

Evidence, Ontology, and Psychological Science: The Lesson of Hypnosis

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Data are never free of philosophical encumbrances. Nevertheless, philosophical issues are often considered peripheral to method and evidence. Historical perspectives likewise are not considered integral to most data-driven disputes in contemporary psychological science. This paper examines the history of the investigation of hypnosis over the last 75 years to illuminate how evidence and method are entangled with epistemology and ontology, how new research directions are forged by changes in the cultural and philosophical landscape, and how unacknowledged philosophical assumptions can result in confusion and empirical cul-de-sacs. Theoretical disputes that appear to be simple empirical matters often entail hidden philosophical issues, and apparent historical continuity at the theoretical level can belie discontinuity at the ontological level. The lesson of hypnosis is that philosophical analysis is as important as methodological rigor.

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Not all disputes in psychological science are empirical matters—many entail philosophical entanglements and confusions. Unfortunately, the training of most scientific practitioners is heavy in method and fact and dismissive of philosophical analyses, resulting in continued calls for more data, better methods, and yet more again, while the philosophical issues animating disputes remain unexamined (Slife, 1997). Most reviews of the literature underscore this blinkered view, focusing on recent methods and findings and concluding with suggestions for future research. Rarely are disputes about data considered in concert with philosophical assumptions. Furthermore, the historical horizon of most reviews is only one or two decades, presuming that prior research is obsolete. The reasons for obsolescence, however, are often not factual inaccuracy, but changes in basic epistemological and ontological assumptions that, in turn, are related to significant alterations in the cultural context. The exclusive focus on data and the foreshortened historical perspec-

tive render such changes and influences invisible. Empirical evidence is but one facet in the advancement of psychological science for, indeed, the very ground of what constitutes legitimate scientific evidence continues to shift.

One way, then, to expose the importance of these issues is to examine a particularly enigmatic phenomenon that has incited scientific debate for decades, if not centuries. Such a phenomenon would enable a broader vision of the changing criteria for legitimate scientific evidence, the cultural factors related to these changes, and the types of problems that can arise when fact is not considered alongside philosophy. Hypnosis is such a phenomenon. Hypnosis is one of the most provocatively generative topics in the history of psychology, giving rise to psychotherapy, personality theories, formulations of unconscious processes, and appreciation of the power of suggestion and social influence. It has befuddled investigators for over two centuries, and continues to do so (Gauld, 1992). This paper selectively reviews the history of hypnosis over the past 75 years to illuminate how evidence and method are entangled with epistemology and ontology, how new research directions are forged by changes in the cultural and philosophical landscape, and how disputes about evidence uninformed by philosophical analysis can give rise to empirical co-

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nundrums. Three historical epochs will be examined: Hullian behaviorism and World War II; postwar disputes about the validity of hypnosis; and neuroscience and the future.

Behavior to Constructs

The investigation of hypnosis in the 19th century was conducted by physicians who used case studies to ascertain its effects on patients suffering from some illness or malady. During this time, the explanation for hypnotic effects moved from an external force, “animal magnetism” at the beginning of the century; to internal biological causes, neural disruption, and hysteria, by mid to late century; to psychological factors, unconscious processes, and suggestion, by the end of the century. These last explanations proved to be an ontological watershed, as physical, biological, and medically based entities, rooted within a scientifically established explanatory framework, were superseded by psychological phenomenon, which was not. Psychological explanations of hypnosis have prevailed throughout the 20th century, but what constitutes legitimate scientific explanation within this domain has been the source of continued, often acrimonious dispute. Indeed, at the beginning of the 20th century, when psychosocial replaced biomedical explanations, the scientific status of these explanations was deeply problematic. Even Janet (1925), a pioneer in forging one of the leading psychological theories of hypnosis, had this to say about his own efforts:

All this (psychological) formulation seemed simultaneously true and void. It seemed superficial and not really scientific . . . psychology was not favorably regarded by the leaders of the medical profession, who remembered that they had heard vague talk on the subject in the later days of school life, and who fancied that it was a mishmash of literature and ethics (p. 205).

Janet’s doubts may echo others in the biomedical sciences, who throughout the 20th century, ignored or eschewed the topic of hypnosis.

The doubts about the scientific viability of psychology in the first decades of the 20th century gave impetus to the rise of behaviorism, which was based on a positivistic philosophy of science, utilized methods that focused on overt, observable behavior, and banished all references to internal, mental phenomenon (e.g., Watson, 1913). Consequently, the study of hyp-

nosis, with its mentalistic explanations and mystical associations, was greatly diminished (Gauld, 1992). Hypnosis, if it was to reemerge as topic of scientific inquiry, would have to be repositioned within this new epistemological and methodological context.

Behaviorism and Hull

Hull’s (1933) remarkable book, *Hypnosis and Suggestibility* repositioned the study of hypnosis. The conclusion offered, that hypnosis derives from suggestion, is not new, but the argumentation is unlike anything published on the topic prior to the 20th century. Whereas the evidential base for the study of hypnosis in the 19th century was case studies of individuals with medical disorders, Hull described the results of a series of experiments with nonclinical populations whose reactions were quantitatively measured, sometimes through cleverly designed instruments that, for example, recorded minute bodily sway, and the evidence was presented in tables and graphs. Those given hypnotic induction are compared to controls who were given the same suggestions in a normal waking condition, so that the true effects of hypnosis, if any, could be unambiguously detected. Hull’s investigations led him to reject many assumptions associated with hypnosis. It is not sleep; rapport plays no role; it is not hysteria; it is not dissociation; there is no increased sensitivity to weak stimuli. The only difference between hypnotized and control groups is increased susceptibility to suggestion in the hypnosis condition, which Hull argued was likely due to increased muscle relaxation that accompanies hypnotic induction. The state of hypnosis, if it can be called that, is simply a quantitative change in susceptibility.

