, 2003: 143). Thus, somewhat paradoxically, deceptive suggestions should be more difficult to experience, rather than easier.

Our impression is that truly deceptive suggestions are quite rare in hypnosis research. The suggestions on the standard hypnosis scales, including ones for hallucinations, have a clear 'as if' (or invitational) quality (Sarbin and Coe, 1972). There is not much mystery about why this is so: consider that deceptive motor-challenge suggestions, implying that the subject is actually paralysed, could raise tricky ethical issues. Similarly, the recent study of colour hallucination by Kosslyn, Thompson, Costantini-Ferrando, Alpert and Spiegel (2000), which Kallio and Revonsuo cite approvingly as a model of the kind of research they advocate, clearly used invitational rather than deceptive suggestions. Subjects were 'asked to alter actively the stimulus, to drain or add color' (Kosslyn et al., 2000: 1281).

In contrast, a good example of a deceptive suggestion for a hallucination is the one used by Szechtman, Woody, Bowers and Nahmias (1998). After being asked to listen to a taped message and then imagine it, subjects were told the taped message would be played again, but it actually was not. The hallucinators in the study heard the message again, as real, whereas the non-hallucinators, who were otherwise equally highly hypnotizable, heard nothing. In addition, Szechtman and his colleagues were able to show that the neurophysiological signature of hallucinating was clearly unlike that of imagining.

Let us close with a brief, related anecdote. Many years ago, Ken Bowers invited the students in his lab to construct a variety of special, very difficult suggestions for use in a demonstration session with a hypnotic virtuoso. As it turned out, this subject's most impressive responses were, indeed, elicited by what Kallio and Revonsuo would call deceptive suggestions. For example, Ken asked the subject to experience the smell of chocolate, which was actually a bottle of strong-smelling ammonia. When the subject reported that it smelled like chocolate, Ken ventured further into the deceptive by saying, 'Well, of course, but what *kind* of chocolate?' The students were amazed as the subject snorted away vigorously at the bottle of ammonia, eventually reporting his discovery with a big smile: 'A Hershey bar!' (Prior to the demonstration, none of the students had been able to take even a whiff of the bottle without a strong, involuntary flinch.)

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At the close of the demonstration, the students surprised Ken by expressing doubt whether what they had just witnessed could really be the same kind of thing as the tamer phenomena they were studying in their own hypnosis experiments, using non-virtuosos ('highs') preselected in the usual way on standardized hypnosis scales, and using the more typical 'as if' kinds of suggestions. Ken duly reassured them. However, if Kallio and Revonsuo are right, the students' reservations were well founded, and we might have been better advised to study the virtuoso!

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LEVELS OF EXPLANATION AND THE CONCEPT OF A HYPNOTIC STATE

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Abstract

Kallio and Revonsuo's account shows that the state-non-state debate remains a fundamental controversy in the area of hypnosis. However, we argue that sociocognitive criticisms of state/dissociation theory do not arise primarily because these approaches involve different levels of explanation, but because the postulated cognitive and physiological mechanisms involved are different. We also point out the limitations of using hypnotic virtuosos in hypnosis research, and question the utility of using the notion of ASCs as an explanatory construct. We conclude that the issues that divide theoretical opinion on hypnosis operate across at a variety of explanatory levels.

Key words: ASC; hypnotic virtuosos, levels of explanation, sociocognitive theory, state-non-state debate, state/dissociation theory

Kallio and Revonsuo's account is both interesting and thought provoking, and shows that the state/nonstate debate remains a fundamental controversy in the area of hypnosis. However, there are a number problems with their approach.

For example, the central tenet of Kallio and Revonsuo's argument is that state and nonstate accounts differ primarily in terms of their level of explanation; the idea is that nonstate theories reside primarily at the social psychological level, whereas state theories reside at the subpersonal level and include cognitive and neural levels of explanation. However, as Kallio and Revonsuo acknowledge, most nonstate theorists now label themselves sociocognitive theorists; in other words, their approaches integrate work from both the social and cognitive domains, including physiological correlates of such related processes. Thus Spanos and his associates have had much to say about the cognitive mechanisms underlying hypnotic responding and the physiological correlates of hypnotic hallucinations (Spanos, 1991; Perlini, Spanos and Jones, 1996); both Kirsch and Lynn (1999) have used modern theories of working memory in their expectancy approach, and Wagstaff (1998, 2000) has postulated some possible neurological correlates of hypnosis from a sociocognitive perspective. In contrast, before the development of Woody and Bowers' ideas of frontal lobe inhibition, little attempt if any was made by those such as Hilgard, Bowers, Evans, Orne etc. to integrate state/dissociation theory with neurological processes or contemporary theorizing in cognitive psychology (such as models of working memory).

Given this, it could just as well be argued that the nonstate, sociocognitive criticisms of state/dissociation theory do not arise primarily because they involve different levels of explanation, but because the postulated cognitive and physiological mechanisms involved

are different; for instance, Hilgard's (1978, 1984[AQ: not in refs], 1986) theory assumes that there exist dissociated and fairly autonomous control subsystems, whereas Spanos (1991) argues that, at a cognitive level, there are no such autonomous systems, and Kirsch and Lynn (1999) argue that control in hypnosis should best be viewed in terms of the sort of cognitive supervisory attentional system postulated by Shallice and others. In the same way, more recently, both state and nonstate theorists have argued that physiological findings have the potential to inform us about what happens during hypnosis, however, there have been disagreements about how the findings are to be interpreted (Wagstaff, 2000).

Another difficulty concerns Kallio and Revonsuo's view that researchers should concentrate their efforts on hypnotic virtuosos. According to Spanos (1991), it is the virtuoso who is most likely to be exaggerating his or her reports; hence those who have been shown to fake that most difficult of suggestions, the negative hallucination, are also most likely to exhibit virtuoso performance on a range of other suggestions. Martin and Lynn (1996) have also shown that a standard hypnotic susceptibility test does a fairly good job of differentiating reals from simulators; i.e. those who score highest on the scale (virtuosos) most closely mirror simulators. If we assume these studies tell us anything, then, in fact, the medium susceptible subject might be a better candidate for close examination, as they are more likely to be truthful.

Perhaps most problematic, however, is Kallio and Revonsuo's view of what constitutes and ASC and its place in the explanation of hypnotic phenomena. For their approach to make sense, it is essential that they disentangle the idea of a hypnotic ASC as a phenomenon to be explained from that of an ASC as the explanation of hypnotic behaviour. The main thrust of the nonstate criticism of hypnosis as an ASC has not been to deny that hypnotic subjects experience ASCs, but rather that the concept of an ASC unique to hypnosis is unhelpful (and even misleading) in explaining the phenomena we associate with hypnosis (including reports of ASCs themselves). Indeed, as Barber (1969[AQ: not in refs]) noted, there is an inherent circularity in the use of ASC as an explanatory construct: hypnosis involves an ASC which gives rise to changes in subjective experience, as evidenced by these changes in subjective experience. From this perspective, the notion of a hypnotic ASC is not a useful explanatory construct, but rather something that itself must be explained (by, for example, role enactment, expectancy, relaxation, attention focus, etc.). In an attempt to get round this problem, Kallio and Revonsuo argue that the ASC of hypnosis only really makes sense in terms of the notion of an altered background state to consciousness, rather than the phenomenal contents of consciousness. As an example, they point out that the experience of seeing an elephant may be the same during dreaming or under the influence of LSD. But this is not necessarily a distinction between background and phenomenal contents. Just because a man may experience some stimuli as the same whilst he awake and taking LSD does not mean that LSD does not affect the phenomenal contents of his consciousness. Indeed, how could one possibly know whether someone's 'state of consciousness' has been affected by something without an indication of a change in the phenomenal contents of that person's consciousness? The idea that one can infer (say from neurological evidence or reaction times) changes in the background state of consciousness in the absence of any change at all in the contents of phenomenal awareness makes little sense, unless one invents an entirely new definition of the term 'consciousness'. Hence, whilst the notion of a hypnotic ASC as a change in background state or set to experience is interesting, it does not escape the problem of circularity; the proposition simply becomes: hypnosis involves a change in the background state of consciousness which gives rise to changes in

phenomenal experience, as evidenced by changes in phenomenal experience.

However, an alternative way of viewing hypnosis as a 'state', and one which might correspond better to Kallio and Revonsuo's position, might be to argue that hypnosis involves an unusual physiological (or cognitive, or both) state of the brain that gives rise to a range of hypnotic phenomena, including a special ASC as manifested in unique changes in conscious experience. Unfortunately, however, the study of sociocognitive neuroscience is insufficiently developed to address questions such as whether hypnosis involves unique patterns of brain activity, or whether patterns of neural activity support the idea of unique, hypnosis specific, changes in conscious experience. Consider the proposed model experiment on page 146 (Kallio and Revonsuo 2003). The idea is that virtuosos would be given a baseline control condition and an induction, and the two would be compared in terms of their neural activity. This model will not help unless we know precisely how to interpret the neural activity. For example, what precise neural activity accompanies phenomena in suitably motivated people such as lying, trying harder, using one's imagination, being excited, in a state of conflict, focussing attention, acting, being given a nonhypnotic or 'waking' suggestion, and combinations of these? Without this knowledge, how do we know we are tapping the neural mechanisms underlying an ASC as opposed to some other kind of behaviour or experience?

To answer such questions, a neuroscientific theory of hypnosis must start by addressing the methodological issues that have been part and parcel of past work on the state/ nonstate controversy as found, for example, in Barber (1969), Sheehan and Perry (1976), and Spanos and Chaves (1989); e.g. they must employ a range of procedures including simulating, imagination, task motivated and waking nonhypnotic suggestion between subjects controls, instead of the flawed within subjects methodology. For instance, there is a large volume of evidence to show that hypnotic susceptibles behave unusually in control situations when they know they will also be tested 'with hypnosis'. Studies on 'virtuosos' tell us little unless we already know precisely how they behave in different contexts, and how others behave in comparable situations, given comparable instructions with and without 'hypnosis'.

In conclusion, we believe that Kallio and Revonsuo are to be commended for fully grasping the nettle and attempting to define what exactly is meant by an altered state of consciousness in the context of hypnosis. We would argue, however, that as an explanatory construct, the notion of a hypnosis as an ASC is non-starter, and the hypnotic state only makes sense as an explanatory construct if divorced from the phenomena it seeks to explain (such as changes in conscious experience). In addition, whilst we agree that some of the differences between modern theories of hypnosis may reflect different levels of explanation, we also believe that many of the differences go beyond levels of explanation and concern more fundamental views about processes and mechanisms. No researcher with any experience would deny that many hypnotic subjects experience ASCs, but more important are issues concerning the nature and veracity of reports of such experiences, whether they are unique to contexts defined as hypnosis, and whether one needs to posit unique processes and mechanisms to account for them and other phenomena associated with hypnosis; such issues concern a variety of levels of explanation.

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EMPIRICAL RESOLUTION OF THE ALTERED STATE DEBATE

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Abstract

Kallio and Revonsuo (2003) correctly identify the central issue in the altered state debate as being whether trance state is needed to produce hypnotic experiences. Their suggested tests of that hypothesis are insufficient to answer that question. The data required for empirical resolution of the state debate are outlined here. In addition, the problems faced by state and nonstate theories are considered.

Key words: hypnosis, hallucinations, nonstate, post-hypnotic suggestion, state

Empirical resolution of the altered state debate

Kallio and Revonsuo (2003) correctly identify the central issue in the altered state debate: ‘Is there a special hypnotic state (ASC) that serves as a background and gives rise to altered experiences produced by suggestion?’ (p.125), and there is no disagreement about the levels at which hypnotic phenomena need to be described and explained. That which is to be explained resides at the phenomenal level,¹ and a full explanation requires consideration of social-psychological, personal, cognitive, and neural influences and correlates of this explanandum (Kirsch and Lynn, 1995; Kirsch, 2001). State and nonstate theorists agree that hypnotic suggestions can produce altered states (i.e., the subjective experience of amnesia, analgesia, involuntariness, etc.). We disagree about whether these altered experiences depend on the prior induction of a trance state.

