



THE FOX, THE HEDGEHOG, AND HYPNOSIS

JOHN F. KIHLMSTROM^{1,2}

University of California, Berkeley, California, USA

Abstract: Isaiah Berlin's contrast between the fox, who "knows many things," and the hedgehog, who "knows one big thing," is the starting point for a consideration of monolithic and pluralistic approaches to hypnosis.

Taken collectively, the papers presented in these special issues of the *International Journal of Clinical and Experimental Hypnosis* on hypnosis and the brain represent a milestone in the progress of theory and method in hypnosis. And for the last several years, whenever I have turned my attention to the status of theory and method in hypnosis, I have thought of Isaiah Berlin.

MONISM AND PLURALISM IN HYPNOSIS

Berlin, who lived from 1909 to 1997 (and whose first name was pronounced *eye-ZIE-uh*), is not exactly a household name, even for an academic philosopher (Ignatieff, 1998). When, sometime during World War II, Winston Churchill scribbled a memo indicating that he wanted to invite "I. Berlin" to dinner, he found himself sitting next to *Irving* Berlin instead. (The error was not discovered until Churchill turned to his guest and asked him to name the most important piece he had written lately; the answer was "White Christmas.") But Berlin was an extremely important figure in both philosophy and literature. At his death, one obituarist wrote that Berlin knew everybody worth knowing in the 20th century. An argument could be made that the British tradition of ordinary-language philosophy was established during the 1930s in his rooms at All Souls College at Oxford University.

Manuscript submitted May 1, 2002; final revision received September 10, 2002.

¹Address correspondence to John F. Kihlstrom, Department of Psychology, MC 1650, University of California, Berkeley, 3210 Tolman Hall, Berkeley, CA 94720-1650, USA. E-mail: kihlstrm@socrates.berkeley.edu

²This article is based on invited addresses presented at the 50th anniversary meeting of the Society for Clinical and Experimental Hypnosis (New Orleans, October, 1999) and at the annual meeting of the American Psychological Association (San Francisco, August, 2001). The point of view represented in this paper is based on research supported by Grant #MH-35856 from the National Institute of Mental Health. I thank Martha Glisky for her comments.

He altered our traditional views of the Enlightenment and of Romanticism. When he was knighted, his citation was not for any particular achievement but simply "for talking." Interestingly, Berlin left us no single magnum opus—just (just!) a mass of essays (Berlin, 1997). Two of those essays have entered the canon of English literature, and both of them are relevant to hypnosis.

One of these essays, "The Hedgehog and the Fox" (originally published in 1953), takes its title from a fragment of verse by the 7th-century (B.C.) Greek poet Archilochus: "The fox knows many things, but the hedgehog knows one big thing." At one level, this aphorism reflects the difference between the fox, with lots of resources at its command, and the hedgehog, with only a single, but highly effective, defense. But there is probably more to it than that, as Berlin made clear in his essay:

Taken figuratively, the words can be made to yield a sense in which they mark one of the deepest differences which divide writers and thinkers, and, it may be, human beings in general. For there exists a great chasm between those, on one side, who relate everything to a single central vision, one system, less or more coherent or articulate, in terms of which they understand, think and feel—a single, universal, organising principle in terms of which alone all that they are and say has significance—and, on the other side, those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause, related to no moral or aesthetic principle. These last lead lives, perform acts and entertain ideas that are centrifugal rather than centripetal; their thought is scattered or diffused, moving on many levels, seizing upon the essence of a vast variety of experiences and objects for what they are in themselves, without, consciously or unconsciously, seeking to fit them into, or exclude them from, any one unchanging, all-embracing, sometimes self-contradictory and incomplete, at times fanatical, unitary inner vision. (pp. 436–437)

Shakespeare was a fox, Berlin asserts, as were Herodotus, Aristotle, Montaigne, Erasmus, Molière, Goethe, Pushkin, Balzac, and Joyce. On the hedgehog side are Dante, Plato, Lucretius, Pascal, Hegel, Dostoevsky, Nietzsche, Ibsen, and Proust. Good company, whichever side you happen to find yourself on. But each side parts company with the other over the matter of whether there is only one truth, in the singular, or many truths, in the plural. For Berlin, the age of Enlightenment, for all its many virtues, is the age of hedgehogs, because of its view that, as he put it (in his essay on "The Divorce between the Sciences and the Humanities," 1974, p. 326), "every genuine question has one true answer. . ."—true for everyone, in every place, at every time; that the process leading to this truth is publicly accessible; and that genuinely true answers do not contradict each other, resulting in a seamless web that holds everything together. Berlin himself preferred a more liberal,

pluralistic (and, for that matter, Romantic) stance, which abjures the quest for a grand synthesis and asserts instead that, in the clash of ideas, it does not necessarily have to be the case that one idea is true and the other is false.

In some ways, "The Hedgehog and the Fox" is related to Berlin's other canonical essay, from 1957, "Two Concepts of Liberty" (Berlin, 1997). In this piece, based on his inaugural lecture as professor of political and social theory at Oxford, Berlin drew an important distinction between *positive liberty*, in which societal restrictions are placed on individual freedom in order to achieve a higher good—a good that is held to be good for everyone, everywhere, at all times—and *negative liberty*, in which individuals are permitted to pursue their own individual visions of the good, and to make their own mistakes in the process. In a sense, negative liberty is for foxes, while positive liberty is for hedgehogs.

Positive liberty has its appeal, Berlin argued, as indicated by the attraction of various utopian schemes of social organization; but it also lays the ground for totalitarianism, however benign that totalitarianism may appear to be. Again, in Berlin's words, this time from a 1988 essay, "The Pursuit of the Ideal" (Berlin, 1997):

Utopias have their value—nothing so wonderfully expands the imaginative horizons of human potentialities—but as guides to conduct they can prove literally fatal. . . . The possibility of a final solution—even if we forget the terrible sense that these words acquired in Hitler's day—turns out to be an illusion; and a very dangerous one. For if one really believes that such a solution is possible, then surely no cost would be too high to obtain it: to make mankind just and happy and creative and harmonious for ever—what could be too high a price to pay for that? To make such an omelette, there is surely no limit to the number of eggs that should be broken—that was the faith of Lenin, of Trotsky, of Mao, for all I know of Pol Pot (p. 13).

According to Berlin, Tolstoy was a fox who believed in being a hedgehog. And, according to some commentators, Berlin himself shared Tolstoy's problem. He was a fox by nature, but he also clearly felt that the world would be better if *everyone* were a fox—an intellectual position verging on hedgehogism and which sometimes made him reluctant to take a stand on the political and social issues of his time, such as labor strife or the Cold War. He understood that the natural sciences are by their very nature monistic, even if the social sciences and humanities must reflect the pluralism of the world that they seek to understand. He fully realized that justice requires a balance between positive and negative liberty.

This tension between pluralism and monism is also characteristic of hypnosis. Hypnosis is a complicated phenomenon. In my view, hypnosis is an altered state of consciousness in which a person has certain

imaginative experiences associated with subjective conviction bordering on delusion and experienced involuntariness bordering on compulsion. Hypnotized people see things that *are not* there, they fail to see things that *are* there, cannot remember what just happened to them, and respond to cues without knowing why. At the same time, hypnosis takes place in the context of a particular social interaction in which the hypnotist gives suggestions and the subject acts on them—an interaction that is embedded in a wider sociocultural matrix of understanding about mind and behavior, including information and misinformation about hypnosis itself. So, hypnosis deserves to be approached from diverse points of view. Even so, the pull of monism is extremely strong, and throughout its history hypnosis has been characterized by controversies between various theoretical points of view, each of which claims to know the “one big thing” about hypnosis, or at least to know the “one big way” to study it.