Hull (1933) also offered a detailed explanation of the mechanisms of hypnotic suggestion, which is achieved by short-circuiting the process responsible for voluntary control of behavior. Volition, he argued, results from “active intraorganic symbolic sequences” (p. 396) that lead to subvocal speech, which acts as a self-directed command, evoking action. Hypnotic induction prompts muscle relaxation, attenuating internal symbolic activity, allowing the vocalizations of the hypnotist to be passively accepted, triggering the suggested activity. Earlier theorists often used ideomotor action to explain

this automaticity of response; that ideas, without intervening deliberation, prompt associated action (e.g., Carpenter, 1874; James, 1892). Hull's reductionistic epistemology led him to reject ideomotor action as an explanation for why suggestions automatically evoke the associated experience. "Ideo," or "ideas," lack physical reality. They are the residue of a failed, unscientific, medieval metaphysics. Hull placed quotes around words such as "idea" and "mental" to underscore that these are empty concepts, not realities. Hull also rejected neurophysiological explanations that usually accompany ideomotor explanations, not because they played no role, but because neuroscience was not sufficiently developed to provide useful scientific understanding. Hull proposed instead that the triggering mechanism is habit. Unlike ideas, habit entails physical behavior, and unlike the microlevel explanations of neurophysiology, habit is a molar phenomenon that can be observed and measured. The hypnotists' words, vocalizations, are not carriers of "ideas," but are themselves physical stimuli; sounds. These sounds become conditioned, through association and habit, to evoke bodily experience and behavior. Thus a "new" hypnosis emerges, shaped and formed by a positivistic epistemology, a reductionistic methodology of observable behaviors, and an attendant antimentalistic nominalism.

After Hull investigated hypnosis he turned to a broader ambition to discover the general laws of habit and learning that underlie all behavior. Hull (1943) assumed that humans and other animals are governed by mechanical laws of cause and effect. Behavior, he argued, is goal directed and determined by external stimuli and internal drives, such as hunger and thirst. These drives are intervening variables that must be operationally defined, and only those assessed by objective criteria, such as food or water consumption, could be included as scientifically legitimate entities. Hull sought to discover the Newtonian-like equations that would describe how environmental stimuli and intervening variables could predict behavioral responses.

Hull's theory of learning generated great interest in academic psychology for a decade or two, promising a methodologically rigorous science that would yield the underlying laws of behavior, based entirely on "physical" evidence and causes. Today, his theory is a historical

curiosity. Hull's hypnosis research has suffered a similar fate; even exhaustive histories of the topic bid him only passing mention (e.g., Gauld, 1992). This is both unfortunate and inaccurate, for it underestimates the significance of his contributions to the study of hypnosis. He was instrumental in shifting the center of gravity of investigation from clinical reports in medical settings to experimental analysis in psychological laboratories. His research provided a new standard for what constitutes acceptable scientific evidence in the study of hypnosis. Furthermore, those who would later share his basic behaviorist epistemology, in updated form, will rally around his flag: methodological rigor.

Nevertheless, the oversight of Hull's work is not accidental. Rather, it reveals that dramatic changes occur within psychological science that can render once-important leaders a historical footnote. And changes this large often do not simply result from new data. Hull's conclusions about hypnosis were not overturned by contradictory evidence. Indeed, later research would echo many of Hull's conclusions: rejection of a special state of hypnosis; situational explanations of evidence for hypnotic experiences; no phenomena produced in hypnosis that cannot be produced in waking-state conditions; the psychological laws governing behavior in other contexts apply equally to hypnosis (e.g., Spanos & Chaves, 1991). Hull's work is no longer referenced because basic philosophical assumptions and attendant measurement considerations were dramatically altered. And this occurred in concert with seismic historical events that situated psychological science within a new cultural context; notably war.

War, Epistemology, and Measurement

World War II irrevocably changed the cultural and scientific landscape and also, thereby, transformed the study and practice of psychology. The exigencies of war demanded immediate, practical solutions, and the war's aftermath required treating thousands of traumatized veterans. The Department of Veterans Affairs, to meet these needs, committed unprecedented funding for the training of mental health practitioners, which almost singlehandedly created the institutional structures for the credentialing and practice of clinical psychology (Benjamin, 2005). Psychotherapy and hypnotherapy be-

came extensively used and a cascade of postwar developments followed: The Society for Clinical and Experimental Hypnosis was founded in 1949; the Institute for Research in Hypnosis was established in the 1950s; government funding was provided for PhD research and training in hypnosis throughout the 1950s and 1960s; the American Society of Clinical Hypnosis began in 1958; the International Society for Clinical and Experimental Hypnosis was founded in 1959; board certification for the study and practice of hypnosis was credentialed by the American Board for Psychological Hypnosis in 1960; a division of the American Psychological Association for the study of hypnosis was established in 1969 (Hilgard, 1993).

The expansion of psychology into applied contexts was entwined with important changes in the epistemological foundations of psychology, which ushered in new approaches to hypnosis. Constructivism arose as a new epistemological framework. Kelly's (1955) constructive alternativism married this philosophical approach with psychological measurement and clinical practice, challenging basic behavioral assumptions about both. The assumption of an objective realm of behavioral fact, uncontaminated by inference or values, a foundational belief for Hull, was dealt a body blow by Kuhn (1962), who persuasively argued that this was a chimera; fact and theory interpenetrate. Furthermore, intrapsychic motivation, an anathema for Hull, was found to influence perception and behavior (e.g., Bruner & Goodman, 1947; Chance & Mead, 1955).