Kallio and Revonsuo postulate that true hypnosis is a rare phenomenon experienced only by hypnotic virtuosos who are capable of experiencing hallucinations without voluntary effort. Thus, they reject the altered state hypothesis for most hypnotic phenomena, including most clinical applications of hypnosis, which are by no means limited to hypnotic virtuosos. Their position is that social-cognitive concepts explain most hypnotic experiences (i.e., those which can be experienced by people who are not at the extreme upper end of hypnotic suggestibility), including those responses in which involuntariness is most characteristic (i.e., ideomotor responses, which can be obtained in 80% of the population). In this respect, their theory is very similar to that recently proposed by T. X. Barber (1999). Actually, Kallio and Revonsuo may be overly restrictive in their application of the altered state hypothesis. Hallucinatory responses and other cognitive alterations are not as rare as they suppose. Across three data sets in two different laboratories, 50% of college students passed the post-hypnotic suggestion, 47% experienced a taste hallucination, 42% hallucinated a mosquito, and 25% displayed suggested amnesia (Kirsch, Silva, Comey and Reed, 1995).

Testing the altered state hypothesis

Kallio and Revonsuo propose two ‘ideal’ experimental designs ‘to test the ASC hypothesis’ (pp. 146 and 147). Neither of these alone really tests the altered state hypothesis, but the two of them combined (with some further elaboration) might.²

Assessing neutral hypnosis

In one of Kallio and Revonsuo’s (2003) ideal designs, neutral hypnosis is induced, with no suggestions (not even relaxation suggestions) other than the suggestion to enter the state of hypnosis. Changes in neural activity are assessed and interpreted as indications of a hypnotic trance state. There are two problems with this proposal. First, not all changes in consciousness qualify for the term altered state. According to Tart (1983: 19), an altered state involves ‘major alterations in both the content and pattern of functioning of consciousness. The major pattern connoted by “state” should not be trivialized by using the word “state” to refer to any change in condition.’ In other words, states such as focused attention, enhanced motivation and altered expectations are not altered states of consciousness. If this were all that the altered state hypothesis denoted, there would be no altered state debate. Nonstate proponents certainly believe that hypnotic inductions produce these mundane changes in consciousness and that they have neural correlates (Kirsch and Lynn, 1995).

A second problem with this ‘ideal’ design is that it does not address the critical issue of the causal role of the trance state in producing other suggested subjective experiences. Hypnotic inductions are suggestions to experience a trance state. For that reason, they should produce altered subjective states in many people, and these altered states should have neural substrates (Kirsch and Lynn, 1995). It is not clear that the experience and neural correlates of trance would be the same for all hypnotized subjects or even all virtuosos. Having different preconceptions about trance might lead to different subjective states and therefore to different neural substrates. In any case, the altered state hypothesis is not about the existence of these altered states, but rather about their hypothesized causal role in producing other hypnotic phenomena.

Assessing responses to suggested hallucinations

In Kallio and Revonsuo’s (2003) second ideal design, hypnotized subjects are given post-hypnotic suggestions to experience hallucinations in response to a specified cue. Their prediction is that ‘whenever the subject reports that they are consciously experiencing the hallucination, and increased level of activation, similar to that typically found for this type of phenomenal content, should be found in the appropriate areas’ (p. 147; but see Kirsch and Lynn, 1995, for a similar hypothesis from a nonstate perspective). Studies of this general type have been done (e.g. Kosslyn, Thompson, Costantini-Ferrando, Alpert, and Spiegel et al., 2000) and are very valuable. They substantiate self-reports of the subjective effect of the suggestion, but because they do not assess the causal role of trance, they do not test the altered state hypothesis.

The claim that this experimental design constitutes an ideal test of the altered state hypothesis rests on an unwarranted assumption. The assumption is that hallucinations are not possible without an altered background state of consciousness. Therefore, the presence of a hypnotic trance can be inferred from the verified presence of suggested hallucinations. The problem with this assumption is that it begs the question. The hypothesis that hallucinations and other exceptional phenomena can be elicited without a hypnotic trance state is precisely what nonstate theorists propose, and self-report data are consistent with that hypothesis. Subjects who have not been hypnotized report experi-

encing suggested hallucinations (Weitzenhoffer and Sjöberg, 1961; Barber and Glass, 1962; Hilgard and Tart, 1966; Braffman and Kirsch, 1999). If these subjects are not lying, one should be able to find corresponding increases in activation at the neural level, and these would suggest that a trance state is not a necessary causal factor.

Here is a real example of the pitfall of assuming that a hypnotically suggested effect indicates the effect of a hypnotic trance. Raz, Shapiro, Fan and Posner (2002) reported that a hypnotic suggestion to see words as if they were in a foreign language reduced Stroop interference in highly suggestible subjects. Because Stroop inhibition is widely regarded as automatic, one might interpret this as a major shift in information processing and hence as an altered state (although Raz et al., 2002 to their credit, did not make this claim). We have replicated these results with un hypnotized as well as hypnotized subjects (Pollard, Raz and Kirsch, 2003). Our data confirm that suggestion can modulate the Stroop effect in highly suggestible subjects, but they also indicate that this does not require the induction of hypnosis. Altered Stroop inhibition may be an altered state, but it is not necessarily a hypnotic state and does not reveal the presence of a trance.

Combining ideal experiments

Although neither 'ideal' experiment alone tests the altered state hypothesis, studies in which they are combined might well do so. One would have to induce hypnosis in exceptionally suggestible subjects, find neural changes indicating 'major alterations in both the content and pattern of functioning of consciousness' (Tart, 1983: 19), and then find subjective and neural changes specific to subsequently administered suggestions. If the altered state hypothesis is correct, these latter neural changes should not be possible without the preceding major alterations in consciousness. Note that this allows assessment of the 'slipping into hypnosis' hypothesis. Subjects who spontaneously slip into hypnosis without trance induction should show the same neural alterations as those in whom trance has been formally induced without any other suggestions being administered.

There are important pitfalls to avoid in studies like this. In particular, the holdback effect (Zamansky, Scharf and Brightbill, 1964) needs to be taken seriously. That means that at least some subjects have to be screened without a hypnotic induction (see Braffman and Kirsch, 1999, for an example of how to do this) and without any mention of hypnosis. Those scoring high in suggestibility and reporting the presence of the hypnotic phenomenon that will be assessed (e.g. the suggested hallucination) can be given the hallucination suggestion again, still without mention or induction of hypnosis, and the neurophysiological correlates of their reports can be compared to those obtained following an induction (either subsequently in the same subjects or in a separate group of subjects).

A second pitfall to avoid is changing the wording of the suggestion, in addition to varying whether hypnosis is induced. For example, one should not tell hypnotized subjects that they will see something, but tell nonhypnotized subjects to imagine something (e.g. Kosslyn et al., 2000). This confounds suggestion with induction and precludes any conclusions about the altered state hypothesis.

Problems that nonstate theories face

In delineating the problems that nonstate theories face, Kallio and Revonsuo focus almost exclusively on the inability of response expectancies to fully account for the hallucinations elicited by hypnotic (and nonhypnotic) suggestion. I too have noted the

incompleteness of expectancy as an explanation of hypnotic phenomena (e.g. Kirsch, 1991). Nevertheless, Kallio and Revonsuo underestimate the potential impact of response expectancy on experience. This may be related to their failure to clearly differentiate between stimulus expectancies and response expectancies. Stimulus expectancies are not 'alterations in perception in association with ambiguous stimuli' (Kallio and Revonsuo 2003: 131). Stimulus expectancies are beliefs about events that are external to the individual (e.g. that the car keys are on the kitchen table; Kirsch, 1985, 1991, 1999). I have not hypothesized these expectancies to be involved in responding to suggestion. In contrast, 'response expectancies are anticipations of one's own automatic reactions...Unlike stimulus expectancies, response expectancies are directly self-confirming' (Kirsch, 1999: 4). These are the expectancies that are involved in the production of suggested responses.

Hypnotic response expectancies are expectancies of experiencing suggested phenomena in the absence of the stimulus events that might also cause these experiences. Highly suggestible people expect their arms to feel light even though they know there are no balloons pulling the arms up; they expect their arms to be drawn together 'as if' there were a powerful force acting on them; they expect to see a kitten on their lap even though they know it is not really there (Comey and Kirsch, 1999). They know that the external situation has not changed, even in those instances in which the suggestion wording implies that it has, because of their cultural understanding of hypnosis. Hypnosis is a situation in which people hear flies that are not there; it is not a situation in which hypnotists subject them to real flies.

The role of response expectancies in hypnosis is similar to their role in generating placebo effects. A person who believes that medication is effective expects to experience medication-engendered responses that vary as a function of the type of medication, just as the hypnotized subject expects to experience whatever the hypnotist suggests. The expectation becomes specific only when the information about the supposed drug is provided or the specific hypnotic suggestion is given. Thus, the expectancy generated by a topical anaesthetic will be less pain in a specific location of the body, which is also the expectancy generated by a hypnotic suggestion for localized analgesia.

Problems that state theories face

Kallio and Revonsuo (2003) delineated some of the problems faced by state theories. In this section, I add two additional considerations to that list. These are the problems of accounting for post-hypnotic suggestion and for the production of the trance state.

Post-hypnotic suggestions are suggestions for the production of a hypnotic response (e.g. a hallucination) that will be triggered by a specified cue (e.g. a handclap) *after* trance has been terminated. This is often tested immediately after trance has been terminated, but sometimes a considerable amount of time (days or weeks) might intervene between the suggestion and its test. If a hypnotic trance is required for the response to a post-hypnotic suggestion to occur, then people must be walking around in trance despite the suggestion to come out of trance. If hypnotic trance is not required, it is a suggestion that could be explained *only* by a nonstate theory. That is, if post-hypnotic, rather than hypnotic, suggestions are used to induce hallucinations, as suggested by Kallio and Revonsuo (2003) in one of their ideal experiments, and if the performance of the post-hypnotic suggestion takes place outside of the trance state, then trance is not directly involved in the production of the hallucination. The only alternative I can think of is to suppose that the cue produces an instant but temporary and spontaneously ending trance

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state, thereby allowing the production of the post-hypnotic response. This is a rather extreme variant of the slip-into-hypnosis hypothesis, and it is comforting to note that no one has (as yet) proposed it.

This brings me to the second additional problem facing state theories. How is a hypnotic trance produced? Since trance is what we are trying to explain, it cannot be part of the explanation. This is not a problem for nonstate theories, even if they acknowledge the reality of suggested trance states (just as they acknowledge the reality of other suggested phenomena). Whatever causal principals they use to explain the production of suggested experiences such as subjective automaticity, cognitive and motor inhibitions, and hallucinations, they can also use to explain the production of trance. Expectancy is a case in point. It is well known that there are no specific procedures that are required for a hypnotic induction. Instead all that is needed is the subject's belief that the procedure is an adequate induction procedure (Sheehan and Perry, 1976). This lack of specific content seems enough to suggest that hypnotic inductions are placebos and that their effects can be considered expectancy effects.

Notes

- 1 The role of subjective automaticity in hypnotic responding needs some clarification. It is true that involuntariness has generally been considered a central characteristic of hypnotic responding, but this pertains more to ideomotor responses (which seem to be excluded from Kallio and Revonsuo's notion of 'true' hypnosis) and much less central to the more difficult cognitive suggestions, in which no overt behaviour is requested. Consider, for example, hypnotic pain control. If a patient reports a hypnotically-induced complete absence of pain during surgery, would you exclude this from being a truly hypnotic response if he or she reported having blocked the pain intentionally? Even with ideomotor suggestions, the requisite sense of automaticity pertains only to the overt behaviour and not to any intentional cognitive effort that is used to induce it. In fact, requests for intentional goal-directed fantasies are often included in ideomotor suggestions. On the Waterloo-Stanford scale (Bowers, 1998), for example, subjects are instructed to 'imagine that you are holding something heavy in your hand,' 'think about a force acting on your hands to pull them together,' and 'think of your arm becoming stiffer and stiffer.' Similar intentional generation of the response is explicitly requested in some of the cognitive suggestions as well (i.e., 'imagine that you have something sweet-tasting in your mouth, like a little sugar' and 'think about a pleasant time when you were in the fifth grade of school').
- 2 Note that the use of hypnotic virtuosos in testing the altered state hypothesis is not controversial. However, this comprises 5–15% of the population, thus enabling group studies, which are certainly preferable to the n=1 case studies advocated by Kallio and Revonsuo.

