

# CONVERGENCE IN UNDERSTANDING HYPNOSIS? *Perhaps, but Perhaps Not Quite So Fast*<sup>1,2</sup>

JOHN F. KIHLMSTROM<sup>3,4,5</sup>

*Yale University*

*University of California, Berkeley*

**Abstract:** The study of hypnosis has been plagued by conflict. Although a more recent trend has been the search for convergence among disparate points of view, two highly salient issues remain contentious: the question of whether hypnosis involves alterations in consciousness, and the nature and correlates of individual differences in hypnotic response. Theoretical convergence is a laudable goal, but not at the expense of obscuring the complexity of hypnosis as a state of altered consciousness, a cognitive skill, and a social interaction. Perhaps the best prescription for convergence in hypnosis is the cautious conviction advocated by Kenneth S. Bowers and so clearly exemplified in his own research.

At least since Liébault and Bernheim countered Charcot's neurological theory of hypnosis with the doctrine of suggestion, and perhaps as far back as the French royal commission's investigation of mesmerism, the study of hypnosis has been plagued by conflict (for historical surveys, see Dixon & Laurence, 1992; Gauld, 1992; Lynn & Rhue, 1991; Sheehan & Perry, 1976; Shor, 1979). The conflict has often been couched in polarities: animal magnetism versus imagination, physiological reflex versus psychological suggestion, credulous versus skeptical, artifact versus essence, state versus role, state versus nonstate, and so forth. Sometimes the conflict has been bitter, sometimes amusing. I have a vivid recollec-

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<sup>3</sup>Now at the University of California, Berkeley.

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<sup>5</sup>Requests for reprints should be addressed to John F. Kihlstrom, Ph.D., Department of Psychology, University of California, Berkeley, 3210 Tolman Hall, Berkeley, CA 94720. (Electronic mail may be sent to kihlstrm@cogsci.berkeley.edu)

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tion of a symposium presented at the annual meeting of the American Psychological Association in the early 1970s, with Ted Barber, Jack Hilgard, Martin Orne, and Ted Sarbin on the stage, and (as I remember it) all of their seconds—Ken and Pat Bowers, Bill Coe, John Chaves, Fred Evans, Nick Spanos, myself—in the audience, along with a standing-room-only crowd (this might have been where I first met Ken Bowers). At one point, Ted Barber was at the podium, talking about how it was easy to respond to suggestions for analgesia even without a hypnotic induction, if only one had the proper attitudes, motivations, and expectancies. Off to the side, Martin Orne flipped open his cigarette lighter and held his palm over the open flame, in apparent demonstration of Barber's point, but really implying that although what Barber was saying was true, it was also beside the point. The symposium was great theater, as so many others like it were, but I am not sure that it really advanced the understanding of hypnosis either inside or outside the field.

### CONVERGENCE AND CONVERGENCE AGAIN

At that point, more than 20 years ago, there were some who felt that despite the public displays of conflict, the field was really headed toward convergence. For example, Spanos and Barber (1974, p. 508) "attempted to highlight the existence of significant areas of theoretical convergence that transcend the state and nonstate paradigms in hypnosis research." These areas were summarized (p. 508) as follows: (a) The subjects' willingness to cooperate in carrying out the aims of the suggestions is an important but not a sufficient condition for a high level of hypnotic responsiveness, and (b) subjects respond overtly and experientially to suggestions when they become involved in imaginings that are consistent with the aims of the suggestions.

Perhaps there was a convergence on these two issues, although Hilgard (1973) thought it necessary to enter at least a mild (and preemptive) demurrer. He noted that there were at least two issues that continued to divide the field and that these were nontrivial: The first was defined as "trance versus nontrance," and the second as "persistent individual differences versus experimental manipulation." Hilgard did acknowledge some agreement on defining what he called *the domain of hypnosis* as "what happens when the hypnotist, with the consent of the subject, attempts to induce hypnosis through conventional procedures" and later "gives suggestions for particular kinds of actions and experiences" (p. 972). But at the same time, he cautioned against defining hypnotic behavior simply in terms of response to suggestion, because there were several forms of suggestion—impersonal heterosuggestion, conformity, and the placebo response—that lay outside the domain of hypnosis. Furthermore, he urged investigators not to ignore the persistence of individual differences in primary suggestibility, independent of hypnotic induction, and he reminded us that overt behavioral response to

suggestions was not the heart of the matter and that phenomenology, by which he meant subjective conviction in the suggested state of affairs and experienced involuntariness of response, was critical to defining the domain of hypnosis. So, even if the field achieved broad agreement on the domain of hypnosis, it remained divided on several critical issues.