This new era revisited many of the topics addressed at the turn of the century, before the behaviorist revolution, from internal motivation to mental processes, including hypnosis. And therein lay potential danger. The ascendancy of behaviorism was prompted by the limitations of method, the lack of scientific rigor, and the failure to produce data that could be replicated, all of which plagued psychology at the beginning of the 20th century. Although the scope of behaviorism was narrow, it was anchored in the bedrock of method, providing legitimacy to the science of psychology. Returning to the topics and concerns of the prior era might also cycle back to the same methodological shortcomings. This did not happen. Postwar developments also included the implementation of new methodological practices that provided scientific, em-

pirical grounding for theory and measurement of complex psychological phenomena. These new practices were not simply more accurate assessments; they altered what constitutes a fact, and consequently, what constitutes legitimate psychological entities and relationships. New methods introduce new ontologies. And for hypnosis, new possibilities arise. These are critical, yet little appreciated advances on which modern psychology rests (Hacking, 1999). One of the most important of these advances is the idea of construct validity.

Construct Validity

Construct validity, along with several other types of validity, such as predictive, concurrent, and content validity, first appeared as systematically formulated methodological concepts shortly after World War II. The timing was not coincidental. Millions of recruits were rapidly mobilized for the war and psychological testing was required to quickly and effectively match soldiers with tasks (e.g., who is most fit for submarine duty?). Psychological investigation was, thus, thrust outside the laboratory and presumed the existence of attributes, abilities, traits, and motivations that are not "operational behaviors" but rather, psychosocial factors that needed to be accurately discriminated—with real-world consequences.

Psychological testing grew in significance but basic questions loomed: How is the validity of a test to be determined? A test may assess behaviors, but are these behaviors accurate indexes of the attribute that the test purports to measure? What, ultimately, is the touchstone on which we can ground conclusions? Prewar behaviorism was skeptical of anything that could not be concretely operationalized. How, methodologically, can psychological assessment of constructs be scientifically legitimized? A special committee of the American Psychological Association was formed shortly after the war to address these concerns. Two prominent members of the committee, Lee Cronbach and Paul Meehl (1955), published a now classic paper that provided the first systematic formulation of construct validity. Their paper initiated intense and continuing development, elaboration, and refinement of the concept of validity in experimental psychology (e.g., Cook & Campbell, 1979; Kazdin, 2003). The soundness of empir-

ical results could now be analyzed, threats to the validity of findings identified, and steps taken to ensure more authoritative outcomes. This was a methodological watershed. Equally important, but much less appreciated, was the ontological revolution that Cronbach and Meehl helped instigate. Hull and the other prewar behavioral reductionists sought to eliminate surplus meaning in their formulations. The inferential distance between observed fact and statement about the variables being measured, they argued, introduces metaphysical assumptions, compromising the ontological reality of the variable so named. Shortly after the war, however, MacCorquodale and Meehl (1948) persuasively argued that inference from data to theory can be scientifically defensible and proposed a terminological distinction between intervening variables, which summarize observable evidence, and hypothetical constructs, which entail theoretical considerations that cannot be deduced from the evidence alone. Hypothetical constructs provide ontological space in which complex phenomena, like hypnosis, can be reconsidered.

Cronbach and Meehl (1955) proposed methods for validating constructs. They also elaborated and extended the conclusions of MacCorquodale and Meehl (1948), arguing that psychological laws, processes, and entities are connected in a nomological network and that “admissible constructs may be remote from observation” (p. 290). Inference does not necessarily lead to metaphysical meaninglessness, but can lead to legitimate scientific constructs. Validation involves evaluating the force of evidence supporting an underlying construct. This is an ongoing, probabilistic enterprise in which a “construct is at best adopted, never determined to be correct” (p. 294). Absolute certainty is fundamentally impossible. Furthermore, they argued that “laws’ governing behavior may be statistical or deterministic” (p. 290). Note that psychological laws are in quotes, suggesting that such statistical laws, probabilistic in nature, may be of a different ontological kind than deterministic laws. The distinction between variables and constructs raises fundamental ontological issues. Variables are physical entities, “out there,” discovered and validated via data. Constructs, in contrast, are “constructed” by theorists that are, or can be, hypothetical. The questions, then, are how well constructs map onto

the measured and observed world, and what level of indeterminacy is acceptable for validity. Also, too, questions arise about the nature of the causal relation among constructs, and between constructs and observables. Psychological constructs may not be “natural kinds” like magnetism, existing in the world independent of human action. Rather, they may be something other, something more ambiguous, and more directly entangled with our efforts to understand ourselves (Hacking, 1999).

Hypothetical constructs entail ambiguity but also introduce new theoretical and ontological possibilities. Indeed hypnosis, a topic largely ignored in the first half of the 20th century (with notable exceptions), becomes a topic of intense interest in the second half. Gauld (1992) labeled this time of investigation, theorizing, and controversy, the “golden age of hypnosis.” This resurgence of hypnosis research did not result from empirical breakthroughs, but from these new, fertile, philosophical, and methodological developments, which allow more complex constructs of hypnosis, more sophisticated options for assessment, and new grounds to challenge its validity.

Hypnosis: Valid or Not?

The history of hypnosis, dating back to the 18th and 19th centuries, can be understood as a dispute between two groups: those who believe hypnosis to be a credible scientific phenomenon, and those who are skeptical (Sutcliffe, 1960). This fundamental dispute, usually fueled by conflicting views about science, ontology, and evidence, also galvanized—and invigorated—investigation in the mid- to late-20th century. The two basic approaches during this time echo, quite explicitly, the theoretical positions at the end of the 19th century; the cognitive dissociative model of Janet versus the social expectancy model of Bernheim (Gauld, 1992). The postwar theories and disputes, however, were not simply a replay. The evidential base differed dramatically. Theory, investigation, and debate occurred within a new empirical framework. Research was voluminous, complex, and sophisticated. Theory needed to be supported by experimental evidence, and disputes often turned on nuanced methodological issues. Debate was repositioned to assessing the methodological justification for the construct

validity of hypnosis and what types of explanations could legitimately count in this new, transformed science of psychology.