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DETECTING HYPNOTICALLY ALTERED STATES OF CONSCIOUSNESS

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Abstract

The non-veridical experiences associated with hypnosis, which are clearly at variance with reality, are a clue that the hypnotized person has ceased to test the validity of experiences. Recent brain-mapping studies implicate the anterior cingulate gyrus as a key region in the establishment of hypnotic misperceptions. Significantly, when that region of brain is damaged patients experience difficulty in distinguishing the imaginary from the real. Taken together, these observations support the claim that hypnosis entails an abandonment of reality testing. It is argued that an altered state of consciousness is an inevitable consequence of ceasing to test reality. Possible ways of researching this altered state are discussed.

Key words: altered state, consciousness, criterion shifting, reality testing, signal detection, time distortion

Introduction

For many years, even in the field of hypnosis, it has been recognized that knowledge is advanced by using a combination of theorizing on existing data, and experimentation to generate new observations. It has not always been so. Certainly as late as the seventeenth century, Descartes was of the opinion that ultimate truths could be discovered by reasoning alone. Of course, reasoning was not to be a profligate process, and three hundred years earlier William of Ockham had laid down guidelines that have come to be known as Occam's Razor. This was the principle of maximum parsimony; we should select the simplest theory that also explains the largest number of facts, cutting away all other accounts. Unfortunately, the principle by itself is not an infallible guide to the truth. Kallio and Revonsuo (2003: 126) point out that nonstate theorists appeal to Occam, as they cut away concepts such as 'altered state of consciousness', but what could be a more parsimonious explanation of the range of hypnotic phenomena than the simple theory that hypnotized people have had their consciousness altered? The nonstate rejoinder is that psychologists should be trying to produce theories of *all* human behaviour; an altered state may be a convenient explanation of hypnosis, but in the wider scheme of things it is unsettling suddenly to introduce the concept, just for this specific phenomenon. It is a question of levels: was Occam thinking of reaping whole fields or merely shaving chins? Kallio and Revonsuo have done well to highlight the hierarchical structure of explanations; it is a hierarchy that not only shows the different levels at which a phenomenon may be seen to reside, but also suggests a map of where to wield the Razor.

Turning from the process of reasoning to the activity of experimentation, history again shows that it is difficult to do well and apply incisively. It took a long time for research on combustion to extinguish the old theory of phlogiston; more recently, it was many years before observations supporting the 'Big Bang' theory finally exploded the 'Steady State' concept of the Universe. Perhaps more appositely, Faraday is known to have stated that research should advance by trying to falsify rather than to support a theory. However, we know from his detailed notes that, all too human, he proceeded to ignore his experiments that 'went wrong', and to persist in trying to prove his ideas right. Fortunately, although he did not practise what he preached, he was a man of genius and successfully developed the laws of electromagnetism. Perhaps there is a parallel here with much of hypnosis research. One thinks, for example, of the late Nick Spanos, so many of whose copious and scrupulous studies were devoted to re-proving that people in hypnosis did only what they thought they should do (e.g. Spanos, 1986, 1991). We all tend to devise studies intended to support our theoretical stances, but we cannot all be as fortunate as Faraday. It is useful that Kallio and Revonsuo have set out an attempt to devise what might be termed a bias-free experiment.

There is thus much to recommend the very substantial paper from Kallio and Revonsuo, presenting as it does an extensive review of thinking in hypnosis, and a reasoned suggestion for research. In the detail there is a good deal with which one would agree, although inevitably there are points to expand and some that one would contradict. I will now consider some of these.

Abandoning the social approach

I have long been of the opinion (Naish, 1986a) that the area of hypnotic responding in need of research was not its social setting, nor even the behaviour itself, but the non-veridical perceptions that underpin the behaviour. It was gratifying, therefore, to see that Kallio and Revonsuo state that subjective experience is the core of what needs to be explained for any theory of hypnosis (2003: 138, 139). Moreover, they go on to say (p. 140) that an explanation must always be based in a lower level of the hierarchy than the phenomenon explained. In other words, experience must be explained by cognition. Since the social psychological level is at the top of the hierarchy, this rules it out as an appropriate vehicle for explaining anything!

The first item under the heading 'Experimental research today' in the Kallio and Revonsuo paper, is 'Sociocognitive research'. This term is accurate, in the sense that it is used by those carrying out the research, but to me the label lacks pedigree. It appears to be a hang-over from the days when it seemed that hypnosis could best be described in straightforward social psychological terms, such as compliance (e.g. Wagstaff, 1981). Gradually it was acknowledged that participants (at least sometimes) actually experienced the phenomena they claimed: a cognitive element was required. Like a priesthood seeing its religious beliefs eroded by science, the 'Socialites' embraced the new cognition and claimed it for themselves, ensuring that their name endured. Of course one accepts that there is a social element in the procedures and experiences of hypnosis, but so there is in the 'hardest' of psychological science. We are aware of the need to employ criterion-free testing in psychophysics, knowing that instructions such as 'Make sure you don't say you see it if you are not sure', or alternatively 'Please try not to miss any', will enormously influence an experimental outcome. Nevertheless, no one has proposed a sociocognitive theory of depth perception! Even more socially influenced is the process of remembering, yet where are the sociocognitive accounts of memory? Surely, it is time

to drop the 'socio', leaving it at the top of the hierarchy, as suggested by Kallio and Revonsuo, and to get on with elucidating the cognitions.

Reasoning about existing data

At a number of points in their exposition, Kallio and Revonsuo either fail to explore existing findings, or imply that there is a complete absence of data to address key issues. In their section on the effects of expectancy (p. 131) they mention the possibility of false judgements being made in conditions with a poor signal to noise ratio, and consider the likelihood that something of the sort might occur during hypnosis. This is exactly the suggestion I have made, when taking a signal-detection approach to hypnosis (Naish, 1985). However, Kallio and Revonsuo reject a purely expectation-based explanation, on the grounds that we are not normally the victims of expectation-induced perceptions that fly in the face of reality. Nevertheless, it is the case that hypnosis can induce large and sustained criterion shifts (Naish, 1986b), so, rather than reject the hypothesis that expectation leads to experience, attention should be directed to discovering whether (and how) reality testing can temporarily be abandoned (Naish, 2002).

If it were indeed the case that hypnotic phenomena arise through the effects of expectation, freed from the constraints of reality testing, then inevitably the testing for hypnosis would become very difficult. This is because it raises the well-known circularity problem, expressed well by Kallio and Revonsuo (p. 128). In the current context, the question and answer sequence would be: Why does s/he perceive falsely? Because s/he has stopped reality testing. How do you know that reality testing has stopped? Because s/he is having false perceptions. An independent indicator of abandoned-reality testing (ART) is required.

A further problem for the ART account is much the same as for the altered state of consciousness (ASC) account, as expressed by Kallio and Revonsuo (p. 142): although ART has the potential to explain a wide range of effects, those that actually occur will be dependent upon other factors, such as expectation. Consequently, there may be no repeatable behavioural signature for the ART condition. Kallio and Revonsuo (p. 135) go so far as to state that there is *no* unambiguous behavioural criterion for hypnosis. This may not be true, since the distortion of time judgements appears to be a very good candidate (Naish, 2001). True, St. Jean (1988) claimed that the distortion's dependence upon hypnosis had not been established, but Mozenter and Kurtz (1992) showed that simulators were unable to reproduce the effect, an observation generally taken to imply that hypnosis is causal.

Clearly, ART and ASC have much in common, at least in the problems the two accounts face in demonstrating that they are anything more than hypothetical constructs. With these commonalities, it is reasonable to consider whether ART would actually constitute an example of ASC. If consciousness is conceived as being, at least in part, the experiencing of one's environment, then simply not to test the reality of the experience would not immediately alter the state of consciousness. Only when imagination and expectation supplanted the 'real' (but no longer tested) percepts could consciousness be claimed literally to have altered. Nevertheless, with one condition following almost inevitably from the other, it seems a convenient shorthand to treat ART (if it exists) as an example of the AST concept that Kallio and Revonsuo have set out to test.

Completing the ASC picture

Thus far, we have only disjointed elements of a complete description of an altered state of

consciousness. It has been suggested that time distortion might be a dependable behavioural indicator of hypnosis, but no connection has been demonstrated with ASC. Evidence has been cited that hypnosis can produce criterion shifting, (in signal detection terms), but enduring experiential changes could not follow, unless reality testing were abandoned. At this stage the ART explanation is little more than conjecture.

Kallio and Revonsuo provide a clue as to how these elements should be assembled; they mention (p. 144) that hypnosis produces a state similar to that of frontal lobe patients. A startlingly close similarity is observed when the region of damage is the anterior cingulate (Whitty and Lewin, 1957), since patients with this condition experience great difficulty in distinguishing imagined from actual events; they appear to be unable to perform reality testing. Moreover, there is a growing body of MRI and PET data to support the claim that the anterior cingulate gyrus is implicated in the generation of believed-in hypnotic experiences (e.g. Crawford, Horton, Hirsch, Harrington, Plantec, Vendemia, Shamro, McClain-Furmanski and Downs, 1998; Szechtman, Woody, Bowers and Nahmias, 1998). Thus, ART is not groundless conjecture; there is good reason to suppose that hypnosis impacts upon a region of brain known to be involved in the reality testing process.

There remains the issue of hypnotic time distortion, and whether that could plausibly be a behavioural indicator that an ASC is in place. Kallio and Revonsuo (p. 112) point out that it is hard to find a clear definition of normal consciousness (let alone one that has altered). Gray (1995) has proposed a plausible account of consciousness, suggesting that its contents are continually updated in a sample-test-predict cycle, using neural circuitry that includes the cingulate. I have shown (Naish, 2003) that there is evidence for Gray's cycle-rate and the 'inner clock' rate to be one and the same. If true, changes to the sampling of reality, brought about by alterations in cingulate activity, could very well impact upon time judgements.

Future research

Kallio and Revonsuo suggest that research should concentrate upon hypnotic virtuosos. The implication is that only this small fraction of the population actually experiences an ASC, and presumably this implies that it is an all-or-nothing state of affairs. Admittedly, something has either altered or it has not, but that is a different issue from the extent or range of the alteration. Time distortion is experienced by a large proportion of those subjected to hypnotic procedures, suggesting that a large proportion may be experiencing some level of ASC. Of course, to research the more extreme phenomena, such as hallucinations, it is necessary to use participants who experience them, and they are likely to be from the 'high' end of the susceptibility spectrum, but to abandon studies with any but the very best responders would seem unnecessarily restricting.

Moving from who to what should be the subject of research, Kallio and Revonsuo offer a rather unremarkable list, preceding it with such cautions as avoiding the possibility of compliance and faking (pp. 146, 147). There is nothing very novel here, but they finally become more specific (p. 148) by suggesting perceptual effects, such as 'pop-out'. This is an intriguing idea, and perhaps worth attempting, but a failure to find a virtuoso who could perform the feat would not constitute a disproof of ASC. In fact the reasoning here appears to be muddled. The authors are correct in stating that the effect is pre-attentive, and hence often assumed to occur at an early stage of processing (e.g. Treisman and Gelade, 1980), but they themselves (p. 143) suggest that hypnosis impacts upon sensory representations. The whole thrust of Treisman's work (e.g.

Treisman, 1998) is that these representations require focused attention. To take the example used by Kallio and Revonsuo, if there were truly red 5s in a display, that fact would not be known until the outputs from the independent colour and shape analyses were combined via attentive processes; pop-out cannot occur for items that require feature combination. If the 5s were the only characters in the display that were red, then redness would pop out, but the fact that the vehicle for the colour was a particular character would not be known without employing attention. For hypnotic suggestions to 'paint' the 5s red, those particular digits would have to be located, and that would require attention, since they could not pop out until they had received their paint.