We can see this tension at several different levels. For example, J.P. Sutcliffe famously noted how easy it is for investigators to fall into a stance of either credulity or skepticism and tried to find a third way to approach hypnosis (Sutcliffe, 1960, 1961). In Sutcliffe’s view, Mesmer, Elliotson, Charcot, Milton Erikson, and Leslie LeCron represented the credulous camp; Franklin, Braid, Bernheim, and R.W. White, the skeptics.³ Representatives of the “third way” were hard to spot in the landscape of the time (as I will argue later, I think that White was one). The best that Sutcliffe himself could do was to argue that the hypnotic subject was deluded as to the “actual stimulus state of affairs”—a position that takes seriously subjects’ self-reports that they are blind (for example) without requiring them to behave as if their eyes had been plucked out. Later, Ronald Shor (Shor, 1979) picked up the theme, discussing the “clannish loyalties and polemics” that force investigators to choose between the Charybdis of assured optimism and the Scylla of disciplined objectivity—or was it the other way around? Shor also discussed how important it is to achieve a situation

³For the record, I think Sutcliffe made a mistake in classifying White, who died in 2001, as a skeptic, because in fact White’s theoretical position was quite pluralistic. White, a pioneer in the early revival of scientific hypnosis, was a student of Henry Murray’s at Harvard, Murray’s successor as director of the Harvard Psychological Clinic, and mentor to Martin T. Orne. He posed for the original of the “hypnosis card” (14BM) of the TAT (Morgan, 2001), and published several groundbreaking research studies (White, 1937a, 1937b; White & Shevach, 1942), including an experiment on hypnotic hypermnesia that is still cited today (White, Fox, & Harris, 1940), as well as several important theoretical papers (White, 1941a, 1941b). In Berlin’s terms, White was a fox who understood that hypnosis is a complex phenomenon, involving both cognitive and social processes. There was no doubt in White’s mind that hypnosis was “an altered state of the person.” But there was also no doubt in his mind that “the hypnotized person is . . . a human being who hears and understands and who tries to behave in the different ways which (*sic*) are proposed to him.”

where the investigator maintains methodological rigor without losing the positive catalyst that is critical to producing hypnosis in the first place. He criticized Charcot for missing the importance of the social context in which hypnosis occurred and Hull for destroying the phenomenon of hypnosis under the guise of scientific objectivity. The point is that we would not have these problems if hypnosis were not a multifaceted phenomenon requiring explanation at multiple levels.

THE STRUCTURE OF HYPNOSIS

One topic within hypnosis that illustrates the tension between monism and pluralism is the question of the internal structure of the scales developed to measure hypnotizability. These scales typically represent individual differences in hypnotizability in terms of a single sum score—for example, the 12 points of the Harvard Group Scale of Hypnotic Susceptibility, Form A (HGSHS: A; Shor & Orne, 1962); or the Stanford Hypnotic Susceptibility Scale, Forms A, B, and C (SHSS: A, B, C; Weitzenhoffer & Hilgard, 1959, 1962). However, factor analyses indicate that they have a somewhat complex internal structure (Spanos, D'Eon, Pawlak, & Mah, 1989–1990; Weitzenhoffer & Hilgard, 1959, 1962). At the first level, factor analysis typically distinguishes between direct, challenge, and cognitive suggestions. Direct suggestions are for the facilitation of motor response, as when subjects are instructed to hold out their arms and told that a magnetic force is pulling their hands together. Challenge suggestions are for the inhibition of motor response, as when subjects are asked to interlock their fingers and told that they cannot unclasp them (direct suggestions are sometimes labeled as “ideomotor” suggestions, but challenge suggestions are also ideomotor in nature, because an idea leads to a motor response). Cognitive suggestions involve alterations in perception and memory, such as hallucinations, posthypnotic suggestion, and posthypnotic amnesia. Here the response is not so much motoric as it is ideational. Discovery of the multidimensional nature of hypnotizability led to the development of the Stanford Profile Scales of Hypnotic Susceptibility, Forms I and II (SPSHS: I, II; Weitzenhoffer & Hilgard, 1967), which permit assessment of subjects' strengths and weaknesses on various subcategories of suggestions falling within the domain of hypnosis.

Although the factorial complexity of hypnosis has been widely accepted among hypnosis researchers, assigning a content interpretation to the factors is made difficult by the fact that the factors also differ in terms of the difficulty levels of their constituent items. Thus, as Hilgard originally noted, direct suggestions are relatively easy, whereas challenge and cognitive suggestions are considerably more difficult. In fact, Coe and Sarbin argued strongly that the factor

structure of hypnotizability was an artifact of item difficulty and that, appearances to the contrary notwithstanding, a "single role-relevant skill" (p. 1) ran through the matrix of interitem correlations (Coe & Sarbin, 1971). Spanos and his colleagues made a similar argument almost 20 years later (Spanos et al., 1989–1990), and Kirsch and his colleagues have also taken a position along these lines (Kirsch, Silva, Comey, & Reed, 1995). The factorial complexity of hypnotizability is good for the fox, but a single-process interpretation of this apparent complexity is what the hedgehog has in mind.

It has not been easy to separate difficulty level from content, because the difficulty levels of the various scale items are fixed by their standardized scoring procedures. The obvious solution is to adjust the scoring criteria so as to equate the mean difficulty levels of direct, challenge, and cognitive suggestions. However, investigators have been reluctant to take this step, because it would effectively destroy the standardized scoring procedure and compromise the value of the scales as instruments for subject selection (it is also a lot of work). However, Tellegen and Atkinson offered a promising solution (Tellegen & Atkinson, 1976). Employing a shortened version of the HGSHS:A, these investigators evaluated subjects' hypnotizability in terms of both the standard dichotomous behavioral criteria and a four-point scale of subjective experience. By adjusting the dichotomous scoring criterion for each item on the subjective scales, they were able to equate the difficulty levels of direct and challenge suggestions. Principal factor analysis, with varimax rotation, yielded a two-factor solution. One of these factors was defined by four challenge suggestions, the other by three direct suggestions.

Tellegen and Atkinson concluded that the factor structure of hypnotizability was not an artifact of item difficulty and that a content interpretation of the factors was appropriate (Tellegen & Atkinson, 1976). Specifically, they proposed that there are several dimensions of hypnotizability, analogous to Thurstone's primary mental abilities. However, these hypnotizability factors are themselves positively intercorrelated, so that assessing hypnotizability in terms of a single score remains justifiable. Of course, the conclusion of multidimensionality strictly applies only to ideomotor suggestions, because suggestions of a more cognitive type, such as hallucinations and amnesia, were left out of their analysis. However, there is evidence that factorial heterogeneity applies to the cognitive as well as the motor items. The evidence for this comes from the SPSHS themselves, which focus on the cognitive items (scores for the ideomotor items, grouped into a single "motor pool," are derived from earlier testing on the SHSS, Forms A and C). Hilgard reported a somewhat impressionistic analysis of the SPSHS, which yielded a number of different profiles: some subjects were good at positive hallucinations but not negative hallucinations, others the

reverse (Hilgard, 1965). At the very least, there may well be facilitative and inhibitory factors within the cognitive domain, as there are in the ideomotor domain.

A single-factor theory cannot explain a multifaceted phenomenon, but single-factor theories are what we mostly get in hypnosis. Partly, I think, this is a legitimate response to the complexity of the phenomenon itself. Hypnosis *is* complex, and we probably need different theories to account for different aspects of the phenomenon, and different investigators are going to pursue different lines of inquiry. But that is thinking like a fox. If you are thinking like a hedgehog, then what you want is *one big theory*, a universal theory that will encompass everything. And that is where we get problems: when a theory that offers a perfectly good explanation of one aspect of hypnosis is over-generalized to hypnosis in its entirety. The problem is exacerbated by our embrace, at least at the level of rhetoric, of the logic of falsificationism popularized by Karl Popper (Popper, 1935/1959). According to this principle, scientists are not supposed to seek evidence that is consistent with their theories, but rather they are supposed to seek evidence that is *inconsistent* with them. Popper's reasoning here is of course correct, from a strictly logical point of view, because no amount of positive evidence that a hypothesis is true can guarantee that tomorrow we might not discover an exception that shows it to be false; but in practice hardly anybody actually does this. Rather, what seems to happen is that, when there are two competing theories, proponents of one view attempt to falsify *the other*, and when that attempt succeeds, claim that their own theory wins by default.

HYPNOTIC EFFECTS ON MEMORY

As an example, in 1980 I reported that subjects experiencing post-hypnotic amnesia will be unable to recall a list of words memorized during hypnosis, but this did not impair their ability to use these words as free associates and category instances (Kihlstrom, 1980). In fact, the unremembered learning experience caused a priming effect, such that words from the memorized list were *more* likely to appear as free associates or category instances than they would have been had the learning experience not occurred at all. The experiment was subsequently cited as a convincing demonstration of the distinction between episodic and semantic memory (Tulving, 1983) and later came to be recognized as one of the earliest demonstrations of a dissociation between explicit and implicit expressions of episodic memory (Schacter, 1987).