Neodissociative Approach

Ernest Hilgard (1973a, 1973b, 1973c, 1975, 1977, 1977), coauthor of the early and now (in revised form) standard measure of hypnotic responsiveness, was also the leader of those who affirmed the validity of hypnosis, who also included Bowers (1976); Kihlstrom (1984), and Orne (1972) among others. These theorists assumed that a special disjuncture from normal, everyday life occurs in hypnosis. Hypnotic phenomena, such as induced analgesia, amnesia, and the experience of nonvolition, contrast dramatically with ordinary waking experience. These experiences do not require proof of their existence—they require explanation for being so unusual. The explanation offered focuses on rational structures of consciousness, similar to those proposed earlier by Janet. What was an anathema to Hull and others schooled in a positivistic reductionism now constitutes basic theory; definition and explanation does not reside within behavioral observables in the external environment, but within the intrapsychic structures of individuals that are inferred from reliable and valid measures of hypnotic responsiveness. What was scientifically problematic for Janet is now a legitimate scientific hypothesis, made possible, not by new evidence, but by new philosophical and methodological developments that give rise to new kinds of evidence and new kinds of inferences.

Hilgard (1973a, 1973c, 1975, 1977, 1977) argued that consciousness consists of a hierarchical cognitive organization governed by an executive ego, or central control system, that makes plans, initiates action, monitors progress, and makes adjustments in the face of challenges, obstacles, and distractions. To achieve desired goals, specific subsystems are activated that operate habitually and automatically, with little monitoring from the executive ego. These subsystems are, however, monitored at a lower level of awareness and coordinated with other subsystems necessary to accomplish the desired goal. For example, the executive ego may decide to drive a car to a destination. The actual driving involves subsystems of action; feet coordinated on gas and brake pedals in accordance

with changing driving conditions, and this seamlessly integrated with hands-on-the-wheel activity. These subsystems are executed automatically, without attention from the executive ego, but their performance is monitored and altered by lower level control systems.

Hypnotic induction disrupts the planning, monitoring, and critical, rational functioning of the executive ego, thereby increasing the readiness to relinquish control to the hypnotist and diminishing the desire for self-initiated action. This results in the central control function dividing itself into two parts, which are separated by an amnesic barrier, that operate as independent streams of consciousness. The part behind the barrier maintains the usual control functions, perceptions, memories, and critical appraisals found in nonhypnotized conditions. These, however, are not accessible to the other part, which is attentive to the hypnotist's suggestions. Thus, in an obviously painful situation accompanied by a hypnotic request to "feel no pain," the part behind the barrier continues to experience and record the pain as it would in a nonhypnotic context. The other part, however, uncritically accepts the hypnotists' suggestions and reports "no pain." Similarly, for hypnotic amnesia, the barriered part remembers while the hypnotist-influenced part reports "no memory." Although the streams are dissociated, the barrier is not complete, as communication between the two can occur and the experience of the nonaware part can be accessed (Hilgard, 1973a, 1973c, 1977). This differs from Janet's formulation of a complete dissociative separation; hence Hilgard labeled his a neodissociative approach. Also, although he used the Freudian inspired term of an executive ego, he distinguished his approach from Freud, arguing that dissociation can occur for purely cognitive—organizational reasons and need not be the result of repression of anxiety producing impulses.

The dramatic disjunctures of hypnosis, in which pain is banished and salient events forgotten, results from significant alterations in the cognitive control structures and constitutes an altered state of consciousness; the greater the number of dissociated cognitive systems, the more pervasive the altered state. However hypnosis is not caused by the state; rather, the state is a concomitant feature of the cognitive dissociation that defines the hypnotic experience. Hilgard (1977) decoupled the phenomenologi-

cal experiences in hypnosis from the underlying causes, thus avoiding the pitfalls of equating hypnosis with a trance state or implying that it somehow plays a generative role. Hilgard also (1977) suggested that the altered state of consciousness produced in hypnosis parallels that found in “whirling dervishes or the dancing Balinese” (p. 165). The behaviors differ, the cultural contexts vary, but the underlying cause of the altered states is the same: dissociated streams of consciousness.

Finally, Hilgard (1977) argued that there are individual differences in hypnotic responsivity. Some individuals are very responsive, others less so, and others hardly at all. Hypnotic responsivity is a trait-like ability that cannot be significantly altered by changing the social context, providing training, or altering the circumstances under which hypnosis is performed. These individual differences are stable across situations and time, underscoring the importance of explanatory factors residing within the individual.

Hilgard (1977) focused on the structure, functioning, and alterations of intrapsychic processes. It is here that the causal nexus as well as the defining features of hypnosis reside. Social, situational, interpersonal, and cultural factors warrant only a passing nod. The “power of the word” was occasionally cited as the reason why hypnotic experiences can be altered or ended by the hypnotist, but this is not integral to the theory. Hilgard used a diagram to depict the process of hypnosis, and it underscores this point; the workings of the hierarchical systems of cognitive functioning are pictured in great detail, whereas the external world, including the interpersonal aspects, is depicted by a single box labeled “Constraints on Ego Autonomy” (Hilgard, 1977, p. 218). The “power of the word” and social factors constrain, instigate but do not constitute hypnosis. Ontologically, hypnosis is fundamentally an intrapsychic phenomenon.

Neobehavioral Approach

The neodissociative theory of hypnosis is challenged by neobehaviorists, who are skeptical about the scientific validity of the construct. They reject that hypnosis is a special state or unique phenomenon, set apart, somehow, from the rest of waking experience, requiring the theoretical gymnastics of cognitive barriers and

unconscious processes. They propose that the behaviors associated with hypnosis can be explained by the social-situational factors that constitute the hypnotic situation. Established psychological theories that offer accurate and empirically substantiated explanations across a variety of social contexts are not abrogated or superseded in hypnosis, and to begin with the presupposition of specialness, as the neodissociative theorists do, is scientifically unacceptable.