The whole idea behind a search for outlandish perceptual effects is based upon the desire to produce phenomena that cannot be faked. A far simpler approach is to use simulators, and an example of a misperception that is not simulated has already been identified: time distortion. If ASC is to be defined as a situation in which perceptions are distorted, then the well-known timing effects have already settled the state/nonstate debate. However, although time distortion has the merit of being accommodated by the ASC account developed in the previous section, perceptual distortions in general are not conclusive evidence of an ASC. Distortions of all sorts are commonplace, ranging from size constancy to the fact that people sometimes select clothes to distort others' perceptions. Even in the latter case, it seems unreasonable to claim an ASC in the observer!

There is no doubt still much to learn through exploring hypnosis-induced perceptual distortions, but the ASC explanation for them would be best tested by employing a non-hypnotic means of obtaining the same effects. There must be agreement that the distorted perceptions of those suffering from brain damage, or under the influence of drugs, represent altered states of consciousness. Suppose it were possible, outside the hypnosis context, to act upon the brain in such a way as to generate typical hypnotic effects. By extension, this alteration would be another example of an ASC. Moreover, if the neural changes required proved very similar to those observed in the hypnosis setting, then it would be reasonable to say that hypnosis too altered the state of consciousness.

As outlined above, there is good reason to believe that hypnotic procedures act by modifying activity in the cingulate region. The claim that this is the vehicle for producing non-veridical perceptions could be tested by determining whether other means of disrupting the cingulate would produce similar experiences. It would be of particular interest to employ the technique with those normally incapable of experiencing hypnotic phenomena. In effect, one is suggesting a bringing up to date of the Whitty and Lewin (1957) observations, by carrying out a series of controlled cingulectomies. To carry out the lesioning in a literal sense is of course not possible, but trans-cranial magnetic stimulation (TMS) does provide a route for this type of study. TMS has been likened to a temporary lesion; it disrupts neural activity in a defined region of brain tissue by means of an intense magnetic field. It is proving an effective tool in localizing brain function. A simple first question to investigate is whether a series of magnetic pulses directed at the anterior cingulate gyrus would lead a participant to misjudge the passage of time. That participant would not even have to be a hypnotic virtuoso!

Conclusions

The Kallio and Revonsuo (2003) paper is valuable for its balanced evaluation of a complex debate. The clarification of the issues, by setting them within a hierarchical

explanatory structure, is particularly helpful. However, the paper is less strong as it moves into more practical applications of the theorizing, and the examples of possible experiments are not convincing.

Building upon the Kallio and Revonsuo material, the current paper has shown that there is already strong evidence to support the claim that hypnosis induces an altered state of consciousness; this change, it is proposed, is closely linked to changes in the reality-monitoring process. In line with the position adopted by Kallio and Revonsuo (that phenomena are best explained by investigating the next lower layer of the hierarchy), it is proposed that the changes in perception are investigated at the neural level. The transcranial magnetic stimulation technique may provide an elegant solution to the problem of testing neural processes.

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MULTILEVELING THE PLAYING FIELD: ALTERING OUR STATE OF CONSCIOUSNESS TO UNDERSTAND HYPNOSIS

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Abstract

The state/nonstate controversy involving hypnosis is examined through six principles of explanation. The importance of hypnotizability is emphasized as a moderating factor in studying hypnotic phenomena. Framing discussions of social, intrapsychic and neurophysiological influences on hypnosis in terms of relative contributions to the explanation of variance rather than winner-take-all approaches is recommended.

Key words: hypnosis, hypnotizability, neurophysiological, social influence

Kallio and Revonsuo are to be congratulated for a very thorough and thoughtful review of the hypnosis literature and for their suggestions of methods to examine the state/nonstate controversy. Their idea of studying special cases – hypnotic virtuosos – is a useful one that we and others have employed. It serves to underscore the fact that hypnotizability is a crucial dimension in understanding the phenomenon – hence the need to select for the highest on this dimension.

There are some fundamental problems in the rather enervating state/nonstate, social psychological, and altered state of consciousness debate. I propose some basic principles.

1) *When you have a hammer, everything starts to look like a nail.* Like patients with a dissociative disorder, scientists tend to see the world in terms of their particular explanatory framework. We should be asking not whether hypnosis is merely a social psychological phenomenon, but rather how much of the variance in hypnotic experience can be accounted for by social psychological factors. Likewise, since we use our brains to do everything (most of us do, anyway), there will be neural correlates of much human activity, including, as the authors have documented, hypnosis. Again the question is one of unique variance. When hypnotized and highly hypnotizable people can change brain function in ways that typically occur only with alterations in actual perception, it looks as though the hypnotic state is something special. For example, hypnotic visual hallucinations are opaque, and are associated with changes in event-related potential amplitude and blood flow in specific visual processing regions (Spiegel, Cutcomb et al., 1985[AQ: **all authors needed here and in refs**]; Spiegel, Bierre et al., 1989[AQ: **all authors needed here and in refs**]; De Pascalis, 1994; De Pascalis and Carboni, 1997; Barabasz, Barabasz et al., 1999[AQ: **ditto**]; Kosslyn, Thompson et al., 2000[AQ: **ditto**]; Jensen, Barabasz et al., 2001[AQ: **Ditto**]; Bryant and Mallard, 2003; Spiegel, 2003). Social psychological theories postulate that such alterations are efforts to comply but not experienced as ‘real’ (Spanos, 1986). But to hypnotized individuals with the requisite hypnotic

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capacity, there are real – believing is seeing (Spiegel, 1987). Kallio and Revonsuo have proposed other clever experiments to further test the idea that hypnotic performance is non-ordinary (Spiegel, 1998).

2) *Categories are artificial*. We simplify the world by creating them, but most real phenomena are continuous. It is always possible to blur real differences by comparing two points on a 10-point scale, or to exaggerate differences by comparing extremes. Hypnotic virtuosos are interesting in a variety of ways, but they represent simply a more extreme form of what many (though not all) people can do.

3) *Hypnosis is not confined to context*. Hypnotic phenomena occur with or without a formal hypnotic induction. The absorption literature makes it clear that hypnotic-like experiences occur among hypnotizable individuals, whether or not they have ever been exposed to a dangling watch or a social psychologist. The Wagstaff argument that you could not slip into a hypnotic state without being aware of it is tautological and wrong. As many have noted (Spiegel, 1988) (Spiegel, 1990) (Hilgard, 1965) (Hilgard, 1986), the very focus of the hypnotic state in the central percept may hamper or eliminate the type of superordinate awareness that is required to make an accurate observation about being in that state, as in states of ‘flow’ (Csikszentmihalyi, 1991).

4) *Social influence theory is a social influence*. The pure social psychological ‘explanation’ for hypnosis is fundamentally as arid as pure behaviourism, which similarly, patronizingly, and inaccurately referred to the brain as a ‘black box’. It is one thing to make a set of assumptions, as mathematicians do, and see what you can learn from sticking to them. It is another to treat the assumptions as facts. Hypnosis occurs outside of social contexts as well as in them, for example in self-hypnosis and absorption, among people capable of it. Furthermore, most social psychological explanations ignore hypnotizability as an intervening variable.

5) *Is consciousness a social construct?* We are, of course, social creatures. We have a state of waking alertness in which we function most of the day. We could call it a non-altered state of consciousness. Does it exist only because of social suggestion, i.e. we all have learned to expect to be awake at times? Is it merely a result of social influence or expectation?

6) *Occam’s razor can bleed the life out of phenomena*. Multilevel explanations are an absolute necessity in understanding human mind/brain/body phenomena because we are both neurally-based and social creatures who experience the world in mental phenomenal terms. To choose one of these domains as the complete explanatory context is to be by definition wrong.

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IS HYPNOSIS AN ALTERED STATE OF CONSCIOUSNESS *OR WHAT?*

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Abstract

The debate over whether hypnosis is an altered state of consciousness is a distraction from the real business of studying the phenomena that occur in the context of hypnotic suggestion. These phenomena can be profitably studied at several levels of analysis: the psychological, the sociocultural, and the neurobiological. A comprehensive understanding of hypnosis must emphasize that the phenomena of hypnosis reflect both alterations in consciousness and social interactions.

Key words: consciousness, hypnosis, nonsate, state

Kallio and Revensuo (2003) are correct to note that a debate over whether hypnosis is an altered state of consciousness has been raging for decades, but they are incorrect that the debate is really over ‘the level of description at which the phenomenon “hypnosis” should be conceptualized’ (p.111; see also p.138). If the debate were merely about levels of analysis, there would not be a debate, because levels of analysis are optional and do not contradict each other. Cognitive psychologists can describe memories as bundles of features, neuroscientists can describe them as reverberating neural networks, and social psychologists can describe them as beliefs shaped by conformity and persuasion, all without contradicting each other or engendering any debate at all. So what *is* the problem?

It cannot be that the term ‘consciousness’ is poorly defined. Consciousness has two principal aspects: *monitoring* ourselves and our environment, so that objects, events, and our internal mental states are accurately represented in phenomenal awareness; and *controlling* ourselves and the environment, through the voluntary initiation and termination of thought and action (Kihlstrom, 1984). From this point of view, the alterations in perception and memory exemplified by hypnotic analgesia or deafness, positive and negative hallucinations, posthypnotic amnesia, and posthypnotic suggestion constitute disruptions in conscious awareness: the subject appears to be unaware of percepts, memories, and thoughts that nevertheless continue to influence ongoing experience, thought, and action outside of conscious awareness (Kihlstrom, 1998). This disruption in awareness, in turn, gives rise to the experience of involuntariness that frequently accompanies suggested hypnotic experiences – an apparent loss of control over cognition and behaviour (Kihlstrom, 1992). It would seem that the only way to deny that the phenomena of hypnosis reflect alterations in consciousness would be to deny that the phenomena themselves are genuine – to assert, for example, that hypnotic subjects really do feel pain, and really do remember, despite what they say after they have been given suggestions for analgesia and amnesia.

Perhaps, though, the problem lies in the way that *altered state* is defined – which is, admittedly, a little fuzzy (Ludwig, 1966). If we believe that every altered state should be associated with a unique physiological signature, much as sleep is associated with the absence of alpha activity in the EEG and dreaming with the occurrence of rapid eye movements (REM), then the lack of a physiological indicator for hypnosis may be taken as evidence that hypnosis is not an altered state of consciousness after all. But of course, this puts the cart before the horse. Physiological indices are validated against self-reports: Aserinsky and Kleitman (Aserinsky and Kleitman, 1953) had to wake their subjects up during periods of REM and ask them if they were dreaming. As such, physiological correlates have no privileged status over introspective self-reports: Aserinsky and Kleitman were in no position to contradict subjects who said that they were not dreaming. It is nice when our altered states have distinct physiological correlates, but our present knowledge of mind-body relations is simply not sufficient to make such correlates a necessary part of the definition. After all, cognitive neuroscience has made very little progress in the search for the neural correlates of ordinary waking consciousness (Metzinger, 2000). How far in the future do the neural correlates of altered states of consciousness, like hypnosis, await?

In the final analysis, it may be best to treat hypnosis and other altered states of consciousness as *natural concepts*, represented by a prototype or one or more exemplars, each consisting of features that are only probabilistically associated with category membership, with no clear boundaries between one altered state and another, or between altered and normal consciousness (Hilgard, 1969; Kihlstrom, 1984). And because we cannot have direct knowledge of other minds, altered states of consciousness must also remain *hypothetical constructs*, inferred from a network of relationships among variables that are directly observable (Campbell and Fiske, 1959; Garner, Hake, and Eriksen, 1956; Stoyva and Kamiya, 1968), much in the manner of a psychiatric diagnosis (Orne, 1977). From this point of view the diagnosis of an altered state of consciousness can be made with confidence to the extent that there is convergence among four kinds of variables: an induction procedure, alterations in subjective experience, associated changes in overt behaviour, physiological correlates, and an induction procedure. Let me address each of these aspects briefly in turn.