In performing the experiment, I have to admit that I did not have any theory of hypnosis in mind. In fact, I have never considered myself to be

a theorist of hypnosis; my interest in hypnosis—other than the fact that hypnosis research is intrinsically more interesting for both experimenter and subject than most other psychological research—has always been for what it can tell us about other things, such as cognition, personality, psychopathology, and consciousness. My purpose was mostly descriptive: following recent advances in memory theory, I was interested in seeing how posthypnotic amnesia would affect different classes of memory tasks. The phenomenon of priming had only relatively recently come to investigators' attention (Meyer & Schvaneveldt, 1971), and the formal distinction between explicit and implicit memory was still in the future (Schacter, 1987).

As it happens, in that paper I announced a revision in my previous theory of posthypnotic amnesia, which entailed a disruption in the search process postulated by classical two-stage theories of memory retrieval (Kihlstrom & Evans, 1979). Rather, I speculated that posthypnotic amnesia entailed a dissociation between contextual and other features of memory traces, impairing performance on tests that required information about episodic context but sparing performance on tests that did not. I even used the term "dissociation"—not to describe hypnosis as a dissociative state but rather in a manner that was entirely compatible with the usage of that term as it was evolving within cognitive psychology more generally—as when we would now say that posthypnotic amnesia dissociates explicit from implicit memory, impairing the former but sparing the latter (Kihlstrom, 1985, 1997). The preservation of priming in posthypnotic amnesia was confirmed in better-controlled subsequent research by Dorfman and Kihlstrom (1994) and by David, Brown, Pojoga, and David (2000).

Spanos and his colleagues subsequently responded with a "re-evaluation" of my claim that posthypnotic amnesia produces impairments on episodic but not semantic memory tasks (Spanos, Radtke, & Dubreuil, 1982).⁴ One condition of their experiment essentially replicated my procedure, with a suggestion that their subjects would be unable to remember the words that they learned while they were hypnotized—e.g., "You will have difficulty remembering things you did while you were hypnotized... You will not be able to remember that you learned any words while you were hypnotized. . . ." In a comparison condition, however, Spanos et al. gave an "alternative" suggestion that subjects would be unable to think of the words in any way: "The words will be completely gone from your

⁴The differential effects of posthypnotic amnesia on episodic and semantic memory had been noted earlier by Williamsen, Johnson, and Eriksen (1965) and confirmed by Barber and Calverley (1966), but both these studies missed the priming effect and its significance, as did Spanos et al. The preservation of priming in posthypnotic amnesia was confirmed by subsequent research (David et al., 2000; Dorfman & Kihlstrom, 1994).

memory... you will be unable to bring these words to mind..." When Spanos et al. replicated my procedure, they replicated my results (they even obtained significant priming on the free association test). However, the alternative suggestion abolished the priming effect and even produced a little *negative* priming. As a result, they concluded that "there is no intrinsic relationship between posthypnotic amnesia and episodic memory tasks. The dissociation between episodic and semantic memory... is dependent on the task requirements conveyed to subjects..." This conclusion, in turn, was held to be consistent with their view that hypnotic behavior represents a strategic social enactment, rather than any alteration in consciousness.

Here is an excellent example, I think, of experimenters so bound to their own theoretical point of view, and so eager to discredit the findings of their theoretical competitors, that they miss the point of their own experiment. The simple fact of the matter is that their "alternative suggestion" is not for posthypnotic *amnesia* at all. Rather, it entails what neurologists and neuropsychologists call *agnosia*—a failure of *semantic* memory. In fact, hypnotic agnosia has been observed before—for example, there are items representing it on SPSHS: I and II (Hilgard, 1965; Weitzenhoffer & Hilgard, 1967). The fact that hypnotic suggestions for agnosia can impair performance on tests of semantic and procedural memory is interesting, and it is sad that the effect has gone almost completely unstudied. But this experimental result cannot impeach dissociations between episodic and semantic memory, or between explicit and implicit expressions of episodic memory, because it concerns a different kind of suggestion entirely.

This episode from the history of hypnosis research illustrates, I think, the particular virtues of the fox, as opposed to those of the hedgehog. Spanos's single-minded devotion to a single conceptual framework, by which everything about hypnosis was to be explained (Spanos, 1986a, 1991), effectively blinded him both to an interesting phenomenon waiting to be (re)discovered and explored and to the possibility of expanding his framework so that it might do better justice to the phenomenon being explained—in this case, the very complex effects of hypnosis on memory. Hypnosis involves cognitive changes that take place in a particular interpersonal context. You cannot ignore either of these factors, either the cognitive ones or the social ones, if you wish to understand what hypnosis is all about. Spanos's insight, that the subject's interpretation of the hypnotist's suggestion is critical to how the subject will respond to the suggestion, is completely valid—it is what Martin Orne had in mind when he constantly reminded us that we needed to understand experiments *from the subject's point of view* (Orne, 1962, 1970, 1973). And for this reason, Spanos et al. were quite right to conclude, as they did, that hypnosis "cannot be adequately understood without giving due consideration to subjects' understandings of the

context in which they are tested" (Spanos et al., 1982, p. 572). But explanation does not end there: we still have to understand the cognitive mechanisms by which that understanding is translated into the subjectively compelling experiences of hypnosis. Once the subject has understood that the suggestion targets words as items in a memorized list, rather than words as entries in the mental lexicon, then other processes take over. These processes are cognitive in nature, and a theory that focuses on interpersonal processes has no conceptual vocabulary to describe them—nor, for that matter, any methodological apparatus to investigate them. If you want to understand posthypnotic amnesia, you have got to do research on memory, and that research has to take account of the principles by which memory operates.

That is why Fred Evans and I looked at organizational strategies in posthypnotic amnesia (Evans & Kihlstrom, 1973; Kihlstrom & Evans, 1979). At the time we began our work, the dominant theory of memory held that retrieval was facilitated by organizational strategies. Posthypnotic amnesia, being reversible, is a disruption of retrieval, so it made sense to look for evidence of *disorganization*, and we found it. Moreover, a set of studies conducted with Leanne Wilson showed that the disorganization was specific to the temporal relations among items in memory (Kihlstrom & Wilson, 1984), not their semantic relations (Wilson & Kihlstrom, 1986)—a pattern of results consistent with the idea that posthypnotic amnesia is a disruption of *episodic* memory, where the temporal relations among events are more important than the semantic relations (Kihlstrom, 1985). After Evans and I published our initial studies of disorganized recall, Spanos and his colleagues interpreted the effect from a social-psychological point of view (e.g., Spanos & Bodorik, 1977). However, the original hypothesis of disorganized retrieval, and the later hypothesis that the disorganization would be specific to the temporal relations among items, would never have arisen from any purely social-psychological theory of hypnosis (Kihlstrom & Wilson, 1988).

To take another example, there is no way, from a purely self-presentational point of view, to understand the fact that posthypnotic amnesia impairs recognition less than it impairs recall (Kihlstrom & Shor, 1978). It does not suffice to say that the apparent dissociation between recall and recognition reflects the subject's understanding of posthypnotic amnesia and the context in which it is tested, because the suggestion for amnesia is perfectly clear. Subjects are not supposed to remember the things they did or experienced while they were hypnotized until the hypnotist says the words, "Now you can remember everything." Simulators understand this clearly, which is why they perform poorly on both recall and recognition, in contrast to real hypnotic subjects, who recognize more than they recall—as we have known since the mid-1960s (Barber & Calverley, 1966; Williamsen et al.,

1965), and as Spanos and his colleagues were reminded when they did the comparison (Spanos, James, & Degroot, 1990).