The neobehavioral approach begins with Barber in the 1960s and was continued by his colleagues, most notably Spanos, into the 1990s. Throughout, they were unified in objecting to hypnosis as a special phenomenon, using control groups to demonstrate that the effects of hypnosis can be generated in nonhypnotic contexts (rendering it moot), and locating the ontological explanation in “observables” in the environment. Beneath this continuity, however, basic epistemological and ontological assumptions, presupposed in the early disputes, are abandoned for new ones in the later disputes—without acknowledgment. The focus and content of the disputes center on data and methods, resulting in a disorienting parallax; raging disputes over the empirical validity of hypnosis are accompanied by quiet shifts in what constitutes legitimate scientific evidence and ontologies. And this shift, like that which rendered Hull’s obsolete, was influenced, in part, by changes in the cultural context of investigation. The following discussion traces these changes from the early positivistic reductionism of Barber to the later social constructivism of Spanos.

Barber. Barber’s (1969) seminal book, *Hypnosis: A Scientific Approach*, systematically challenged the validity of the construct, hypnosis. Throughout the book, hypnosis is placed in quotes or italicized to underscore the basic contention that hypnosis is only a name, and lacks an existent referent. Barber continued Hull’s practice as scientific gatekeeper, using quotes to demark empirically bankrupt constructs, words, concepts, and ideas, as adjudicated from a behavioral-positivistic epistemology. Hypnosis, Barber argued, is a behavioral response to a test situation, similar to other test situations, and the unusual responses are simply the response to unusual social context demands, procedures, and expectations—and no more. Once the effects of the social, procedural, and contextual

factors are removed, there is, then, nothing left; hypnosis is an empty set.

Barber's (1969) critical analysis centers on two broad issues. The first is methodological. Barber argued that much more sophisticated nonhypnotic control conditions are required than that pioneered by Hull. Barber's argument reflects the substantial methodological and theoretical advances since Hull, and is a textbook case of the steps needed to protect against threats to construct validity: Results demonstrating that a construct (e.g., hypnosis) produces an outcome (e.g., analgesia) presume that no confounding factors integral to the testing situation produced the observed effects. However, if it can be demonstrated that important differences between the experimental (e.g., hypnosis) and control (e.g., nonhypnosis) conditions exist, (e.g., task instructions), and when controlled, the same outcome is produced in both conditions (yielding no difference), then the validity of the construct (hypnosis) is compromised (Cook & Campbell, 1979; Kazdin, 2003). Barber offered evidence that a host of factors, not usually controlled, may be responsible for the observed experimental effects of hypnosis, including participants' attitudes and expectancies, wording and voice tone of instruction, the task-motivational and relaxation-sleep nature of the instructions, and the personal role played by the experimenter. His research agenda, subsequently adopted by Spanos and others, was to demonstrate that when situational factors are controlled, no "hypnosis" remains.

The second issue Barber (1969) raised is the validity of theories that purport to explain hypnotic behavior. Barber, a post-Hullian behaviorist, argued that the explanatory linkages between the independent (e.g., "hypnosis") and dependent (e.g., analgesia) variables be "trimmed of surplus meaning" (p. 10). His "theory" was a summary of the collection of test-situation behaviors that influenced the outcome behaviors. Future theory, Barber argued, should seek further parsimonious reductions in the explanatory constructs. This squares with the recommendations of MacCorquodale and Meehl (1948) who, although allowing for an inferential distance between fact and theory, stress that it should be as minimal as possible, and that the explanatory constructs need to be embedded in an empirically established theory. They explicitly address—and reject—constructs tied to

Freudian theory, which they argue are purely metaphorical and, thus, bereft of scientific validity. Furthermore, the Freudian concept of unconscious processes, adopted in modified form by Hilgard, is the single most troubling construct for generations of traditional behaviorists (Meichenbaum & Gilmore, 1984). Although never directly stated by Barber and his colleagues, this is, no doubt, one of the most critical constructs to be "trimmed."

The critique by MacCorquodale and Meehl (1948) was one of a number of broadsides leveled against Freud at midcentury, when psychoanalytic theory played a central role in the understanding of psychopathology and the practice of psychotherapy (e.g., Eysenck, 1953). Hilgard, although differentiating his neodissociative approach from Freud's repression model, nevertheless shares a similar emphasis on intrapsychic unconscious processes. Indeed, this kinship drew the ire of Barber and his colleagues, who viewed Hilgard's streams of consciousness separated by barriers as Freudian-like metaphors; a scientific anathema to be banished from psychological theory. This second issue was much less overtly stated but no less important in animating the exchanges between the advocates and skeptics of hypnosis. And the dispute about both issues, construct validity and the validity of the explanatory theory, involved more than a disagreement about hypnosis. What was at stake was something more fundamental—establishing what constituted scientific legitimacy in the changing field of psychology.

Barber's (1969) book occurred at a transformational time in the field of psychology. Hull would have agreed with Barber's approach and conclusions, with few exceptions. Much changed, however, in the next two decades. Imagery, for example, a metaphysical extravagance to Hull and unmentioned by Barber, becomes an intense topic of study in the 1970s (e.g., Paivio, 1970). Behaviorism, too, becomes more inward, more cognitive (e.g., Bandura, 1969). Barber's work also changed. Five years after his first book, he coauthored another entitled, *Hypnosis, Imagination and Human Potentialities* (Barber, Spanos, & Chaves, 1974). The theoretical explanation for hypnosis is labeled a cognitive-behavioral approach and focus is given to both interpersonal factors and to internal strategies individuals employ to success-

fully respond to hypnotic suggestions. The importance of imaginative involvement is also now viewed as key to understanding the successful responses to hypnotic suggestions. And, finally, hypnosis is situated within the human growth and potential movement, an explicitly contra-behavioral orientation that swept psychology and American culture in the 1960s and 1970s. Barber and his colleagues maintained the methodological challenge to the construct validity of hypnosis, but embraced a theoretical perspective alien to the theoretical austerity of the earlier neobehavioral reductionism. Hull would not have approved.