Operationally, an altered state of consciousness can be defined, in part, by the means employed to induce it – or, alternatively, as the output resulting from a particular input. Barber (Barber, 1969) employed such an input-output definition as the sole index of hypnosis, largely ignoring individual differences in hypnotizability. Operational definitions of this sort are a residue of functional behaviourism in psychology, and we should get over them. At the very least, hypnosis would seem to require *both* a hypnotic induction *and* a hypnotizable individual to receive and act upon it. But in the case of very highly hypnotizable subjects, even the induction procedure may be unnecessary.

Introspective self-reports of changes in subjective experience would seem to be central to the definition of any altered state of consciousness. The domain of hypnosis is defined by suggested changes in perception, memory, and the voluntary control of behaviour – analgesia, amnesia, the experience of involuntariness, and the like (Hilgard, 1973). If the hypnotist gives a suggestion – for example, that there is an object in the subject's outstretched hand, getting heavier and heavier – and the subject experiences nothing of the sort, it is hard to say that he or she has been hypnotized.

Of course, a reliance on self-reports has always made psychologists nervous, so another residue of radical behaviourism is a focus on overt behaviour. If a subject hallucinates an object in his outstretched hand, and feels it grow heavier and heavier, eventually

his arm ought to drop down to his side. Individual differences in hypnotizability are measured in terms of the subject's publicly observable, overt, behavioural response to suggestions. But in this instance, the overt behaviour is, to borrow a phrase from the Anglican Book of Common Prayer, an outward and visible sign of an inward and spiritual grace. Overt behaviour is a consequence of the subject's altered subjective experience, and is of no interest in the absence of corresponding subjective experience. For this reason, requests for 'honesty reports' (Bowers, 1967; Spanos and Barber, 1968) or other appropriate post-experimental interviews (Orne, 1971; Sheehan and McConkey, 1982) can help clarify subjects' overt behaviour, and serve as correctives for simple behavioural compliance.

Because both self-reports and overt behaviours are under voluntary control, and thus subject to distortion by social-influence processes, hypnosis researchers have long been interested in psychophysiological indices of response. Over the years, a number of such indices have been offered, including skin conductance and alpha activity, but these have usually proved to be artifacts of relaxation, and not intrinsic to hypnosis. In retrospect, it was probably a mistake to expect that there would be any physiological correlates of hypnosis in general, following an induction procedure but in the absence of any specific suggestions (Maquet et al., 1999), because subjects can have a wide variety of experiences while they are hypnotized. Progress on this issue is more likely to occur when investigators focus on the physiological correlates of specific hypnotic suggestions – as in brain imaging work that shows specific changes in brain activity corresponding to hypnotic visual hallucinations (Kosslyn, Thompson, Costantini-Ferrando, Alpert, and Spiegel, 2000) or analgesia (Rainville, Hofbauer, Bushnell, Duncan, and Price, 2002).

In fact, this may be a good strategy for traditional, performance-based investigations of hypnosis as well. Kallio and Revensuo (Kallio and Revensuo, 2003) argue that the 'state' view is troubled by the fact that hypnotic induction procedures do not appear necessary to produce hypnotic effects, but this is a problem only if an induction procedure is construed as a defining feature of an altered state. An even more troubling fact is that every phenomenon produced in hypnosis can also be produced posthypnotically – that is, after the subject has been brought out of hypnosis. This only reinforces the point that alterations in consciousness are not *caused* by a state of hypnosis. Studying hypnosis, as an ostensible state, is likely to be far less productive than studying specific hypnotic phenomena, such as analgesia, amnesia, posthypnotic suggestion, or hypnotic hallucinations (positive and negative).

Whatever the focus of study, we would do well to bear in mind the multifaceted nature of hypnosis itself. As White (1941: 502) noted at the dawn of the modern era of hypnosis research, 'The theory of hypnotism will never prosper until, outgrowing the dialectic dichotomy of "striving" and "state", it considers the possibility of interaction'. In White's view, hypnosis was an 'altered state of the person' that takes place in a context of 'meaningful, goal-directed striving' (p. 504). Orne (1959), who was White's protege as both an undergraduate and a graduate student at Harvard, famously tried to distinguish between artifact and essence of hypnosis, but a careful reading of his work makes it clear that the demand characteristics that *surround* hypnosis are as important as any 'trance logic' that might arise *in* hypnosis. Similarly, at the beginning of what might be called the 'golden age' of hypnosis research, Sutcliffe rejected both the credulous and sceptical views of hypnosis (another version of the state-nonstate dichotomy), and offered a third view: that hypnosis involves a quasi-delusional alteration in self-awareness constructed out of the interaction between the hypnotist's suggestions, and the subject's interpretation of those suggestions (Sutcliffe, 1960, 1961).

Hypnosis entails changes in conscious perception, memory, and behaviour, to be sure, but these changes also occur following specific suggestions made by the hypnotist, as they are interpreted by the subject. These changes in conscious mental life, occurring in the context of suggestion, define the domain of hypnosis; hypnosis as a state is characterized by these changes, but it does not *cause* them to occur (Hilgard, 1969, 1973). The ‘third way’ in hypnosis research construes hypnosis simultaneously as both a state of (sometimes) profound cognitive change, involving basic mechanisms of perception, memory, and thought, *and* as a social interaction, in which hypnotist and subject come together for a specific purpose within a wider sociocultural context. A truly adequate, comprehensive theory of hypnosis will seek understanding in both cognitive and interpersonal terms.

We do not yet have such a theory. Until we produce one, individual investigators will naturally emphasize one aspect over the other in their work, whether altered consciousness or social context. The inter-individual competition that is part and parcel of science as a social enterprise often leads investigators to write as if alterations in consciousness and social influence were mutually exclusive processes. Taken together with the null-hypothesis statistical tests that remain part and parcel of the experimental method, and a propensity for making strong rather than weak inferences from experimental data, investigators will often present evidence for one process as evidence against the other. But if there is one reason why hypnosis has fascinated successive generations of investigators, since the very dawn of psychology as a science, it is that hypnosis exemplifies the marvellous complexity of human experience, thought, and action. In hypnosis and elsewhere, comprehensive understanding will require a creative synthesis in the spirit of discovery, rather than the spirit of proof – a creative synthesis of *both-and*, as opposed to a stance of *either-or*.

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HYPNOSIS AND THE ALTERED STATE DEBATE: SOMETHING MORE OR NOTHING MORE?

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Abstract

Kallio and Revonsuo (2003) present an Altered State Theory of hypnosis that they contend provides a multilevel framework to guide research that can lead to the ultimate resolution of the debate about whether or not hypnosis is an altered state of consciousness. In our commentary, we take the opportunity to clarify a number of simplifications and misrepresentations of sociocognitive theories inherent in the Kallio and Revonsuo's presentation, consider some of their criticisms of nonstate theories of hypnosis, and discuss a number of problems and limitations of their theory.

Key words: hypnosis, nonstate, state, virtuoso

Since the nineteenth century, observations of the seemingly extraordinary, puzzling, and provocative behaviours and experiences (e.g. amnesia, hallucinations) of hypnotized participants have contributed to the idea that hypnosis is 'something more' than simply responding to suggestions. Mesmerism and later hypnotism became associated with the idea of unusual and even supernatural capabilities. Some early investigators argued that magnetized subjects could see without the use of their eyes, travel mentally to distant planets and report back accurately about the inhabitants, spot disease by seeing through the skin to the internal organs of sick individuals, and communicate with the dead (Lynn, Rhue and Spanos, 1994). During the nineteenth and early twentieth centuries, hypnosis researchers concluded that hypnotized individuals acquired greater strength, more accurate memory, and the ability to accomplish feats previously impossible when not hypnotized (Orne, 1959). Whereas many such claims have since been debunked, the contemporary field of hypnosis remains riven with controversy regarding the question of whether or not hypnosis involves an altered or 'trance' state of consciousness that is responsible for the heightened suggestibility manifested by hypnotized individuals. Or put another way, are the effects of hypnotic suggestions the byproduct of something more, or nothing more than waking suggestibility and processes and mechanisms that govern mundane behaviours and experiences?

In their interesting and provocative article, Kallio and Revonsuo (2003) propose that hypnosis is 'something more', and forward the Altered State Theory of hypnosis (AST), according to which hypnotic behaviour consists of both a special state of hypnosis and a 'degree of suggestibility that is not dependent on the presence of the state but is affected by it' (p. 113). They contend that 'if the empirical predictions of this theory would be systematically tested in the future, the central question of hypnosis research could be solved once and for all' (p. 113). We applaud the authors for generating a new theory of

hypnosis and for providing empirical predictions and suggested research to evaluate the theory. In this commentary, we take the opportunity to clarify a number of simplifications and misrepresentations of sociocognitive theories inherent in the Kallio and Revonsuo's presentation, consider some of their criticisms of nonstate theories of hypnosis, and discuss a number of problems and limitations of their theory.

The authors actually divide the 'central question' of hypnosis research into two related questions that are each somewhat ambiguous. The first is: 'Is reference to a special psychological state required when describing and/or explaining hypnosis?' (p. 112). The way the question is posed does not distinguish between a description and an explanation of hypnotic phenomena. Researchers and theorists are generally not at odds regarding descriptions of hypnotic phenomena (e.g. amnesia, participants reports of involuntariness), nor what phenomena encompass the domain of hypnosis (Hilgard, 1973). There are, however, sharp disagreements about explanations of hypnosis.

The second question is: 'Is there a special psychological state, an altered state of consciousness (ASC) involved in the generation of these phenomena?' (p. 112). The authors label workers in the field who answer the question in the affirmative as state view (SV) theorists, while those who answer the question in the negative are termed nonstate view (NSV) theorists, a convention we adopt in our commentary. Kallio and Revonsuo's (2003) second question does not distinguish between whether an altered state causes hypnotic phenomena, or whether an altered state is merely correlated or associated with suggested responses, possibilities that the authors often interchange in their discussion. The correlation possibility is not the subject of much debate, especially if the state in question is construed as nothing more than altered experiences that have suggestion-related physiological correlates. Even if a causal link is taken to mean that a suggestion-related shift in experience is a precursor of an 'hypnotic' response, there may be considerably less disagreement among SV and NSV theorists than Kallio and Revonsuo (2003) imply in their discussion. Indeed, one sociocognitive theory (Kirsch and Lynn, 1998) holds that convincing suggestion-related subjective experiences must be present to trigger hypnotic responses.

Although there are points of little dispute, if not agreement, among state and nonstate theorists, the greatest divergence of opinion pivots around the question of whether a trance or altered 'background state' (Kirsch, 2000) is necessary to account for hypnotic suggestibility in a causal sense. In Kallio and Revonsuo's (2003: 141) terms, 'The state of consciousness refers to the underlying context in the brain in which the phenomenal contents of consciousness are brought about'.

By defining hypnosis in this manner, Kallio and Revonsuo (2003) seek to advance the theoretical debate between SV and NSV theorists, which they contend has been

doomed to be unresolved because theories are inadequate for testing the central questions they propose due to the lack of a generally accepted or shared empirically testable definition of an altered state of consciousness (p. 113). However, a serious problem with the authors' formulation is that they reverse the burden of proof on NSV theorists. To be more specific, the burden of proof of the claim that hypnosis involves an altered state, and the imperative to define such a state, does not rest on NSV theorists, who do not postulate that hypnosis involves a special state to begin with. Accordingly, the lack of definition of an ASC is not so much a problem for NSV theorists as it is for SV theorists, for which the concept of an altered state is crucial to their position.

Kallio and Revonsuo (2003) hold that research designed to demonstrate that hypnosis involves an ASC must carefully define the referent state. On this point we agree. To our knowledge, much research inspired by a SV (e.g. psychophysiological research), has

failed to define or operationalize an altered state of consciousness, distinguish whether such a state is construed in a causal or explanatory sense, or indicate what evidence would falsify the hypothesis that an altered state is present. Thus, in our view, much of the psychophysiological research has been of great interest yet not determinative vis-à-vis the altered state issue.