Why, then, do amnesic subjects recognize hypnotic experiences that they cannot recall? The answer, I think, lies in a *cognitive* theory, proposed by George Mandler, that recognition can be mediated by two quite different processes: retrieval, which is closely related to explicit memory, and familiarity, which is closely related to implicit memory (Mandler, 1980). The theory predicts that where amnesia impairs explicit memory but spares implicit memory, subjects can use a priming-based feeling of familiarity to make relatively accurate recognition judgments. This is what happens in the amnesic syndrome resulting from damage to the hippocampus and other brain structures (Verfaellie & Cermak, 1999; Verfaellie, Giovanello, & Keane, 2001; Yonelinas, Kroll, Dobbins, Lazzara, & Knight, 1998) and in the amnesia resulting from electroconvulsive therapy (Dorfman, Kihlstrom, Cork, & Misiaszek, 1995). And this is exactly what appears to happen in posthypnotic amnesia, as well. Moreover, the theory only makes this prediction in circumstances where subjects are actively trying to remember what happened to them—an attitudinal stance that contradicts the notion that subjects are trying to present themselves as amnesic or actively suppressing retrieval through self-distraction and other strategies. Moreover, the theory makes this prediction *in advance*—it does not have to be elaborated post hoc to accommodate an unexpected observation.

The dissociation between explicit and implicit memory can also elucidate another paradox in posthypnotic amnesia, which is that amnesic subjects can give positive physiological responses to items that they cannot recall or recognize (Allen, Iacono, Laravuso, & Dunn, 1995; Bitterman & Marcuse, 1945; Schnyer & Allen, 1995). By analogy to the psychophysiology of lie detection, it has sometimes been argued that such results indicate that posthypnotic amnesia is not the same as true forgetting and that hypnotic subjects only behave *as if* they had forgotten (Coe, 1978; but see Kihlstrom, 1978). The only problem with this argument is that brain-damaged patients with the amnesic syndrome or prosopagnosia also respond positively to forgotten or unrecognized items (Bauer, 1984; Bentin, Moscovitch, & Heth, 1992). This is because psychophysiological responses, like priming effects, are evidence of implicit memory—and implicit memory is spared in posthypnotic amnesia, as it is spared in many other forms of genuine, undisputed amnesia.

The point of this is not that Coe and Spanos were wrong, and I am right. The point is that Coe, Spanos, and I were investigating different aspects of hypnosis: they were interested in how subjects interpret suggestions and enact the hypnotic role, whereas I was interested in how the principles of memory mediate subjects' responses to these

suggestions. Both kinds of questions are perfectly legitimate. There never was any necessary incompatibility between cognitive and social-psychological approaches to hypnosis, and it is a mistake to believe there was or to act *as if* there were. There is plenty of hypnosis to go around for everyone, and everyone can make a positive contribution, so long as nobody makes a claim that his or her theory is universal and sufficient.

EXPECTANCIES, DISSOCIATIONS, AND AUTOMATICITY

History repeats itself, the philosopher Hegel is supposed to have asserted—and Marx added, rather unkindly, “the first time as tragedy, the second time as farce” (Marx, 1852/1972). More recently, Kirsch and Lynn have proposed a new “social-cognitive” theory of hypnosis (Kirsch & Lynn, 1995, 1998a, 1998b, 1998c), which differs in some respects from the “sociocognitive” account of hypnosis proposed earlier by Spanos (1986a, 1986b, 1991). According to Kirsch and Lynn’s theory, responses to hypnotic suggestions are generated automatically from response expectancies developed in the hypnotic context. In various papers, they have mustered evidence supporting their view, especially with respect to the correlates of hypnotizability, and applied their theory to the experience of involuntariness that lies at the core of hypnosis. That is fine so far as it goes, but they went further by attempting to discredit various versions of the “neodissociation” theory of hypnosis proposed by Hilgard (1973b, 1977) and elaborated in various respects by others (Kihlstrom, 1992a, 1998; Woody & Bowers, 1994; Woody & Sadler, 1998). Here, again, I think we see the Popperian legacy: if one theory is to be true, all competing theories must be wrong. And here, again, I think that the Popperian legacy is being misapplied. There are problems with neodissociation theory, to be sure, but it is not necessarily the case that any failure of neodissociation theory counts as evidence in favor of the social-cognitive alternative—or, for that matter, vice-versa.

In fact, both theoretical approaches have virtues, and both have their problems. Kirsch and Lynn may not care for the hidden observer paradigm on which Hilgard based much of his argument; but the fact is that dissociations are ubiquitous in hypnosis (Kihlstrom, 1987). They occur between explicit and implicit memory, in posthypnotic amnesia and posthypnotic suggestion (Kihlstrom, 1985, 1997); and they occur between explicit and implicit perception, in hypnotic analgesia, the sensory anesthetics, and the negative hallucinations (Kihlstrom, Barnhardt, & Tataryn, 1992). Neodissociation theory does not attempt to explain all of hypnosis: it merely seeks to draw attention to the dissociations that occur *within* hypnosis and to attempt to understand

them in terms of cognitive structure and process. These phenomena cannot be ignored, and they cannot be explained solely in terms of automatic responses to suggestions or response expectancies. Something has to go in the middle, after formulation of the expectation and before the response to suggestion occurs. And that is where neodissociation theory focuses its attention—asking questions about cognitive architecture that social-psychological theories set aside. Neodissociation theory may be unable to explain dissociations between explicit and implicit memory and between explicit and implicit perception, but at least it acknowledges them, and at least it tries.

On the social-cognitive side, I do not think that anyone would argue that expectancies are irrelevant to hypnosis, but it remains to be demonstrated whether expectations, taken by themselves, are all that powerful predictors of hypnotic response. For example, a pair of studies by Shor and his colleagues assessed the impact of expectations on hypnotizability by administering to subjects, prior to their taking the HGSHS, a questionnaire that described each suggestion on the scale and asked them to predict their response to it (Shor, 1971; Shor, Pistole, Easton, & Kihlstrom, 1984). The correlation between predicted and actual total scale scores was very low, with r s ranging from .25 to .34—higher than the typical correlation between hypnotizability and absorption, to be sure, but still leaving the vast bulk of variance unexplained. To my knowledge, the highest correlation yet obtained between naturally occurring (i.e., not experimentally manipulated) expectancies and actual behavioral response is $r = .55$ (Council, Kirsch, & Hafner, 1986). However, this study measured subjects' expectancies *after* they had experienced the response to induction. Although I am sure that the subjects had expectancies before the induction, I am also certain that these expectancies were revised on the basis of the actual induction experience—by the speed with which eye closure occurred, for example (Graham, 1971). So, there must be at least part of hypnotic response that is independent of expectations; or, put another way, expectations are at least as much a function of behavior as behavior is a function of expectations.

It has to be said, too, that the concept of automaticity is highly problematic. In the first place, any notion that response expectations automatically generate responses to hypnotic suggestions has to describe the nature of the automatic processes that intervene between expectancy and experience, and this is the domain of cognitive psychology. So there is no escaping the details of internal, cognitive mechanisms—the same sorts of internal cognitive mechanisms that apparently worry Kirsch and Lynn when it comes to dissociations (Kirsch & Lynn, 1998a, 1998b). Moreover, cognitive psychology offers a technical definition of automaticity: according to traditional formulations, automatic responses are inevitably evoked by appropriate

stimuli, and their execution consumes no attentional resources. But neither of these statements is true of hypnotic responses.

Consider, as just one example, the prototypical example of hypnotic automaticity: posthypnotic suggestion. Posthypnotic behaviors seem to interrupt the normal flow of the subject's experience, thought, and action, occurring involuntarily and outside awareness. But appearances are deceiving. Spanos and his colleagues showed that posthypnotic responding did not occur outside the experimental setting in which the posthypnotic suggestion was originally administered (Spanos, Menary, Brett, Cross, & Ahmed, 1987). The ability of the cue to evoke the posthypnotic response depends on the context in which the cue is administered—or, perhaps, on the subject's interpretation of the cue *as a cue*. All by itself, this violates a fundamental feature of automaticity, which is that automatic responses are inevitably executed upon presentation of the appropriate stimulus, regardless of context or interpretation. Moreover, in a doctoral dissertation completed in my laboratory, Irene Tobis (née Hoyt) found that execution of a posthypnotic suggestion interfered with other ongoing activities, and vice-versa, even when there was no conflict between them (Hoyt & Kihlstrom, 1986, August). Tobis's finding violates another fundamental feature of automaticity, which is that automatic responses do not interfere with other responses, because their execution consumes no cognitive resources.