This pattern of ever-more elaborate theoretical explanations while maintaining the methodological challenge to neodissociative explanations (e.g., replicating hypnotic behaviors in nonhypnotized controls) continues for the rest of the century. One reason for increased theoretical complexity is that what constitutes empirically based, scientifically legitimate psychological constructs, and explanations expands dramatically. What Hull would (and did) decry as medieval metaphysical nonsense would become theoretical commonplace. This philosophical shift is why Hull's theory became a relic, why the skeptical argument became more elaborate, and why hypnosis became less easily dismissed as a scientifically bankrupt construct.

This trajectory is exemplified in Barber's own work, which begins in neobehavioral skepticism, proceeds to embellished theoretical opposition, and ends in rapprochement. The final step in the evolution of his theory occurred in the 1980s, when he proposed that there are two differing types of hypnotic involvements. One consists of motivated, positive-set responses, which is congruent with his earlier approach, while another results from imaginative involvement by high fantasy prone individuals, which is consonant with neodissociative approaches (Wilson & Barber, 1981). Hypnosis is heterogeneous, and there is room for both perspectives. Barber's rapprochement did not, however, mark the end of the dispute. Rather, it became more heated, as the lance of skeptical opposition was assumed by his colleague, Nicholas Spanos.

Spanos. Spanos (1986, 1991) attacked what he called the neodissociative "special state" theory on three levels. First, he continued Barber's methodological challenge through a compre-

hensive research program aimed at demonstrating that any hypnotic effect can be produced in nonhypnotic control conditions—if the situational factors are properly arranged. Spanos used this methodological strategy to investigate many hypnotic behaviors, including amnesia, analgesia, nonvolition, and the hidden observer, as well as individual differences in responsivity. The results of his research, he argued, demonstrate that hypnotic effects in experimental contexts (and hence, others as well) are entirely the product of the social psychological factors that constitute the context.

Second, like Barber, Spanos contested the theoretical legitimacy of the neodissociative approach, and offered a theory that he labeled, at various times, cognitive-behavioral, social-psychological, sociocognitive and cognitive-social-psychological. The collection of labels signals that the scope of explanations embraces the ever-widening corpus of psychological approaches used to explain behaviors in other, everyday, "nonspecial" contexts. Spanos asserted that hypnosis is the product of goal-directed striving. Humans are inherently active and behavior, hypnotic or otherwise, reflects decisive efforts to solve problems and overcome obstacles to achieve desired goals. The power of hypnosis resides in the abilities, motivation, and commitment of subjects to meet the goals inherent in the contextual expectancies of hypnosis. This motivational transparency to achieve desired goals is analogous to that proposed by Hull and Barber. Spanos' explanation of hypnosis, then, addresses the social-contextual cues (e.g., wording of requests), role expectancies (e.g., as someone to be hypnotized) and cognitive functioning (e.g., attentional focus; attributions of agency). He explicitly rejected the cognitive explanations of the neodissociative approach because, Spanos believed, they presume a mechanistic metaphysics of barriers, unconscious streams, and dissociation (Spanos & Chaves, 1991).

Despite the apparent continuity with Barber, fundamental alternations in the argument are introduced. Spanos (1986) moved Barber's quotes from "hypnosis" to Hilgard's explanatory constructs, "dissociations among cognitive systems" and "amnesic barriers" (p. 449). This subtle change has profound ontological implications. The "liberation" of hypnosis tacitly acknowledges that its ontological status is no

longer contested. Skepticism is now exclusively focused on the validity of the neodissociative explanation that presumes a special state. This most important of concessions, that hypnosis is, itself, a valid construct, is never directly addressed. Indeed, he continued vigorously arguing against the “special state” theories using the same methodological strategies used to challenge the construct validity of hypnosis. Now, however, the sole aim of this strategy was to discredit the explanatory validity of the neodissociative approach. The presumed and oft-stated continuity of the skeptical argument, from early Barber to later Spanos, is one of form. The most ontologically important feature of the argument—skepticism about the reality of hypnosis—is silently dropped.

This silence is most unfortunate, not only for the confusion created by the unacknowledged shift in the locus of the argument, but for the failure to identify the criteria that now give legitimacy to the construct, hypnosis. Are these criteria empirical? Spanos employed the same methodological strategies, and asserted that the results demonstrate no “residual” hypnotic effect beyond social-situational factors, so it is difficult to intuit how this evidential base, previously used to discredit the validity of “hypnosis,” could now be used to support it. Are the criteria theoretical? Spanos argued for the continuity of the social-cognitive approach, so it is unclear how theoretical continuity could result in ontological discontinuity. Spanos’ theory did, however, change; not in the focus on social situational factors, but on how these factors were positioned within a broader philosophical context. It is here that we must look for the most likely reason why hypnosis was accorded legitimacy. This is also the third level of Spanos argument: epistemology.

Spanos and his colleagues argued that hypnosis is not a “thing” like smallpox or gravity, but a socially constituted entity like table manners or law; phenomena resulting entirely from social expectancies, cultural practices and interpersonal agreement, enacted as contextual supported goal directed actions (Spanos & Chaves, 1991; Spanos & Gottlieb, 1979). Hypnosis is not an independent entity residing within individuals and evoked by mysterious incantations. Nor is it a singular phenomenon underlying whirling dervishes, dancing Balinese, and animal magnetism. It is, rather, an experience

whose ontological home resides in the social-interactive context. Hypnosis is “called into existence” by both the participant and hypnotist, who share a cultural background of knowledge about hypnosis; about what types of behaviors, experiences, roles, relationships, and outcomes are expected. This shared background has been shaped by the history that has given rise to hypnosis as a socially constituted entity. Spanos contrasted this social constructivistic approach with what he called the scientifically discredited mechanistic essentialism of the neodissociative approach, with its postulation of a will-of-the-wisp intrapsychic essence (Spanos & Coe, 1992). Although never explicitly stated, the shift in quotes by Spanos from “hypnosis” to “dissociative cognitive structures,” whereby ontological skepticism is transferred from the construct to the explanatory framework, derives, not from new evidence, but from an epistemological sea change.