The validity and value of sociocognitive theories of hypnosis do not depend on 'proving the negative – that is, demonstrating that hypnosis is *not* associated with an altered state of consciousness. Nor is the burden on nonstate theorists to prove that subjects are 'in hypnosis' or that 'hypnosis is present,' as the authors imply (p. 116). Sociocognitive theories view suggestibility as being on a continuum, and not as the byproduct of a state that is present or absent at any given time. NSV theorists seek to identify the determinants of individual differences in suggestibility and hypnotic phenomena rather than a point of entry into an altered state. Accordingly, the criticism of failing to delineate specific criteria for 'being in hypnosis or hypnotized' is based on an arguably irrelevant and arbitrary dichotomy, as far as NSV theories go.

Indeed, the thrust of much sociocognitive research has been to show that hypnotic and nonhypnotic responses have similar antecedents, and that hypnotic suggestibility can be increased or decreased as a function of social and cognitive mechanisms. In fact, Kallio and Revonsuo (2003) acknowledge that a great deal of evidence has accumulated over the years to indicate that sociocognitive variables play a highly influential role (see Lynn and Rhue, 1991). Indeed, many of the so-called markers or distinguishing characteristics of hypnosis (e.g., literalness of response, involuntariness, hidden observers, amnesia) can be reasonably well accounted for in terms of sociocognitive factors and/or are evident in comparable frequency in nonhypnotic conditions (see Spanos, 1986; Lynn, Rhue and Weekes, 1990; Lynn and Rhue, 1991; Wagstaff, 1991; Kirsch and Lynn, 1998). The failure to find consistent differences between hypnotized and nonhypnotized individuals, when appropriate controls are imposed, constitutes strong evidence that it is not necessary to invoke an altered state of consciousness to explain hypnotic responding. Moreover, individual differences are apparent in response to imaginative suggestions regardless of whether the situation is defined as hypnosis or not. Further, research in our laboratory and elsewhere (see Braffman and Kirsch, 2001[**AQ: 1999 only in refs - please clarify**]; Meyers and Lynn, 2004[**AQ: 'Meyer' in refs - which?**]) has shown quite convincingly, we believe, that hypnosis does not increase suggestibility much above nonhypnotic suggestibility, if it increases suggestibility at all.

Kallio and Renonsuo (2003) cite Kirsch's (1985, 1990) expectancy theory to proffer a blanket criticism of NSV theories for being unable to account for hallucinations, noting that if expectations produced hallucinatory experiences, then individuals might experience hallucinations of everyday objects such as keys in places where it was expected they were located, even though they were not physically present. However, there are two problems with this criticism. First, the analogy between hypnosis and everyday life is strained. Hallucinations during hypnosis are suggested, and do not arise spontaneously. It is the context of hypnosis, which provides subjects licence to hallucinate, along with the expectation that it is possible to have a vivid hallucination that legitimizes the use of imagination hallucinate, while specific suggestions (e.g. hallucinate a cat just like a real cat) provide the scaffolding for the hallucination. Second, many NSV theorists (e.g. Sarbin, Coe, Lynn, Wagstaff) do not emphasize expectancies so much in the context of hallucinations as the importance of imaginative abilities, which are no doubt crucial to generating vivid hallucinations. In all likelihood, expectations are important in because they engage imaginative processes, not because they result in the spontaneous occurrence

of hallucinations, although this hypothesis warrants further study.

Given that a great deal of evidence supports an account of hypnosis that does not depend on the ‘something more’ ASC hypothesis, the question arises, ‘What phenomena can such a special altered state account for, above and beyond the “nonhypnotic” variables identified by NSV theorists?’ According to Kallio and Revonsuo (2003), the altered state of consciousness of hypnosis is a rare phenomenon, restricted only to a few individuals dubbed hypnotic virtuosos (p. 141). In fact, virtuosos represent such a vanishingly small percentage of the population that the authors contend that they are so rare that it may be impossible to conduct group studies of them for practical reasons.

For virtuosos, the hypnotic induction purportedly alters the background state of consciousness in such a way as to ‘represent available information non-veridically and/or delusionally so that the resulting phenomenal content of conscious experience will misrepresent reality... regardless of what the specific contents of consciousness are’ (p. 142). The background state is further divided into an input phase after the induction that facilitates the activation of representations in any sensory modality that correspond to the verbal suggestions, and an output phase during which the representations are released so that they ‘modulate the phenomenal content of consciousness’ (p. 143). Because the output phase happens entirely outside the participants’ conscious experience or voluntary control, the altered contents of consciousness ‘simply appear’ in a manner quite distinct from ordinary mental imagery which requires intention and focused attention.

The authors suggest an experiment that they claim holds the potential to reveal differences between virtuoso participants, who receive a neutral hypnotic induction vs. those who do not, in terms of an imagery task that can purportedly reveal the reality of colour experiences in synesthesia. Kallio and Revonsuo (2003) contend that if hypnotized virtuosos exhibit what amounts to suggested eidetic imagery abilities on such a task, it could provide evidence for an ASC, assuming that psychophysiological correlates of successful task responding could be identified and the results could not be attributed to the deliberate use of imagery or focused attention.

In our estimation, the rather extraordinary claim that a tiny minority of the population evidences an altered state of consciousness that is specific to hypnosis and not imagination, and is not present in nonhypnotic situations or in less suggestible subjects demands rigorous research that considers a variety of alternative explanations, if not ‘extraordinary proof’, as Carl Sagan was fond of saying. The problem as we see it is that even if the virtuosos show suggestion-related eidetic imagery, it would demonstrate little more than that the experimenters have succeeded in identifying those rare individuals who possess eidetic imagery abilities, and that these abilities have neural correlates. If such eidetic abilities were not evident in nonhypnotized individuals, then the results may well be attributable to sociocognitive variables related to the failure to employ imagery abilities or perform optimally in the nonhypnotic context (e.g. a ‘holdback effect’ due to poor motivation, demand characteristics). Kallio and Revonsuo make no mention of the need to study the influence of such variables in the context of the research they propose, nor do they elaborate exactly why sociocognitive factors would not be influential in virtuoso subjects. Additionally, the meaningfulness of the data obtained by their proposed study is limited by the failure to include comparison groups of less highly suggestible individuals (high, medium, low suggestible), or participants who are given motivational instructions but not hypnotized. And finally, it seems like it would be difficult to distinguish spontaneous imaginings and more goal-directed imagining, and, given the limits of introspection, to rule out the possibility that individuals were not using strategies to produce hallucinations, even though they reported otherwise. At any rate, the devel-

opment of a 'model system' with reference to a tiny percentage of the population - virtuosos - who are purportedly the only individuals who experience 'hypnosis proper' (p. 113) severely limits the generalizability of the ASC theory to the general population and to understanding hypnotic phenomena across the range of individual differences manifested in response to imaginative suggestions.

Kallio and Revonsuo (2003) suggest that a complete understanding of hypnosis demands consideration of multiple levels of analysis (e.g., social psychological, cognitive, phenomenological, and neurophysiological). We concur, and appreciate their delineation of a multilevel framework of 'description and explanation.' However, the authors oversimplify differences between the state and nonstate views by claiming that proponents of nonstate views localize explanations of hypnosis at the level of social interaction, and that NSV theorists contend that a sufficient explanation should refer to the personal level (e.g. expectations). Moreover, the authors claim that SV theorists localize the crucial phenomena inside the person who is hypnotized. The outside-inside dichotomy is fuzzy and perhaps artificial at best, but, that said, virtually all NSV theorists (see Lynn and Rhue, 1991) have acknowledged the potential importance of imaginative abilities, and mental strategies that participants employ to respond to the requirements of suggestions - all of which are localized 'inside the person' and clearly encompass what Kallio and Revonsuo (2003) term the 'cognitive level' (i.e. how information is processed). To cite only one example, Lynn and his colleagues (Frauman, Lynn, Hardaway, and Molteni (1984), conducted a study to determine whether subliminal stimuli could impact participants' unconscious fantasies and thereby their responsiveness to hypnosis. Additionally, we are not acquainted with a single NSV theorist who currently denigrates the value or relevance of examining neural correlates of behaviours or cognitive activities. Such information is interesting in its own right, independent of its ultimate bearing on the altered state issue. Accordingly, we do not agree with the authors' contention that the 'SV versus NSV debate is really a disagreement about the level of description at which the phenomenon of hypnosis should be conceptualized and the level at which an explanation should be constructed' (p. 138). NSV theories, while regarding hypnotic behaviour as social behaviour, also take into consideration variables that span most of the levels identified by the authors. It is tempting to speculate that had reliable psychophysiological correlates of hypnosis emerged relatively early in the history of hypnosis, there might be no state-nonstate debate today.

Contrary to Kallio and Revonsuo's (2003) implication, we believe the reason the debate between state and nonstate theorists has not resulted in a conclusive victor, much less a satisfactory resolution, has much less to do with the failure to find common definitional ground, than it has to do with the failure of the available evidence to convince proponents of opposing theoretical camps. To make a case that compels NSV theorists, it will be necessary to show that something beyond responsivity to the imaginative suggestions administered during hypnosis, cognitive strategies, participants' motivation and imaginative abilities, and the unique expectational context of hypnosis, accounts for changes in neural/physiological markers observed during hypnosis. Even if the neural underpinnings of responses to hypnotic suggestions could be delineated some day, the burden is on state theorists to demonstrate that the identified markers provide a satisfactory causal explanation of hypnotic phenomena. Still and all, it is difficult to see how if such a case was made it would diminish the explanatory power of sociocognitive variables that have been identified in hundreds of studies over the past four decades.

Despite the wealth of evidence marshalled by NSV theorists, SV theorists still insist that there is 'something more' to hypnosis, and remain adamant that sociocognitive

variables do not provide a complete account of hypnosis. We agree. In fact, a 'complete account' has proven elusive, even though much valuable research has been undertaken in the service of a complete understanding of hypnosis. Scientific discovery is a never ending story, and we are far from reaching a satisfactory conclusion, a least as far as the issues raised by Kallio and Revonsuo (2003) go.

So how do we proceed? Delineation and operationalization of constructs is crucial to any research study, and the enterprise of science in general. Yet there is no 'one way' to operationalize a concept such as 'altered state'. In fact, different ways of operationalizing the construct can yield a rich trove of information, insofar as research based on different conceptualizations of an 'altered state' can expand our understanding of both hypnosis and alterations in consciousness. Accordingly, we see no reason for there to be a uniform definition or operationalization of hypnosis, or altered states, in order for science to advance our understanding of these phenomena.

It may ultimately be more important for researchers who embrace opposing viewpoints and all too commonly virtually shun one another, to begin to talk to each other regarding the pros and cons of scientific methodologies (e.g. control groups, selection of subjects, fMRI research) and the nature and outcome of research that would constitute more or less definitive evidence for competing viewpoints, than for all to settle on a single definition of hypnosis, criteria for the presence or absence of hypnosis, or level of analysis of hypnosis. Hopefully, Kallio and Revonuso's (2003) provocative paper will usher in a new era of congenial dialogue between state and nonstate researchers. Count us in.

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ALTERING THE STATE OF THE ALTERED STATE DEBATE: REPLY TO COMMENTARIES

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Abstract

The main point of our article ‘Hypnotic phenomena and altered states of consciousness: A multilevel framework of description and explanation’ was to clarify, explicate and reveal the differences between current theoretical viewpoints in explaining hypnosis. Furthermore, we wanted to present a research programme and propose some experiments that if carried out, might lend decisive support to either the Nonstate View (NSV) or the State View (SV) approaches to hypnosis. The commentaries revealed that the concept of altered state of consciousness (ASC) still lacks a commonly accepted definition and is in need of further clarification. The controversy between NSV and SV of hypnosis seems to boil down to the question concerning the explanatory power of the neural level and especially to what the results at this level tell us. In this reply we further clarify the multi-level framework of explanation, the problems associated with the concept of ASC, and we explain the rationale for our proposal of using virtuosos as a model system in hypnosis research.