Now, it is true that cognitive psychology offers a revisionist view of automaticity, which loosens the requirements of context and resource independence (Logan, 1988). But this is a memory-based view of automaticity, and it holds that automatized responses become automatized by virtue of extensive practice: hypnotic subjects simply have not had enough opportunity to practice their responses between the time the posthypnotic suggestion is administered and the time it is given. Nor, of course, have they had the opportunity to practice any of their other hypnotic responses. Therefore, the experience of involuntariness, as powerful as it is for the subject, appears to be illusory—just as neodissociation theory, or at least one form of it, says it is (Kihlstrom, 1992a).

Interestingly, another form of neodissociation theory, proposed by Woody and Bowers (Woody & Bowers, 1994; Woody & Sadler, 1998), holds that hypnotic involuntariness is by no means illusory and reflects a disruption of executive control similar to that found in neurological patients with frontal-lobe damage. Moreover, Spanos proposed a version of social-cognitive theory that holds that hypnotic involuntariness *is* illusory, by virtue of the subject's misattributions concerning the causes of his or her behavior (Spanos, Cobb, & Gorassini, 1985; Spanos & Gorassini, 1984). So it is not the case that experimental findings on automaticity necessarily strike blows for one side and against the other.

Spanos could have cited Hoyt's experiment as evidence in favor of his theory and against that of Woody and Bowers. By the same token, Hoyt's finding that posthypnotic response is *not* automatic, at least in the technical sense of the term, is problematic for both Woody and Bowers, who are dissociation theorists, and for Kirsch and Lynn, who are not.

The point is that each theory needs to be tested in its own terms. And when we do this, we will find that each theory corrects itself, just as my findings about spared implicit memory caused me to abandon my original theory that posthypnotic amnesia resulted from a disruption of the search process in memory retrieval (Kihlstrom, 1985). If it should happen that each theory has a piece of the action, this should not surprise or distress us, because in the final analysis hypnosis is a complex phenomenon, and a variety of different theories can shed light on it. The hypnotic subject responds to suggestions from the hypnotist, and so it is critical to understand how the subject interprets these suggestions, what the subject expects is going to happen, what the subject's attitudes toward the whole thing are, and the like. But these responses entail profound changes in mental state, and so it is also critical to understand how internal cognitive structures and processes operate to produce hypnotic responses. There is more than enough hypnosis to go around, and everyone can get a piece of the action. We are more likely to learn about how hypnosis works if we work cooperatively to approach the phenomenon with an attitude of open inquiry, than if we constantly talk past each other in an atmosphere of competitive hypothesis testing.

NEURAL CORRELATES OF HYPNOSIS

Similar considerations apply when we approach the neural correlates of hypnosis, as the authors of the articles in these Special Issues do. Of course, the neural correlates of hypnosis have been of interest ever since Braid gave hypnosis its name—which, it must be remembered, was originally *neuro-hypnotism* (Kihlstrom, 1992b). Historically, proposals concerning the neural correlates of hypnosis have been rather monolithic in nature (Crawford & Gruzelier, 1992). Braid himself thought that hypnosis reflected the paralysis of certain nerve centers, a physiological state in turn caused by ocular fixation—though, given the state of neurology at the time, he had no idea where those centers might be. William James endorsed the view that hypnosis was a sleep-like state (Kihlstrom & McConkey, 1990), whereas Pavlov believed that hypnosis was a state of cortical inhibition (Edmonston, 1981). More recently, of course, it was commonly suggested that hypnosis is a function of the right cerebral hemisphere (e.g., Bakan, 1969;

MacLeod-Morgan & Lack, 1982; but see Jasiukaitis, Nouriani, Hugdahl, & Spiegel, 1995). For a time, hypnosis—or, at least, hypnotizability—was linked to a predominance of alpha activity in the EEG (for critical reviews, see Dumas, 1977; Evans, 1979); more recent work has emphasized EEG theta (Sabourin, Cutcomb, Crawford, & Pribram, 1990).

Such monolithic theories of the neural correlates of hypnosis are not likely to be correct, if only because the brain does not function in a monolithic manner. Two centuries ago, the brain was considered to be a single organ—or, as we might put it today, a general-purpose information-processing machine. Beginning in the mid-19th century, the case of Phineas Gage (Macmillan, 2000), followed by the pioneering studies of aphasia by Broca and by Wernicke and the cytoarchitectonic studies of Brodmann, led to the view that the brain includes a cluster of specialized modules, or systems, each devoted to a different information-processing function (Fodor, 1983; Gazzaniga, Ivry, & Mangun, 1998). That is, different parts of the brain are activated when the person is engaged in different types of mental activity. Hypnosis does not change this situation, and in fact may exacerbate it, for the simple reason that hypnosis is not a monolithic state. At one moment, the hypnotic subject is experiencing a direct motor suggestion; at another, a challenge suggestion; at another, suggestion for a positive or negative hallucination; at yet another, a suggestion for amnesia or posthypnotic response. Each of these hypnotic activities and experiences is likely to be mediated by a different brain module or system, and it is not clear that they will have anything in common.

Thus, the neural correlates of hypnosis may depend on what the hypnotized subject is *doing*. For example, Crawford and Gruzelier have proposed that hypnosis selectively activates a variety of cortical and subcortical structures and systems, depending on the task required of the subject (Crawford, 2001; Gruzelier, 1998). Put another way, the hypnotizable brain, even when hypnotized, is just like any other brain—only more so. In my view, the Crawford–Gruzelier hypothesis marks the abandonment of the search for unique neural correlates of hypnosis or hypnotizability, because it predicts quantitative, but not qualitative, differences between hypnosis and control conditions. It also marks the abandonment of a particular research strategy: comparing subjects before and after they have been hypnotized. Studies of so-called “neutral” hypnosis, regardless of whether they employ behavioral (Kihlstrom & Edmonston, 1971) or neuroimaging (Maquet et al., 1999) techniques, may be of limited use because response to suggestions—at least, suggestions of a certain type—lies at the core of what hypnosis is. For this reason, brain-imaging studies of hypnosis must not merely induce hypnosis in their subjects: they must also insure that subjects are responding positively to suggestions for the kinds of

subjectively compelling imaginative experiences that define the domain of hypnosis (Hilgard, 1973a).

For example, hypnotically suggested positive auditory hallucinations appear to activate the primary and secondary auditory areas in the temporal cortex (Szechtman et al., 1998; Woody & Szechtman, 2000a, 2000b); this same area is activated when subjects actually listen to an auditory stimulus. Interestingly, hypnotic hallucinations, but not actual listening, also activate the ACC—an area that is also activated in schizophrenic patients who experience auditory hallucinations (Cleghorn et al., 1992). Hypnotic suggestions to drain color from a colored stimulus, or to add color to a gray scale stimulus, appear to reduce and increase activity in the “color area” of the fusiform gyrus (Kosslyn, Thompson, Costantini-Ferrando, Alpert, & Spiegel, 2000). Agnosia-like suggestions that subjects will find color words (e.g., red or blue) to be meaningless symbols in a foreign language abolish the Stroop effect (Raz, Shapiro, Fan, & Posner, 2001), and also induce changes in anterior cingulate structures involved in the executive control of attention (Raz, Fan, Shapiro, & Posner, 2002). Note that these studies reveal neural correlates that closely resemble those associated with similar effects achieved in the absence of hypnosis. For that reason, they may not provide evidence of the unique correlates of hypnosis (assuming that there are any). But at the very least, they should satisfy those who, for whatever reason, distrust self-reports of subjective experiences.

Sometimes, very similar hypnotic effects appear to be achieved through very different neural means. For example, Barabasz et al. (1999) showed that hypnotic suggestions for blindness enhanced the P300 component of the event-related potential in response to a visual stimulus, whereas suggestions for an obstructive visual hallucination diminished this same response. Similar findings were obtained with an auditory stimulus. In every case, the subject failed to perceive the target stimulus; but the structure of the suggestion modulated the precise psychophysiological correlate of the effect. Something similar happens with hypnotic analgesia. Hypnotically suggested alterations in sensory pain appear to be associated with changes in the primary somatosensory cortex, whereas hypnotic alterations in suffering may be associated with changes in the anterior cingulate cortex (Crawford, Gur, Skolnick, Gur, & Benson, 1993; Rainville, Carrier, Hofbauer, Bushnell, & Duncan, 1999; Rainville, Duncan, Price, Carrier, & Bushnell, 1997; Rainville, Hofbauer, Bushnell, Duncan, & Price, 2002). Pain is reduced in either case, but the neural correlate of the hypnotic response depends on the aspect of pain targeted by the hypnotic suggestion. For this reason, investigators who attempt to replicate and extend each others' findings must take care to make sure that they replicate each others' suggestions as well.