Spanos’ constructivistic epistemology could not be more different than Barber’s early neobehavioral reductionism. Barber aimed to trim excess meaning and banish “hypnosis” as a faux construct. Spanos, in contrast, highlighted it as a noteworthy exemplar of a histo-cultural constructivistic process that yields psychosocial constructs that govern behavior. Although not directly addressed, “constructs,” for Spanos, now meant something more than a methodological term situated somewhere between a variable and a theory. Constructs now entailed an epistemology of socially constructed kinds, like hypnosis, that are constitutionally different than natural kinds found in physics, like magnetism. The construct-validation procedures of Barber are no longer employed by Spanos to challenge the ontological significance of hypnosis, which is now presumed, but to discredit what he argued are the unscientific explanations of the neodissociative approach. It is ironic that Spanos, self-professed heir to Barber’s austere behavioral positivism, should embrace a social constructivistic epistemology, a viable but not exclusive or even dominant philosophy of science, and attack as unscientific what he called the mechanistic positivism of neodissociative cognitive structures. And this, when an emerging cognitive science was embracing theory and constructs most congruent with Hilgard’s approach.

The shifting epistemological ground of the debate suggests a host of new, difficult philosophical questions for Spanos and his colleagues: Are the explanatory constructs of roles, expectancies and attributions more ontologically fundamental than hypnosis and table manners? Can we assert that roles, expectancies, and attributions are more scientifically valid than dissociative systems of cognition? By what criteria? What is the causal meaning, if any, of the statistical regularities found between social contextual factors and outcome behaviors, especially if we assert that individuals are purposeful, active agents? Are these explanations, and the processes of establishing validity, themselves a product of sociocultural historical construction? Does psychological science unearth bedrock causal understanding of human life, or is it a form of history taking (e.g., Gergen, 1973)? Unfortunately, these questions largely have been overlooked, while vigorous empirical skirmishing continued, apparently presuming that all questions are ultimately matters of evidence. Examination of responsivity to waking suggestion and nonvolition are two important disputed issues that offer insight into the types of cul-de-sacs that arise from this presumption.

Disputes. Both Hilgard and Spanos agreed that individuals demonstrate high levels of responsivity in nonhypnotic waking control conditions and, also, that responsivity increases in a hypnotic context. Hilgard (1977) reported evidence from a study demonstrating that when individuals are given instructions for eye fixation without labeling the situation as hypnosis, they were less responsive than when identical procedures were used and labeled hypnosis. He argued that the difference is due to the expectations of hypnosis that alter the background for receiving suggestions, resulting in a special state. Spanos (1991) agreed that the social expectations prompt the increase in responsivity, but that the causal nexus resides in the expectancy qua expectancy, not in a special state, which if reported by subjects, is merely an epiphenomenon of the causative social factors. The dispute, then, is about interpretation, not evidence. It also reveals that the prototypical methodological strategy of the social-nonstate skeptics, initially used to challenge construct validity, cannot provide decisive evidence to resolve the dispute about ontology.

The neodissociative advocates argue that one consequence of hypnosis as a special state is that there are fundamental changes in the way information is processed, and one of the hallmarks of hypnosis that graphically demonstrates these changes is the experience of nonvolition. The neodissociative position is that nonvolition results from two dissociated streams of thought; a barriered, nonconscious stream initiating and executing the actions, the other an “uninformed spectator” to the actions produced. Spanos and his colleagues (Spanos & Coe, 1992) did not dispute the report of nonvolition, but offer an alternative explanation:

Subjects tend to interpret their suggested responses as involuntary when they succeed in deflecting attention away from cues associated with the idea of voluntary arm movement, and instead attend to situational cues (e.g., suggestion wording) and imaginal cues that are consistent with an involuntariness interpretation. p. 105).

This explanation avoids reference to unconscious process and dissociated streams of consciousness, but the proffered alternative is structurally very similar to the neodissociative approach without the clarity. Who, exactly, deflects the attention? How can it be the same “one” as that who is unaware of having initiated the action? A hierarchy of cognitive controls is presumed, as is a complex relation among attention, cues of agency, attentional processes, interpretations of actions, and verbal reports. The explanation is not better situated within established psychological theory, as no detailed theory of cognition functioning is given. Rather, the explanation appears ad hoc, utilizing common terms employed in cognitive psychology, sans a theoretical framework, with the purpose of “saving” the basic contention that hypnosis resides, ontologically, in the social situational context. Thus, the evidence of nonvolition is the basis for theory (re)construction, not theory refutation.

The dispute between the two approaches exposed the limitations of each. The neodissociative focus on intrapsychic processes at the expense of interpersonal factors is as one legged as the sociocognitive emphasis on situational factors that employs ad hoc cognitive explanations. Subsequent theory and research have moved toward a closer integration. Revisions to Hilgard’s theory have provided more explicit linkages between social cues and dissociated cog-

nitive processes (e.g., Bowers, 1992), and socio-cognitive theory has incorporated sophisticated, research-based models of cognitive functioning that embrace many of the features found in Hilgard's theory (e.g., Kirsch & Lynn, 1997). Furthermore, the two factor rapprochement proposed by Barber has received empirical support, suggesting that situational factors influence performance on easier items, such as eye closure; enhance the procedure, provide training, performance improves. However, for more challenging items, such as posthypnotic amnesia, performance may be tied to individual differences that are impervious to the influence of contextual factors (Balthazard & Woody, 1989). The dichotomous positions have become more nuanced, the lines of dispute more blurred, and the points of agreement more clearly developed (Kirsch & Lynn, 1995). Nevertheless, the fault lines between the two positive continue to frame theory and research.