Key words: [AQ: please supply 4 - 6 of these]

Introduction

We are grateful for the thoughtful commentaries on our target paper. Although the commentators have some reservations about our approach to hypnotic phenomena, it seems that many issues have been clarified in the discussion and there is promise of unification of the field in the future. In this reply to the commentaries on our paper, we want to highlight issues that everyone seems to agree about as well as some issues that require further clarification.

Irrespective of the problems encountered already in the definition of the central concepts of the discussion, we regard the state issue as being both highly interesting and relevant in order for the field to proceed towards a better understanding of hypnosis and the phenomena associated with it. We sincerely hope that the ‘state debate’ would go on or, rather, that it could now reach a new level (or a new ‘altered state’) where the central questions can be approached from a fresh angle. We were glad to note that most of the commentators seemed to share this optimistic prospect with us, regarding the debate more as an opportunity to sharpen our understanding of hypnosis rather than a hostile battle between squarely opposing ‘camps’. We respectfully disagree with the view according to which the attempt to reveal the causal mechanism of a theoretically crucial

phenomenon is just 'a distraction from the real business' as Kihlstrom (2004: 2) suggests.

As the commentators pointed out (e.g. Kirsh, 2004; Wagstaff and Cole, 2004), everyone agrees that hypnosis is associated with altered experiences. Therefore, if the mere occurrence of altered experiences were the criterion of an altered state, then everyone would agree that hypnosis is an altered state, and there would be no debate. Also, there seems to be overall agreement that it is necessary to study hypnosis in a multi-level context (social-psychological, personal, phenomenal, cognitive and neural levels). Therefore, experiments at all these levels can add to our understanding of hypnosis. Also the phenomenal level is regarded as playing a central role, as pointed out by many researchers long before us.

Regardless of the theoretical viewpoint defended, SV or NSV, it seems that everyone takes the occurrence of the key phenomenon to be a matter of degree and, therefore, for it to manifest itself in the normal population as almost normally distributed, or as forming a single continuum. This background assumption leads to the view that the explanans behind all kinds of changed behaviours and experiences in the hypnotic context is a single phenomenon: either a nonstate phenomenon that occurs in different degrees in different people and different situations or a state phenomenon that reaches different levels or 'depths' in different individuals. If the NSV is correct, then both simple ideomotor responses as well as vivid sensory and perceptual hallucinations must be explained by referring to the same types of nonstate factors; respectively, if the SV is correct, then both types of phenomena must be explained by the altered state they share. Therefore, our suggestion about focusing primarily on virtuosos and on hallucinations was generally regarded as being too restrictive an approach.

The disagreement that still divides the field is this: is there an altered background state, underlying or causing the altered experiences, that is unique to hypnosis ('hypnotic trance')? If there is such a state, then it is also supposed to figure in the explanation of the altered experiences. The NSV views hold that *all* the altered experiences occurring in the context of hypnosis can be elicited without *any* altered hypnotic state being *ever* involved. The whole range of altered experiences is instead produced by such things as imagination, expectation, placebo effects, the social context that gives the 'license' to hallucinate, eidetic imagery, strategies, and so on. By contrast, the SV holds that hypnotic inductions produce a special hypnotic state and this state is needed when explaining *all* hypnotic phenomena ranging from ideomotor responses to hallucinations. After a hypnotic induction, a person can be in this state to varying degrees and the degree or 'depth' of the state is directly related to the probability that different types of suggestions are fulfilled. An analogy can be drawn with the effects of alcohol: the relative amount of alcohol in a person's blood volume is correlated with the effects that alcohol has on the behaviour and experience of the person.

Another controversy seems to focus on how the empirical evidence from the different levels of explanation and description should be interpreted. This concerns especially the neural level: How to interpret the findings from the neural level, obtained by using the different brain sensing and imaging methods. What exactly do the results from the neural level tell us?

The point we want to raise is this: do the NSV theorists truly need the lower levels (cognitive and neural) in order to explain hypnosis? If the answer is 'yes', then why exactly for what do they need it? Another way of putting this is to ask whether anything crucial would be missing from the NSV explanation of hypnosis if it had all the social-psychological and cognitive factors and mechanisms already sorted out perfectly, but it wouldn't describe those mechanisms at the lower, neural level? The same question can

also be asked from the SV theorists in a slightly different way. How exactly would the hypothesized ASC reveal itself in the data we get from brain sensing and imaging methods?

Lynn et al. (2004) stated that in order to claim that there is an ASC involved in hypnosis we need 'extraordinary proof'. One purpose of our article was to try to figure out what that 'proof' could look like in the experimental context. The crucial question is this: is it possible to devise or at least imagine an experimental setting that puts the SV and NSV against each other in a decisive test?

Furthermore, we wanted to expose and analyse the problems that are not purely experimental but derive from the difficulties of arriving at a clear and empirically testable definition of the concept of 'ASC'. The commentaries demonstrate that the question of the relationship between ASC and hypnosis cannot be solved unless the concept of ASC is first clarified, and everyone participating in the discussion shares approximately the same notion of ASC.

Here we try to further clarify the points the commentators raised as follows. First, we will focus on the theoretical background of multilevel explanation and clarify some issues concerning the levels and their explanatory power. Second we will discuss the problems of the concept of ASC and argue that the only way to proceed in the SV-NSV controversy to a stage where they can be experimentally contrasted with each other is to regard the hypothesized 'state' (or ASC), if it exists at all, as being both ontologically real as well as having causal powers. Third, we will further explain the rationale of focusing on hypnotic virtuosos as research subjects.

Multilevel biological explanation

It seems that both NSV researchers as well as SV researchers regard the phenomenal level as crucially important when studying hypnosis. Kirsch (2004) even noted that the phenomenon that needs to be explained resides at the phenomenal level. Furthermore both Lynn, Fassler and Knox (2004) and Kirsch (2004) pointed out that the levels residing under the phenomenal level (cognitive and neural levels) provide us with highly interesting data concerning hypnosis. Also Wagstaff and Cole (2004) stressed that NS theorists have had much to say about the cognitive mechanisms underlying hypnotic responding.

However, there seem to be strongly conflicting views concerning the explanatory power of the cognitive and the neural levels (e.g. Naish 2004, versus Kirsch 2004). This disagreement seems to focus on our conclusion that the most important levels of explanation for hypnosis are somewhat different for SV and NSV. Since our argument is based on the framework of multilevel explanation, let us briefly take another look at the theoretical basis of this framework.

In current philosophy of science there are two somewhat opposing views of scientific explanation (Salmon 1989): the unification approach, which is mostly based on how explanation works in physics, and the mechanistic approach, which applies especially to the biological sciences.

The mechanistic approach sees explanatory knowledge as an understanding of the hidden mechanisms by which nature works. Proponents of this view (e.g. Bechtel 1994[AQ: not in refs - please give details]; Machamer et al., 2000[AQ: not in refs and please supply all authors]) have pointed out that the traditional notion of a universal law of nature has few if any applications in neurobiology and molecular biology. Biologists typically first identify and describe an interesting system or phenomenon at one level of

organization in nature and then try to figure out what the components of this system are, how they interact, and how they produce the effects that can be observed at the level of the whole system. After this biologists attempt to build an idealized multilevel model of the system, the purpose of which is to show the general structure and function of the system across several levels of organization. (Revonsuo, in press)

When applied to hypnosis research this means that if the phenomenal level is the level to be explained then there have to be some lower level mechanisms which are crucial in explaining and understanding this higher level. This does not mean that the sole and only explanation would reside at the lower level (i.e. cognitive and neural levels) but rather that also an account of the lower levels is necessary in order to fully understand the higher level (i.e. the phenomenal level). We do not argue that other levels such as the social level would somehow be less important since the whole explanation requires all the relevant levels.

But make no mistake, even the multilevel explanation of hypnosis need not and should not reach beyond the explanatorily relevant levels. Otherwise we would have to consider an endless sequence of more and more microscopic levels, such as the chemical, the physical, and quantum levels etc., before we would have a full explanation of any higher level phenomenon.

Therefore, multilevel explanations are restricted to the explanatorily relevant levels. Below the lowest explanatorily relevant level, the explanation 'bottoms out'. Above the highest explanatorily relevant level, the explanation 'tops out'. Beyond these levels, there is nothing further to be found that would help us to understand the explanandum. Thus, also the multilevel explanation of hypnosis will also bottom out at some lower level. Our suspicion is that for the NSV it tends to bottom out earlier than for the SV theorists.

Since the NSV theorists argue that the full explanation of hypnosis/hypnotic phenomena is found at the social-psychological and personal level, this means that what they expect to gain from research done at cognitive and neural levels are correlates of ordinary psychological concepts such as expectations, mental strategies, or perhaps neural correlates of extraordinary imaginative abilities. But still, even those correlates are correlates of fundamentally the same type of imaginative abilities as found in any other situation or population rather than anything special to hypnosis.

Thus, since at the neural levels there is nothing truly crucial to be found required for the satisfactory explanation of hypnosis, the multilevel explanation from the NSV viewpoint will bottom out before the neurophysiological levels come into play. If this interpretation of NSV is not correct, then the NSV theorists should tell us what precisely is it that the neural level specifically adds to their explanatory model of hypnosis. There seems to be nothing to be found there that would be unique to hypnosis, therefore the description of the neural mechanisms does not seem to add anything special to the explanation, just as describing the sub-atomic level would not bring anything special or relevant to the explanation of, say, volcanic eruptions or prey-predator population dynamics.

Correspondingly, for the SV theorists there should also be some lower level where the multilevel explanation bottoms out and therefore investigating such lower levels (e.g. single neurons, molecules) would not provide any additional explanatory power and would therefore not be needed when explaining hypnosis.

Thus it can be concluded that NSV theorists regard the neural level as an interesting extra while the SV theorists regard the information from this level as absolutely crucial in order to fully explain hypnotic phenomena. It is crucial because at that level they expect to find some well-defined phenomenon that is causally potent and unique for hypnosis. Reference to that phenomenon will therefore be necessary when explaining and under-

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standing the special state associated with hypnosis. However, as we noted (Kallio and Revonsuo 2004: 134) so far the SV theorists have not been able to tell how exactly the hypothetical hypnotic state would reveal itself in the context of experimental manipulation, (i.e. how its existence could be empirically either confirmed or falsified).

What is an ASC?

The question of an ASC, trance or hypnotic state has been the centre of the whole state debate throughout its history. Contrary to Kihlstrom (2004) we consider that one of the main problems of this debate is the persisting lack of a commonly accepted definition of consciousness and therefore, also a similar lack concerning the definition of ASC. As long as different researchers do not have a shared understanding of the concept of ASC, it is difficult to reach the stage at which fruitful discussions about hypnosis and ASC can take place. Therefore it is crucially important to present a clear and explicit formulation of the concept of ASC and of its hypothesized relation to the phenomenon of hypnosis.

We believe that hypnosis research should be integrated with multidisciplinary research on consciousness in general and with the models of other altered states of consciousness in particular. Therefore, we have defined the concept of ASC in such a way that the same concept covers as well some of the most common forms of ASC (dreaming, drug states) as well as the hypothesized ASC of hypnosis. We noted, however, some lingering misunderstandings in the commentaries concerning the definition of ASC. Therefore, we try to further clarify our position.

We defined the paradigmatic baseline state of consciousness as a state where we are awake and perceive the environment and ourselves more or less accurately. In this baseline state the contents of consciousness are modulated by the physical environment and the physical body and therefore consciousness succeeds in accurately representing them. For a state of consciousness to count as an altered state, it must be altered in certain ways in relation to this baseline state (for a comparable view, see Hobson, 2001).

Paradigmatic altered states of consciousness (ASCs) are such that the background mechanisms of subjective experience (i.e. the etiological and constitutive mechanisms; see Kallio and Revonsuo, 2004 or Revonsuo, in press) for definitions for these concepts) are temporarily predisposed to create contents of consciousness that misrepresent or create delusional beliefs about the environment or the self.