The advent and wide availability of neuroimaging techniques such as positron emission tomography (PET) and functional magnetic resonance imaging (fMRI) open new vistas for the understanding of hypnosis. But brain imaging will not solve all of the problems of hypnosis research, any more than it will solve all the problems in any other area of psychological research. This is because brain-imaging, properly done, requires a complete understanding of the task performed by the subject sitting in the machine—an understanding that must first be worked out at the psychological level of analysis, based on behavioral data of human performance. Without an understanding of the task at hand, the illumination of pixels on a computer screen can only show patterns of relative brain activation; they cannot, alone, tell us what the brain is doing. Brain imaging is a nice technique, but it is only one of many possible techniques—one to be employed in order to address certain questions but not to the exclusion of other techniques (and, for that matter, other questions). Foxes engaged in hypnosis research will use brain imaging as appropriate, but they will also do other things.

THE FOX, THE HEDGEHOG, AND PLURALISM IN HYPNOSIS RESEARCH

“The fox knows many things, but the hedgehog knows one big thing.” A nice turn of phrase, but as Berlin himself cautioned us:

Of course, like all over-simple classifications of this type, the dichotomy becomes, if pressed, artificial, scholastic and ultimately absurd. But if it is not an aid to serious criticism, neither should it be rejected as being merely superficial or frivolous; like all distinctions which embody any degree of truth, it offers a point of view from which to look and compare, a starting-point for genuine investigation. (p. 437)

As noted earlier, this caution did not prevent Berlin from providing us with a list of hedgehogs and foxes in politics and literature.⁵ In order to achieve an adequate understanding of hypnosis, it is not enough that we know *one big thing*. We must know many things, either individually or collectively. As hypnosis enters the 21st century and begins to apply increasingly sophisticated brain-imaging technologies to the study of its neural correlates, let us give each other the negative freedom to investigate what interests us about hypnosis. In this way, each of us will work out a part of the puzzle, a puzzle that has intrigued psychology from the time of James, and Freud, and Pavlov, to now.

⁵I have assembled a similar list of hedgehogs and foxes in hypnosis, but space does not permit me to present it here (Aczel, 1996).

REFERENCES

- Aczel, A.D. (1996). *Fermat's last theorem: Unlocking the secret of an ancient mathematical problem*. New York: Four Walls Eight Windows.
- Allen, J.J., Iacono, W.G., Laravuso, J.J., & Dunn, L.A. (1995). An event-related potential investigation of posthypnotic recognition amnesia. *Journal of Abnormal Psychology, 104*, 421-430.
- Bakan, P. (1969). Hypnotizability, laterality of eye movements and functional brain asymmetry. *Perceptual and Motor Skills, 28*, 927-932.
- Barabasz, A., Barabasz, M., Jensen, S., Calvin, S., Trevisan, M.S., & Warner, D. (1999). Cortical event-related potentials show the structure of hypnotic suggestions is crucial. *International Journal of Clinical and Experimental Hypnosis, 47*, 5-22.
- Barber, T.X., & Calverley, D.S. (1966). Toward a theory of "hypnotic" behavior: Experimental analyses of suggested amnesia. *Journal of Abnormal Psychology, 71*, 95-107.
- Bauer, R.M. (1984). Autonomic recognition of names and faces in prosopagnosia: A neuropsychological application of the guilty knowledge test. *Neuropsychologia, 22*, 457-469.
- Bentlin, S., Moscovitch, M., & Heth, I. (1992). Memory with and without awareness: Performance and electrophysiological evidence of savings. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 18*, 1270-1283.
- Berlin, I. (1997). *The proper study of mankind: An anthology of essays*. New York: Farrar, Straus & Giroux.
- Bitterman, M.E., & Marcuse, F.L. (1945). Autonomic response in posthypnotic amnesia. *Journal of Experimental Psychology, 35*, 248-252.
- Cleghorn, J.M., Franco, S., Szechtman, H., Brown, G.M., Nahmias, C., & Garnett, E.S. (1992). Toward a brain map of auditory hallucinations. *American Journal of Psychiatry, 149*, 1062-1069.
- Coe, W.C. (1978). Credibility of post-hypnotic amnesia—Contextualists view. *International Journal of Clinical and Experimental Hypnosis, 26*, 218-245.
- Coe, W.C., & Sarbin, T.R. (1971). An alternative interpretation to the multiple composition of hypnotic scales: A single role-relevant skill. *Journal of Personality and Social Psychology, 18*, 1-8.
- Council, J.R., Kirsch, I., & Hafner, L.P. (1986). Expectancy versus absorption in the prediction of hypnotic responding. *Journal of Personality and Social Psychology, 50*, 182-189.
- Crawford, H.J. (2001). Neuropsychophysiology of hypnosis: Towards an understanding of how hypnotic interventions work. In G.D. Burrows, R.O. Stanley, & P.B. Bloom (Eds.), *Advances in clinical hypnosis* (pp. 61-84). New York: Wiley.
- Crawford, H.J., & Gruzelier, J.H. (1992). A midstream view of the neuropsychophysiology of hypnosis: Recent research and future directions. In E. Fromm & M.R. Nash (Eds.), *Contemporary hypnosis research* (pp. 227-266). New York: Guilford.
- Crawford, H.J., Gur, R.C., Skolnick, B., Gur, R.E., & Benson, D.M. (1993). Effects of hypnosis on regional cerebral blood flow during ischemic pain with and without suggested hypnotic analgesia. *International Journal of Psychophysiology, 15*, 181-195.
- David, D., Brown, R., Pojoga, C., & David, A. (2000). The impact of posthypnotic amnesia and directed forgetting versus implicit and explicit memory; New insights from a modified process dissociation procedure. *International Journal of Clinical and Experimental Hypnosis, 48*, 267-289.
- Dorfman, J., & Kihlstrom, J.F. (1994, November). *Semantic priming in posthypnotic amnesia*. Paper presented at the Psychonomic Society, St. Louis, MO.
- Dorfman, J., Kihlstrom, J.F., Cork, R.C., & Misiaszek, J. (1995). Priming and recognition in ECT-induced amnesia. *Psychonomic Bulletin and Review, 2*, 244-248.