Whence the Future?

The emergence of both the neodissociative and sociocognitive positions was a product of a new, post-World War II zeitgeist. Although the outlines of the positions can be discerned in late 19th-century theories, the new methods and measures produced more conclusive evidence, new types of disagreement (e.g., methodological), and novel twists to old conflicts. The dispute has been conducted across a changing landscape of legitimate psychological theory, absorbing relevant pieces as it progressed. Some are now calling for a "spring cleaning" and for moving beyond what is increasingly viewed as sterile debate (e.g., Gruzelier, 2000; Lawrence, 1997). The dispute is kept alive, in part, by the assumption that all scientific advancement results from rounds of evidential point-counterpoint, methodological adjustments, and theoretical refinements. The sterility of the current debate, however, does not lie in the inability to generate data, but the impossibility of empirical resolution. Reconceptualization, not redoubled data collection, is required.

Calling for reconceptualization is easy; producing it is difficult. Hull, for example, whose work represented the pinnacle of psychological science of his era, could not conceive nor would he approve of the changes that swept the discipline in the decades succeeding his triumph.

What prompted reconceptualization was not more data, but a shift in the foundations of how scientific data are conceptualized. The disputes between Hull and his contemporaries were not resolved. They were abandoned. Hints abound that the discipline of psychology is now, again, in the early stages of another transformational epoch; one that will alter the investigation of hypnosis, and other psychological phenomenon, as profoundly—or more so—than the previous one.

The dispute about the nature of hypnosis at the end of the 20th century is made possible by the development of increasingly sophisticated methodologies for the investigation of psychosocial phenomena. Scientific credence is given to complex psychosocial factors that are free from the complications and entanglements of biological speculations and ontologies that burdened, and ultimately broke, medically based explanations of hypnosis in the late 19th century. Mind need not be tethered, methodologically or theoretically, to the body. This era, however, is ending. Breakthroughs in microbiology and brain imaging technologies allow for linking genes and behavior, psychological stress and immunological functioning, psychiatric disorders and psychotropic medication, and psychosocial behaviors and physiological brain states. Mind and body, psychology and medicine, are now being rejoined.

Neuroimaging research is flooding hypnosis journals, repositioning old arguments within a new evidential context. Advocates of the neodissociative position claim that unique neurobiological characteristics of hypnosis have been identified, including neurophysiological features of attention in hypnosis that differentiate it from more mundane but related activities (such as deep relaxation and absorption in a story), specific disruptions in prefrontal cortical functioning reflecting dissociated cognitive processes, and temporal progression of neurobiological transformations in hypnosis that would correspond to automaticity and involuntariness (Gruzelier, 1998, 2000). This is challenged by sociocognitive advocates who argue that the identified brain states are prompted by response to the unique expectancies and demand characteristics of the hypnotic situation. They assert that the evidence only establishes the existence of particular brain states during hypnosis, not that the states played a causal role (Kirsch,

2000; Kirsch & Lynn, 2006; Wagstaff, 2000). These critics admit to dissociative-like cognitive processes, acknowledge neurobiological evidence of brain states that correspond to altered cognitive functioning, offer few proposals themselves for critical brain-state evidence to support their position, and propose ever more stringent demands for controls conditions (that some argue are impossible) (Gruzelier, 1998, 2000; Hasegawa & Jamieson, 2002; Kirsch, 2000; Kirsch & Lynn, 2006; Wagstaff, 1998, 2000). This may simply be part of the refinements in theory and method that occurs as science advances. These arguments, however, are framed within a problematic scientific logic that has bedeviled disputes about hypnosis for the last half century: If experimental and control groups do not differ, then this constitutes evidence that hypnosis is not unique (e.g., the null hypothesis). If differences are discovered, they result from the differing instructions given to the experimental and control groups—it is not parsimonious to make causal attributions beyond the experimental manipulations used to create the differing groups. Checkmate.

This dispute cannot be adjudicated by evidence alone. It is fundamentally a philosophical debate. Indeed, these new empirical possibilities evoke old philosophical quandaries about the nature of consciousness and the mind–body problem. Consciousness and mind were banished from the scientific lexicon for most of the last century; metaphysical excess leading to philosophical wormholes. No more. Bioscientific advances at the interface of mind and body engender and demand attention to these vexing questions. Philosophers, along with psychologists, biologists, physicians, and physicists, are now members of several interdisciplinary teams at major research centers studying brain functioning and consciousness. A host of philosophical approaches have arisen hand-in-glove with advances in the brain sciences, some offered by philosophers informed by neuroscience (e.g., Churchland, 2002), others by neuroscientists informed by philosophy (e.g., Koch, 2004). Philosophical considerations have become integral to theory and research in brain science.

The new methods, theories, and data of behavioral neuroscience are changing psychological science. They also entail profound philosophical challenges requiring careful, explicit attention: How are natural kinds (body) and

constructed kinds (mind) related? Are natural kinds the ultimate ground of explanation? Are constructed kinds mere epiphenomenon? Is influence top down, bottom up, or both? What determines which and when? How are the various levels of organization (biochemical, genetic, neurological, psychological, behavioral, social, cultural, historical) related? As the debate about hypnosis reveals, theoretical disputes that appear to be simple empirical matters often entail hidden philosophical issues, and apparent continuity at the theoretical level can belie discontinuity at the ontological level. The lesson of the history of hypnosis is clear: Beware of the allure of data as the sole source of scientific understanding and progress. Evidence and ontology are entangled; philosophical analysis is as important as methodological rigor.

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