One of the crucial points in our definition of ASC is that the purely phenomenal contents of consciousness, the patterns of subjective experience as such, do not necessarily reveal whether the 'state' of consciousness is somehow altered or not (noted also by Spiegel, 2004). This means that since the person who is in ASC does not always know that this is the case, neither the subjective experience as such nor the subject's beliefs or judgements about his/her mental state can constitute the decisive criterion for ASC. As we pointed out, there are paradigmatic cases of ASCs (such as dreaming) where the content of experience may be mundane or where the subject does not recognize her state as an altered state.

Kirsch (2004) adequately noted that there are two problems when measuring neurophysiological changes in the background state of consciousness. First, since we will probably find some kind of neurophysiological difference between any two conditions, it is crucially important to define what exactly counts as a sufficient alteration. Second, this alteration has to have a causal role in producing hypnotic phenomena. The AST views this state as ontologically real, as causally efficient, and thus as a necessary part of the multilevel and causal explanation of hypnotic phenomena.

Concerning the neural level changes that would constitute firm evidence for an ASC, it seems too early to tell what these changes might be like. In any case, they should indicate a peculiar state of the background mechanisms of consciousness that cannot be found in the baseline state, not even in simulators or during imagination or after waking suggestions in non-virtuosos. The AST we put forward proposes that if hypnosis involves an ASC, then the emergence of this state should be detectable at the neural level as a difference between the pattern of neurophysiological activity before and after a pure hypnotic induction.

Kihlstrom (2004) seems to be pessimistic that such a pure state of hypnosis could be found, and he would opt for looking at the neural correlates of suggested changes in the particular contents of consciousness. While we believe it is also important to study the latter type of changes in the contents of consciousness, it is really the former kind of state change that could really settle the issue about a special hypnotic state being involved. Vivid hallucinations do not seem possible in a brain whose state is not somehow unusual to begin with when the suggestion for experiencing the hallucination is given. So far we believe that the neural correlates of the ASC of hypnosis might be found in the changed activity of the frontal areas (e.g. Gruzelier 1998, 2000). However, it is still too early to tell whether the changes so far observed are sufficient proof that hypnosis involves some sort of altered state that falls into the same category as the other, less controversial instances of ASCs.

The second problem is associated with the question of whether there are some phenomena that can only be experienced or detected after a hypnotic induction and in association with the hypothesized change in the brain state. Kirsch (2004) revealed new data showing that the change in the Stroop effect reported by Raz, Shapiro, Fan and Posner (2002) could also be detected without hypnosis. This is an interesting finding and if it gets further support it clearly shows that hypnosis is not needed to account for the findings. Lynn et al. (2004) further noted that their recent findings show that hypnotic induction hardly adds anything to baseline suggestibility. This question about the difference between baseline suggestibility and hypnotic suggestibility is a complicated issue but has been elegantly analysed by e.g. Hilgard (1973) and we will not go into it in more detail. Furthermore, the studies conducted around this issue have used widely different approaches and criteria when selecting subjects.

What strikes us as extraordinary is the view by Lynn et al. (2004) that no uniform definition or operationalization of hypnosis or ASC is really even required in science. On the contrary, we believe that progressive and unified research programmes in science can only be based on a clear conceptual foundation that is widely shared in the research community. This is what we find in the more mature branches of science where the most basic concepts (say, 'inclusive fitness', 'gene') are understood in the same way by most if not all of the scientists working in the field. Certainly, such a consensus does not come suddenly, out of the blue, but is usually reached slowly and gradually as the result of decades of disagreement, debate, theorizing and experimentation. If such phenomena as 'hypnosis', 'consciousness', or 'altered states of consciousness' exist at all, then for science to describe and explain them coherently, surely the relevant research community in psychology and cognitive neuroscience should aim at developing an internally coherent and widely shared theoretical vocabulary to make genuine progress in their scientific explanation.

Why use virtuosos?

The proposal brought up by us about focusing on hypnotic virtuosos and case studies was

generally seen as too restrictive (e.g. Naish, 2004) and therefore not capturing the whole range of the phenomenon (e.g. Lynn et al., 2004; Spiegel, 2004; Woody and Sadler, 2004). We believe here is a slight misunderstanding of our position. We do not claim that the whole area of hypnosis research could be covered by just studying a few cases of virtuosos.

Instead we suggested using virtuosos and case studies as a model system in hypnosis research because in them, hypnotic phenomena are manifested in an exceptionally prominent form, just like many genetic phenomena are manifested in the fruit fly in a prominent form. As evidenced by the history of biology, a useful strategy to make initial theoretical progress in a relatively new and fuzzy area of empirical research is to utilize model systems. A good model system allows focused and efficient experimental research to be performed on the most interesting phenomena. Furthermore, the model system may lead to theoretically significant observations and insights that would be difficult to reach otherwise. We consider that in virtuosos the phenomena of hypnosis manifest themselves in their clearest possible form and therefore they can be sharply demarcated from other phenomena with which they might otherwise be confused (however, see Wagstaff and Cole, 2004 for a contrary opinion). In hypnosis research the virtuoso as a model system allows easy observation and manipulation of the phenomenon, such as, for example, rapidly changing between hypnosis and baseline while monitoring the brain function of the subject.

Thus the idea behind this approach is that in order to make headway in understanding hypnosis it is useful to begin the research by focusing on subjects where the phenomena are as clear as possible. If we find something interesting in the virtuosos that we can get a proper empirical grip of we should then move forward to study larger groups and non-virtuoso populations. When building further research on the results acquired from studying the model system, it is possible to form empirically testable hypotheses and theoretical principles that guide the field forward. So the use of virtuosos and case studies is just the point where to start the quest.

AST predicts that there is a change in the 'brain-state' associated with a hypnotic induction (for example, a hand clasp as posthypnotic suggestion to enter hypnosis without any further suggestions, i.e. neutral hypnosis) and that that particular change takes place only in a small proportion of selected individuals. Thus, the way to falsify AST is to show that no such changes take place even in the best virtuosos. If nothing like an ASC can be found even in the most astounding hypnotic virtuosos, then AST can be safely discarded. If something hinting to an ASC is however found, then we need to figure out what it is and whether the same sort of thing can be found in other people. AST predicts that outside the virtuoso population, the same sort of state change is not necessarily found. Instead, the ASC of hypnosis is expected to be restricted to a small fraction of the general population.

The commentators also raised the issue about virtuosos not being a homogenous group (e.g. Woody and Sadler, 2004). We agree, and we believe that this variability is another good reason for doing more detailed research on the virtuoso population. Certainly, it might turn out that only a special subgroup of hypnotic virtuosos will show evidence of an ASC. In any case, the virtuoso population should be systematically charted as to their hypnotic abilities and experiences, and then subjected to decisive empirical tests for the presence of an ASC in the brain. It may turn out that the ASC of hypnosis is equally rare as, say, the altered experiences of genuine synesthesia have been found to be, which are estimated to occur in 1/2000 of the general population. But no matter how rare, if it exists at all, it is a real and important phenomenon, just like synesthesia.

As we previously pointed out (Kallio and Revonsuo, 2003: 146) it is highly important to achieve a phenomenal profile of each virtuoso. However, when doing research with virtuosos it is important to analyse carefully the phenomenal experience and not only the behavioural response. As we and many others have previously noted, the behavioural responses are inadequate as such if they are not associated with the appropriate subjective experience.

Lynn et al. (2004) regards the claim that ASCs only occur in virtuosos as 'extraordinary'. He suspects that even if such a group of special individuals is found, it might not be anything else than the same small group who experience eidetic imagery. This is an interesting suggestion, and fortunately an empirically testable one at that. A group of individuals with eidetic imagery should be subjected to hypnotic suggestions, and a group of hypnotic virtuosos to visual imagery tasks and tests, to determine whether Lynn's hypothesis holds. However, even eidetic imagery is, in the normal case, voluntarily controlled, and not confused with external reality. Eidetic imagery is still imagery rather than hallucination, and something would have to be added in the hypnotic context to turn the imagery into a vision that is taken as reality. We suspect, however, that if eidetic imagery and hypnotic hallucinations would be manifestations of a single underlying phenomenon or ability, the close coupling of these two extraordinary phenomena would have been noticed long ago and already reported in the literature. As far as we know, there is nothing in the literature to suggest that such a coupling exists.

Kirsch (2004) raised the question that the clinical usefulness of hypnosis is not restricted only to virtuosos. We want to point out that the clinical usefulness of hypnosis and the theoretical explanation of hypnosis are two different things. If mental imagery, relaxation, suggestions etc., as used in the already well-established clinical practices, have been found helpful, no future theoretical account of hypnosis can change that fact. Conversely, if an ASC related to hypnosis is found only in the virtuoso population, nothing can change the fact that then the clinical usefulness of hypnosis and the altered state of consciousness related to hypnosis, are based on different mechanisms, and perhaps form two distinct categories of phenomena altogether. Then again, if no ASC will be found even in virtuosos, then the entire range of hypnotic phenomena, including their wide clinical usefulness, can probably be explained with the same type of mechanisms operating across the whole population.

The difference between AST and the current SV and NSV

The NSV views hold that *all* the altered experiences occurring in the context of hypnosis can be elicited without any altered hypnotic state being involved. Correspondently, the SV hold that whole range of altered experiences is associated with some kind of ASC.

This is where the theory we propose, AST, differs profoundly from the two traditional views. AST agrees that socio-cognitive mechanisms might well produce all sorts of exceptionally vivid imagery that alter the subject's experience. AST also agrees that these types of altered experiences do not indicate an ASC because they do not count as genuine hallucinations.

In our view, there is a crucial difference between altered experiences produced by imaginative phenomena that are under a subject's top-down attentional and voluntary control, and genuine hallucinations. We hold that a person cannot, by voluntary effort, attention, expectation, strategy, or social license, produce vivid, genuine hallucinations, including the firm belief that the hallucinated content is a part of physical reality out there rather than a product of one's own mind. To vividly see an elephant in front of you

when in reality there's nothing there, and to earnestly believe that such a creature really is there, requires an altered state of consciousness by definition. Some background mechanisms of consciousness must be working in a highly unusual manner to produce a vivid lifelike percept without any effort from the subject's part, and to have the subject believe that there has been a change in the external physical reality rather than just in one's own mind.

Thus, contrary to the commentators, we do not think that hypnotic phenomena form one smooth continuum. There is a continuum of imaginative phenomena that do not involve any ASC and these cover most of the hypnotic phenomena. And then there are genuinely hallucinatory phenomena that necessarily involve an ASC. Or at least that is the AST hypothesis to be tested: that virtuosos experience genuine hallucinations and that in doing so they are in an altered state of consciousness.

Consider an analogy: dreaming and lucid dreaming are different states of consciousness. Although there is a continuum of reflective thought in ordinary dreams, lucid dreaming is a qualitatively different phenomenon that happens only rarely. Most sleep researchers didn't believe that lucid dreams even exist (but held that the experiences happened during brief arousals from sleep) until a few 'lucidity virtuosos' were carefully studied in the laboratory, where it was possible to show to the sceptics that lucid dreaming exists and is a rare and special state of consciousness that takes place in unambiguous REM sleep, the same neurophysiological context where also ordinary dreams are produced. In the same vein, in hypnotic virtuosos there may be a qualitatively different state of consciousness involved. To test this hypothesis, virtuosos have to be studied in great detail in laboratory conditions. The special state, if it exists at all, surely will never be found by studying large groups of individuals who are not even serious candidates for having it.

When using virtuosos in experimental research, it is highly recommendable to use an adequate reference or control group, such as simulators or low hypnotizables. However, we proposed that it would be even better to come up with experimental designs and tasks that cannot be managed successfully by using only mental imagery, compliance, expectation and the other NSV mechanisms. We were glad to note that Naish (2004), for example, had carefully considered our suggestions for experimentally testing our theory and gave some constructive criticism concerning the experiment utilizing the 'pop-out' effect. Also the interesting result about suppressing the Stroop effect by suggestion only (Pollard et al., in preparation) shows that it is not justified to exclude the NSV mechanisms as playing a role before they have actually been tried out in experimental context. However, we believe that it is possible to come up with experimental designs that would constitute decisive tests of AST. We hope that anyone interested in defending or falsifying AST would contribute to this task.

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