- Dumas, R.A. (1977). EEG alpha-hypnotizability correlations: A review. *Psychophysiology*, 14, 431-438.
- Edmonston, W.E. (1981). *Hypnosis and relaxation: Modern verification of an old equation*. New York: Wiley.
- Evans, F.J. (1979). Hypnosis and sleep: Techniques for exploring cognitive activity during sleep. In E. Fromm & R.E. Shor (Eds.), *Hypnosis: Developments in research and new perspectives* (pp. 139-183). New York: Aldine.
- Evans, F.J., & Kihlstrom, J.F. (1973). Posthypnotic amnesia as disrupted retrieval. *Journal of Abnormal Psychology*, 82, 317-323.
- Fodor, J.A. (1983). *The modularity of the mind*. Cambridge, MA: MIT Press.
- Gazzaniga, M.S., Ivry, R.B., & Mangun, G.R. (1998). *Cognitive neuroscience: The biology of the mind*. New York: Norton.
- Graham, K.R. (1971, April). Rate of eye closure and hypnotic susceptibility. Paper presented at the annual meeting of the Eastern Psychological Association, New York.
- Gruzelier, J. (1998). A working model of the neurophysiology of hypnosis: A review of the evidence. *Contemporary Hypnosis*, 15, 3-21.
- Hilgard, E.R. (1965). *Hypnotic susceptibility*. New York: Harcourt, Brace, & World.
- Hilgard, E.R. (1973a). The domain of hypnosis, with some comments on alternative paradigms. *American Psychologist*, 28, 972-982.
- Hilgard, E.R. (1973b). A neodissociation interpretation of pain reduction in hypnosis. *Psychological Review*, 80, 396-411.
- Hilgard, E.R. (1977). *Divided consciousness: Multiple controls in human thought and action*. New York: Wiley-Interscience.
- Hoyt, I.P., & Kihlstrom, J.F. (1986, August). *Posthypnotic suggestion and waking instruction*. Paper presented at the 94th annual meeting of the American Psychological Association, Washington, DC.
- Ignatieff, M. (1998). *Isaiah Berlin: A life*. New York: Metropolitan Books.
- Jasiukaitis, P., Nouriani, B., Hugdahl, K., & Spiegel, D. (1995). *Relateralizing hypnosis; or Have we been barking up the wrong hemisphere?* Unpublished manuscript, Stanford University.
- Kihlstrom, J.F. (1978). Context and cognition in posthypnotic amnesia. *International Journal of Clinical and Experimental Hypnosis*, 26, 246-267.
- Kihlstrom, J.F. (1980). Posthypnotic amnesia for recently learned material: Interactions with "episodic" and "semantic" memory. *Cognitive Psychology*, 12, 227-251.
- Kihlstrom, J.F. (1985). Posthypnotic amnesia and the dissociation of memory. In G.H. Bower (Ed.), *Psychology of Learning and Motivation* (Vol. 19, pp. 131-178).
- Kihlstrom, J.F. (1987). The cognitive unconscious. *Science*, 237(4821), 1445-1452.
- Kihlstrom, J.F. (1992a). Dissociation and dissociations: A comment on consciousness and cognition. *Consciousness and Cognition: An International Journal*, 1, 47-53.
- Kihlstrom, J.F. (1992b). Hypnosis: A sesquicentennial essay. *International Journal of Clinical and Experimental Hypnosis*, 40, 301-314.
- Kihlstrom, J.F. (1997). Hypnosis, memory, and amnesia. In L.R. Squire & D.L. Schacter (Eds.), *Biological and psychological perspectives on memory and memory disorders*. *Philosophical Transactions of the Royal Academy of Sciences*, 352, 1727-1732.
- Kihlstrom, J.F. (1998). Dissociations and dissociation theory in hypnosis: Comment on Kirsch and Lynn (1998). *Psychological Bulletin*, 123, 186-191.
- Kihlstrom, J.F., Barnhardt, T.M., & Tataryn, D.J. (1992). *Implicit perception, perception without awareness: Cognitive, clinical, and social perspectives* (pp. 17-54). New York: Guilford.
- Kihlstrom, J.F., & Edmonston, W.E. (1971). Alterations in consciousness in neutral hypnosis: Distortions in semantic space. *American Journal of Clinical Hypnosis*, 13, 243-248.

- Kihlstrom, J.F., & Evans, F.J. (1979). Memory retrieval processes in posthypnotic amnesia. In J.F. Kihlstrom & F.J. Evans (Eds.), *Functional disorders of memory* (pp. 179–218). Hillsdale, NJ: Erlbaum.
- Kihlstrom, J.F., & McConkey, K.M. (1990). William James and hypnosis: A centennial reflection. *Psychological Science*, *1*, 174–178.
- Kihlstrom, J.F., & Shor, R.E. (1978). Recall and recognition during posthypnotic amnesia. *International Journal of Clinical and Experimental Hypnosis*, *26*, 330–349.
- Kihlstrom, J.F., & Wilson, L. (1984). Temporal organization of recall during posthypnotic amnesia. *Journal of Abnormal Psychology*, *93*, 200–208.
- Kihlstrom, J.F., & Wilson, L. (1988). Rejoinder to Spanos, Bertrand, and Perlini. *Journal of Abnormal Psychology*, *97*, 381–383.
- Kirsch, I., & Lynn, S.J. (1995). The altered state of hypnosis: Changes in the theoretical landscape. *American Psychologist*, *50*, 846–858.
- Kirsch, I., & Lynn, S.J. (1998a). Dissociating the wheat from the chaff in theories of hypnosis: Reply to Kihlstrom (1998) and Woody and Sadler (1998). *Psychological Bulletin*, *123*, 198–202.
- Kirsch, I., & Lynn, S.J. (1998b). Dissociation theories of hypnosis. *Psychological Bulletin*, *123*, 100–115.
- Kirsch, I., & Lynn, S.J. (1998c). Social-cognitive alternatives to dissociation theories of hypnotic involuntariness. *Review of General Psychology*, *2*, 66–80.
- Kirsch, I., Silva, C.E., Comey, G., & Reed, S. (1995). A spectral analysis of cognitive and personality variables in hypnosis: Empirical disconfirmation of the two-factor model of hypnotic responding. *Journal of Personality and Social Psychology*, *69*, 167–175.
- Kosslyn, S.M., Thompson, W.L., Costantini-Ferrando, M.F., Alpert, N.M., & Spiegel, D. (2000). Hypnotic visual hallucination alters brain color processing. *American Journal of Psychiatry*, *157*, 1279–1284.
- Logan, G.D. (1988). Toward an instance theory of automatization. *Psychological Review*, *95*, 492–527.
- MacLeod-Morgan, C., & Lack, L. (1982). Hemispheric specificity: A physiological concomitant of hypnotizability. *Psychophysiology*, *19*, 687–690.
- Macmillan, M. (2000). *An odd kind of fame: Stories of Phineas Gage*. Cambridge, MA: MIT Press.
- Mandler, G. (1980). Recognizing: The judgment of previous occurrence. *Psychological Review*, *87*, 252–271.
- Maquet, P., Faymonville, M.E., DeGueidr, C., DelFiore, G., Franck, G., Luxen, A., & Lamy, M. (1999). Functional neuroanatomy of hypnotic state. *Biological Psychiatry*, *45*, 327–333.
- Marx, K. (1972). *The eighteenth Brumaire of Louis Bonaparte*. New York: International Publishers. (Original work published in 1852)
- Meyer, D.E., & Schvaneveldt, R.W. (1971). Facilitation in recognizing pairs of words: Evidence of a dependence between retrieval operations. *Journal of Experimental Psychology*, *90*, 227–234.
- Morgan, W.G. (2001). *Origin and history of the earliest Thematic Apperception Test pictures*. Unpublished manuscript, University of Tennessee.
- Orne, M.T. (1962). On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist*, *17*, 776–783.
- Orne, M.T. (1970). Hypnosis, motivation, and the ecological validity of the psychological experiment. In W.J. Arnold & M.M. Page (Eds.), *Nebraska symposium on motivation* (pp. 187–265). Lincoln, NE: University of Nebraska Press.
- Orne, M.T. (1973). Communication by the total experimental situation: Why it is important, how it is evaluated, and its significance for the ecological validity of