More recently, Kirsch and Lynn (1995) also sought to identify theoretical convergence in hypnosis, arguing that although hypnosis researchers continued to divide into something like state and nonstate camps, the positions adopted on various issues were more like "points on a continuum" (p. 847) than simple dichotomies. I found much to agree with in Kirsch and Lynn's article. For example, I agree wholeheartedly that many of the major issues in hypnosis cut across the camps. But at the same time, when I read it I was struck by Yogi Berra's feeling of "*deja vu* all over again." Like Hilgard more than 20 years earlier, I became concerned that the celebration of convergence not obscure important persisting issues, especially the two issues that Hilgard (1973) identified as persisting problems.

### STATES AND TRAITS

So, for example, in their treatment of the debate over whether hypnosis is an altered state of consciousness, Kirsch and Lynn (1995) asserted that hypnotic induction has only very modest effects on response to hypnotic suggestions and that there are no reliable markers of a hypothesized hypnotic state. Therefore, they conclude that "most hypnosis researchers agree that the impressive effects of hypnosis stem from social influence and personal abilities, not from a trancelike state of altered consciousness" (p. 849). But there *is* a state of altered consciousness in hypnosis: Amnesic subjects cannot remember things they should be able to remember; analgesic subjects do not feel pain that they should feel; subjects asked to be "blind" and "deaf" do not see and hear things that they should be able to see and hear. Even the most mundane motor suggestions involve alterations in consciousness: We feel heavy objects in our hands, objects that are not there, forcing our outstretched arms down to our sides; we feel magnetic forces, forces that do not exist, pulling our extended hands and arms together. These are alterations in conscious experience observed in hypnosis, and it does not matter if they can also occur in the absence of a hypnotic induction, and it does not matter if there are no physiological markers of hypnosis. These alterations in consciousness are what make hypnosis interesting, and they remain to be described and explained.

Similarly, Kirsch and Lynn (1995) asserted that there was a "trait debate that was not there" (p. 849). But the trait debate *was* there, and it *still* is here. It was there in the early 1960s, when Barber (1969) was running subjects, unselected for hypnotizability, through experiments on amnesia, analgesia, and hallucinations, and asserting that anyone could expe-

rience these things so long as situational factors inculcated the appropriate attitudes, motivations, and expectancies in the subjects. It was there 20 years later, when Gorassini and Spanos (1986) introduced the Carleton Skills Training Program to turn refractory subjects into virtuosos with just a little bit of persuasion and instruction. It continues in the debate over context effects (e.g., Council, Kirsch, & Hafner, 1986; Nadon, Hoyt, Register, & Kihlstrom, 1991) and whether there is any substantive relation between hypnotizability and absorption, over and above expectancies induced by the testing situation. And it continues in the debate over the factorial complexity of hypnosis (e.g., Coe & Sarbin, 1971; Tellegen & Atkinson, 1976), and the question of whether the only meaningful dimension along which hypnotic suggestions are to be arrayed is one of difficulty level. The fact that Barber (Barber & Calverley, 1963) and Spanos (Spanos, Radtke, Hodgins, Stam & Bertrand, 1983) developed their own measures of hypnotic responsiveness is not necessarily a sign of convergence in the field—although Wilson and Barber's (1981) later assertion that hypnotic virtuosos were a special class of fantasy-prone people might be taken as such a sign. The trait debate was, and is, over whether there are meaningful individual differences in hypnotic response, their nature, their sources, and their relations to other individual differences. Kirsch and Lynn (1995) sometimes seemed to agree with this characterization of the debate, but this is not to say that the trait debate was not there in the first place. As Hilgard (1973) noted, individual differences in hypnotic response are part and parcel of the domain of hypnosis, and hypnosis researchers ignore them at their peril.