- findings. In P. Pliner, L. Krames, & T. Alloway (Eds.), *Communication and affect* (pp. 157–191). New York: Academic.
- Popper, K. (1959). *The logic of scientific discovery*. New York: Harper. (Original work published in 1935)
- Rainville, P., Carrier, B., Hofbauer, R.K., Bushnell, M.C., & Duncan, G.H. (1999). Dissociation of sensory and affective dimensions of pain using hypnotic modulation. *Pain, 82*, 159–171.
- Rainville, P., Duncan, G.H., Price, D.D., Carrier, B., & Bushnell, M.C. (1997). Pain affect encoded in human anterior cingulate but not somatosensory cortex. *Science, 277*(August 15), 968–971.
- Rainville, P., Hofbauer, R.K., Bushnell, M.C., Duncan, G.H., & Price, D.D. (2002). Hypnosis modulates the activity in cerebral structures involved in the regulation of consciousness. *Journal of Cognitive Neuroscience, 14*, 887–901.
- Raz, A., Fan, J., Shapiro, T., & Posner, M.I. (2002, November). *fMRI of posthypnotic suggestion to modulate reading of Stroop words*. Paper presented at the Society for Neuroscience, Washington, DC.
- Raz, A., Shapiro, T., Fan, J., & Posner, M.I. (2001). *Hypnotic suggestion and the modulation of Stroop interference*. Unpublished manuscript, Weill Medical College.
- Sabourin, M.E., Cutcomb, S.D., Crawford, H.J., & Pribram, K. (1990). EEG correlates of hypnotic susceptibility and hypnotic trance: Spectral analysis and coherence. *International Journal of Psychophysiology, 10*, 125–142.
- Schacter, D.L. (1987). Implicit memory: History and current status. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 13*, 501–518.
- Schnyer, D.M., & Allen, J.J. (1995). Attention related electroencephalographic and event-related potential predictors of responsiveness to suggested posthypnotic amnesia. *International Journal of Clinical and Experimental Hypnosis, 43*, 295–315.
- Shor, R.E. (1971). Expectancies of being influenced and hypnotic performance. *International Journal of Clinical and Experimental Hypnosis, 19*, 154–166.
- Shor, R.E. (1979). The fundamental problem in hypnosis research as viewed from historic perspectives. In E. Fromm & R.E. Shor (Eds.), *Hypnosis: Developments in research and new perspectives*. New York: Aldine.
- Shor, R.E., & Orne, E.C. (1962). *Harvard Group Scale of Hypnotic Susceptibility, Form A*. Palo Alto, CA: Consulting Psychologists Press.
- Shor, R.E., Pistole, D.D., Easton, R.D., & Kihlstrom, J.F. (1984). Relation of predicted to actual hypnotic responsiveness, with special reference to posthypnotic amnesia. *International Journal of Clinical and Experimental Hypnosis, 32*, 376–387.
- Spanos, N.P. (1986a). Hypnotic behavior: A social psychological interpretation of amnesia, analgesia, and trance logic. *Behavioral and Brain Sciences, 9*, 449–467.
- Spanos, N.P. (1986b). More on the social psychology of hypnotic responding. *Behavioral and Brain Sciences, 9*, 489–502.
- Spanos, N.P. (1991). A sociocognitive approach to hypnosis. *Theories of hypnosis: Current models and perspectives* (pp. 324–361). New York: Guilford.
- Spanos, N.P., & Bodorik, H.L. (1977). Suggested amnesia and disorganized recall in hypnotic and task motivated subjects. *Journal of Abnormal Psychology, 86*, 295–305.
- Spanos, N.P., Cobb, P.C., & Gorassini, D.R. (1985). Failing to resist hypnotic test suggestions: A strategy for self-presenting as deeply hypnotized. *Psychiatry, 48*, 282–292.
- Spanos, N.P., D'Eon, J.L., Pawlak, A.E., & Mah, C.D. (1989–1990). A multivariate study of hypnotic susceptibility. *Imagination, Cognition and Personality, 9*, 33–48.
- Spanos, N.P., & Gorassini, D.R. (1984). Structure of hypnotic test suggestions and attributions of responding involuntarily. *Journal of Personality and Social Psychology, 46*, 688–696.

- Spanos, N.P., James, B., & Degroot, H.P. (1990). Detection of simulated hypnotic amnesia. *Journal of Abnormal Psychology, 99*, 179–182.
- Spanos, N.P., Menary, E., Brett, P.J., Cross, W., & Ahmed, Q. (1987). Failure of posthypnotic responding to occur outside the experimental setting. *Journal of Abnormal Psychology, 96*, 52–57.
- Spanos, N.P., Radtke, H.L., & Dubreuil, D.L. (1982). Episodic and semantic memory in posthypnotic amnesia: A reevaluation. *Journal of Personality and Social Psychology, 43*, 565–573.
- Sutcliffe, J.P. (1960). "Credulous" and "skeptical" views of hypnotic phenomena: A review of certain evidence and methodology. *International Journal of Clinical and Experimental Hypnosis, 8*, 73–101.
- Sutcliffe, J.P. (1961). "Credulous" and "skeptical" views of hypnotic phenomena: Experiments in esthesia, hallucination, and delusion. *Journal of Abnormal and Social Psychology, 62*, 189–200.
- Szechtman, H., Woody, E., Bowers, K.S., & Nahmias, C. (1998). Where the imaginal appears real: A positron emission tomography study of auditory hallucination. *Proceedings of the National Academy of Sciences, 95*, 1956–1960.
- Tellegen, A., & Atkinson, G. (1976). Complexity and measurement of hypnotic susceptibility: A comment on Coe and Sarbin's alternative interpretation. *Journal of Personality and Social Psychology, 33*, 142–148.
- Tulving, E. (1983). *Elements of episodic memory*. Oxford, UK: Oxford University Press.
- Verfaellie, M., & Cermak, L.S. (1999). Perceptual fluency as a cue for recognition judgments in amnesia. *Neuropsychology, 13*, 198–205.
- Verfaellie, M., Giovanello, K.S., & Keane, M.M. (2001). Recognition memory in amnesia: Effects of relaxing response criteria. *Cognitive, Affective, and Behavioral Neuroscience, 1*, 3–9.
- Weitzenhoffer, A.M., & Hilgard, E.R. (1959). *Stanford Hypnotic Susceptibility Scale, Forms A and B*. Palo Alto, CA: Consulting Psychologists Press.
- Weitzenhoffer, A.M., & Hilgard, E.R. (1962). *Stanford Hypnotic Susceptibility Scale, Form C*. Palo Alto, CA: Consulting Psychologists Press.
- Weitzenhoffer, A.M., & Hilgard, E.R. (1967). *Stanford Profile Scales of Hypnotic Susceptibility Scale, Forms I and II*. Palo Alto, CA: Consulting Psychologists Press.
- White, R.W. (1937a). Prediction of hypnotic susceptibility from a knowledge of subject's attitudes. *Journal of Psychology, 3*, 265–277.
- White, R.W. (1937b). Two types of hypnotic trance and their personality correlates. *Journal of Psychology, 3*, 279–289.
- White, R.W. (1941a). An analysis of motivation in hypnosis. *Journal of General Psychology, 24*, 145–162.
- White, R.W. (1941b). A preface to the theory of hypnotism. *Journal of Abnormal and Social Psychology, 36*, 477–505.
- White, R.W., Fox, G.F., & Harris, W.W. (1940). Hypnotic hypermnnesia for recently learned material. *Journal of Abnormal and Social Psychology, 35*, 88–103.
- White, R.W., & Shevach, B.J. (1942). Hypnosis and the concept of dissociation. *Journal of Abnormal and Social Psychology, 37*, 309–328.
- Williamson, J.A., Johnson, H.J., & Eriksen, C.W. (1965). Some characteristics of posthypnotic amnesia. *Journal of Abnormal Psychology, 70*, 123–131.
- Wilson, L., & Kihlstrom, J.F. (1986). Subjective and categorical organization of recall during posthypnotic amnesia. *Journal of Abnormal Psychology, 95*, 264–273.
- Woody, E., & Szechtman, H. (2000a). Hypnotic hallucinations and yedasesentience. *Contemporary Hypnosis, 17*, 26–31.
- Woody, E., & Szechtman, H. (2000b). Hypnotic hallucinations: Towards a biology of epistemology. *Contemporary Hypnosis, 17*, 4–14.

- Woody, E.Z., & Bowers, K.S. (1994). A frontal assault on dissociated control. *Dissociation: Clinical and theoretical perspectives* (pp. 52-79). New York: Guilford.
- Woody, E.Z., & Sadler, P. (1998). On reintegrating dissociated theories: Commentary on Kirsch and Lynn (1998). *Psychological Bulletin*, 123, 192-197.
- Yonelinas, A.P., Kroll, N.E.A., Dobbins, I., Lazzara, M., & Knight, R.T. (1998). Recollection and familiarity deficits in amnesia: Convergence of remember-know, process dissociation, and receiver operating characteristic data. *Neuropsychology*, 12, 323-339.

Der Fuchs, der Igel und Hypnose

John Kihlstrom

Zusammenfassung: Isaiah Berlins Gegenüberstellung des Fuchses, der "viele weiß", und des Igels, der "eine große Sache weiß", dient als Ausgangspunkt für eine monolithische bzw. pluralistische Betrachtungsweise der Hypnose.

ROSEMARIE GREENMAN
University of Tennessee, Knoxville,
TN, USA

Le renard, le hérisson, et l'hypnose

John Kihlstrom

Résumé: La différence, pour Isaiah Berlin, entre le renard, qui «sait beaucoup de choses» et le hérisson, «qui sait une chose importante» est un point de départ pour considérer les approches monolithiques et pluralistes à l'hypnose.

VICTOR SIMON
Psychosomatic Medicine & Clinical
Hypnosis Institute, Lille, France

El zorro, el erizo, y la hipnosis

John Kihlstrom

Resumen: El contraste de Isaiah Berlín entre el zorro, que "sabe muchas cosas," y el erizo, que "sabe una cosa grande," es el punto de inicio para una consideración de los enfoques a la hipnosis monolítico y pluralístico.

ETZEL CARDEÑA
University of Texas, Pan American,
Edinburg, Texas, USA