#### AWARENESS, ATTRIBUTION, AND INVOLUNTARINESS

Kirsch and Lynn (1995) are right that the *special process* label bestowed by Spanos (e.g., 1986) on Hilgard's (1977) neodissociation theory was a misnomer, because Hilgard has always been at pains to stress the continuities between hypnosis and other experiences. But they are wrong, I think, to emphasize the "hidden similarities" (p. 850) between Hilgard's and Spanos's accounts of phenomena such as the experience of involuntariness. The core of Hilgard's account of involuntariness is that hypnotic responses are executed by subordinate cognitive control structures, or domain-specific modules, whose communication with a superordinate central control structure, or executive ego, has been disrupted by an amnesia-like barrier (Hilgard, 1977; see also Kihlstrom, 1992a). The result is a division in consciousness, so that a stream of mental activity carried out by the subordinate module is not integrated with the stream of mental activity ongoing in the central executive. The central executive, being ignorant of the intentional activities carried out by the subordinate module, therefore experiences these modules as involuntary. By contrast, the sociocognitive account of nonvolitional responding denies the importance of an amnesia-like barrier (i.e., a division in awareness),

and instead asserts that features of the social context lead hypnotic subjects to misattribute their voluntary responses to the suggestions of the hypnotist.

In one sense, the neodissociation and sociocognitive accounts of experienced involuntariness are both based on misattributions. But this "hidden similarity" is overshadowed by the dramatic difference between the theories concerning the source of the misattribution in the first place. For Hilgard (1977), the misattribution results from the amnesia-like barrier that prevents the subjects from being aware of what, at some other level, they are doing; for Spanos, the misattribution results from features of the social context that lead subjects' causal explanations astray. This is a big difference in underlying mechanism, and it should not be obscured.

Kirsch and Lynn (1995) asserted that the sociocognitive account of experienced involuntariness is more parsimonious than the neodissociative account, because the sociocognitive account requires "neither altered state nor dissociation" (p. 854) as explanatory constructs. But the neodissociation account need not use either *altered state* or *dissociation* as explanatory constructs. In fact, as Kirsch and Lynn acknowledge, Hilgard (1977) has used both terms as descriptive constructs. Hilgard's neodissociation theory considers divisions in consciousness to be empirical facts of mental life, which in turn are to be explained according to the principles of mainstream cognitive psychology.

### DIVISION, CONVERGENCE, AND PARALLEL STREAMS

Indeed, explaining how we are able to erect temporary amnesia-like barriers, so that we are unaware of things of which we ordinarily ought to be aware, is a great challenge. But we will never get around to addressing this challenge, so long as we focus attention on the relatively small commonality between the neodissociation and sociocognitive theories, having to do with misattribution, and ignore the yawning gap between them, having to do with dissociation. Modern cognitive psychology and cognitive science have come to a point where, at long last, they are prepared to take seriously the problem of consciousness, and of the relations between conscious and unconscious mental life (e.g., Kihlstrom, in press). Hypnosis has something unique to contribute to this discussion, but it will not do so if it achieves convergence by ignoring the alterations of consciousness that lie at the core of the experience of hypnosis.

While outlining important areas of disagreement with Kirsch and Lynn (1995), it is important to underscore at least one way in which they, and Spanos and Barber (1974) before them, were correct. As so often happens in conflict, the struggle to understand hypnosis has produced some false polarities, and a particularly interesting example is the out-group homogeneity effect (Tajfel, 1969), by which members of an ingroup

perceive members of an outgroup as more alike than they really are. No matter how one construes the debate over hypnosis, at least as it played out over the professional lifetime of Ken Bowers, there has always been considerable heterogeneity within each group (for details, see Kihlstrom, 1992b). For example, Orne (1959) may believe that there is an essence to hypnosis, something like an altered state of consciousness indexed by trance logic; however, nothing about this belief has prevented him from acknowledging that social demands, motivation, and expectancies are also important elements in the mix, or from doing his share of debunking experiments.

Ken Bowers, like Orne (1959), was more interested in the clever experiment that would answer a question or shed light on a phenomenon than in a demonstration that his theory was right and someone else's was wrong. My favorite example is Bowers's very first published experiment, derived from his doctoral dissertation, which brought attribution theory, itself then in its infancy, to bear on posthypnotic suggestion (Bowers, 1966). Like Hilgard, he preferred experiments of light to experiments of proof. Here, I think of his demonstration that hypnotic analgesia is not mediated by stress inoculation (Miller & Bowers, 1986, 1993). The title of his popular book (Bowers, 1976) clearly indicates his stance: He was *seriously curious* about hypnosis, and he hoped that his own "cautious conviction" (p. 4) would be contagious, so that when his readers were finished "the credulous will have been rescued from superstition and the skeptics from self-righteousness" (p. 4).

Convergence is nice, when you can get it, but as Piaget and others have noted, a little conflict now and then is a good thing too, if only because it seems so necessary for cognitive growth. Although I have no desire to return to the internecine struggles of the 1960s and 1970s, I also think it would be a grave mistake for the community of hypnosis researchers to settle on one or another false consensus that obscures differences in approach and sweeps areas of conflict under the rug.

If both premature convergence and internecine conflict are unpalatable, what to do? As usual, there is a third way. It was known, in the rhetoric of the Cold War, as "peaceful coexistence," and I think it is an apt description of where we should be headed now. Hypnosis is a complex phenomenon, simultaneously a social interaction, with hypnotist and subject interacting in a larger sociocultural context, and a state of altered consciousness, involving basic cognitive mechanisms underlying perception, memory, and thought. When we do eventually achieve consensus on a theoretical account of hypnosis, that account will have to invoke the constructs of both cognitive and social psychology. Perhaps somewhat paradoxically, it seems to me that the surest way to genuine convergence is to follow parallel noncompetitive streams, cognitive and social (with, perhaps, many individual currents within each stream), in which individual investigators feel free to focus their efforts in one

direction or another, as their preferences (and the phenomena they are studying) dictate, without feeling constantly obliged to defend their preferred paradigms or to pit their own against someone else's. This is not to say that we should ignore each others' work. We have much to learn from each other. But if we seek to promote a new, more collegial era in hypnosis research, then maybe we should start by acknowledging our differences and finding ways to live with them. Ken Bowers was good at that, and we should try to emulate him.

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**Kenvergenz im Verstehen der Hypnose?  
Vielleicht, aber vielleicht noch nicht so bald**

John F. Kihlstrom

**Zusammenfassung:** Die Erforschung der Hypnose wird durch einen Konflikt geplagt. Wenn auch ein neuerer Trend für die Suche nach Konvergenz zwischen verschiedenen Ansichten besteht, so bleiben doch zwei hoch saliente Streitfragen konstant: Die Frage ob Hypnose die Veränderung des Bewußtseins beinhaltet, und die Ursache und Korrelate von individuellen Differenzen in der hypnotischen Reaktion. Theoretische Konvergenz ist ein löbliches Ziel, aber nicht um den Preis der Verwischung der Komplexität der Hypnose als einen Zustand veränderten Bewußtseins, einer kognitiven Fähigkeit, und einer sozialen Interaktion. Vielleicht das beste Rezept für Konvergenz in der Hypnose ist die vorsichtige Überzeugung, die von Kenneth S. Bowers vertreten wird und in seiner Forschung so deutlich zum Ausdruck kommt.

**Convergence dans la compréhension de l'hypnose?  
Peut-être, mais peut-être pas si rapidement**

John F. Kihlstrom

**Résumé:** L'étude de l'hypnose a été marquée par les conflits. Même si une tendance plus récente a été la recherche d'une convergence à travers les points de vue disparates, deux problèmes demeurent litigieux: la question de savoir si l'hypnose implique des modifications de l'état de conscience et, la nature et les concomitants des différences individuelles dans la réponse hypnotique. La convergence théorique est un but louable, mais non au prix d'un obscurcissement de la complexité de l'hypnose en tant qu'état modifié de conscience, habileté cognitive et interaction sociale. La meilleure attitude à prescrire pour une convergence dans l'hypnose est peut-être la prudente conviction précoc-



nisée par Kenneth S. Bowers d'ailleurs si clairement affichée dans ses propres recherches.

**¿Convergencia en la comprensión de la hipnosis?  
Quizás, pero quizás no tan rápidamente**

**John F. Kihlstrom**

**Resumen:** El estudio de la hipnosis ha estado plagado de conflictos. Aunque una tendencia reciente ha sido la búsqueda de convergencia entre puntos de vista diferentes dos temas altamente notorios continúan en litigio: la pregunta sobre si la hipnosis incluye alteraciones de la conciencia y la naturaleza y correlatos de las diferencias individuales en la respuesta hipnótica. La convergencia hipnótica constituye una meta loable, pero no ha expensas de oscurecer la complejidad de la hipnosis como un estado de alteración de la conciencia, una capacidad cognitiva y una interacción social. Quizás la mejor prescripción para una convergencia en hipnosis es la cautelosa convicción defendida por Kenneth S. Bowers y tan claramente ejemplificada en su investigación.