

# Clark L. Hull, Hypnotist

## Hypnosis and Suggestibility: An Experimental Approach

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Review by John F. Kihlstrom

Clark L. Hull is known to most psychologists for his enormously influential theory of learning. But in the 1920s and early 1930s, long before he promulgated the formula  $S_E R = S_H R \times D$ , Hull embarked on a systematic program of experimental research on hypnosis. Many of the individual studies, chiefly conducted by undergraduates at the University of Wisconsin for their senior theses, were published in scholarly journals—more than 30 of them—in most cases with the student as the sole author (an act of generosity few of us would emulate today). In 1933, Hull published a comprehensive account of the entire project in the prestigious *Century Psychology Series*. Notwithstanding its tables, graphs, and high-level scientific vocabulary, the monograph was “intended to make this body of experimental material available to the general public” (p. ix). In fact, the book was very well received, and remained in print long after Hull’s death, and has since inspired several generations of hypnosis researchers. Now it has been reissued in a handsome new edition with an introduction by Michael D. Yapko, a distinguished hypnotherapist.

Exactly how Hull became interested in hypnosis is not clear (Hull, 1952; Kimble, 1991). His graduate research on concept learning had proved that a rigorously controlled, quantitative science could be made out of the study of thought processes, just as Fechner and Ebbinghaus had done in the 19th century for sensation and memory. But the field was not ready for it, and the work fell flat, not to be revived until the cognitive revolution after World War II. Hull also did research on the effects of

tobacco smoke on human performance—in the process, inventing the active placebo control. Hull then turned his attention to aptitude testing, but the work quickly bored him, though not before he invented a machine for calculating correlation coefficients from data coded on punched paper tape that is on display in the Smithsonian Institution. Nothing in any of this work betrays any interest in hypnosis.

Hull might have been introduced to hypnosis by Joseph Jastrow, first his graduate mentor and then department chair, who had included the topic in his book on unconscious mental life. But in fact, Jastrow never demonstrated hypnosis to him, and Hull had never observed hypnosis at all until a student asked him to try it to rid the student of a phobia (Hull, 1952). Swinging a “hypnotic crystal,” which he had received as a gift from a student, and reading instructions from a book, Hull succeeded. Hull included hypnosis in his lectures to Wisconsin undergraduates and medical students as early as 1921 (Triplet, 1982), and he ran a graduate seminar on the topic as well: One of his early students was Milton Erickson, an undergraduate at University of Wisconsin who later took medical training and became famous as a hypnotherapist. But there is no reference to hypnosis in the published excerpts from the “idea books” that Hull had kept since his college years (Hull, 1962) until early (probably January) 1927, when he listed “a volume on hypnosis” (p. 822) as his top-priority project; and not until January 1928 did he propose an “intensive series of investigations on hypnotic phenomena” (p. 824) to be carried out over the next two years, pointing toward

completion of the hypnosis book in 1929.

The answer to the question of origins appears to be, quite simply, that Hull was intrigued by hypnosis, just as James and Pavlov had been, and he thought it would serve as a kind of proving ground for the new experimental psychology that he and his fellow behaviorists were trying to promote. If scientific psychology were worth its salt,

he seems to have thought it ought to be able to tell us something about interesting “higher mental processes” (Hull, 1962, p. 814), like categorization and hypnosis. Moreover, Hull seems to have shared James’s view that hypnosis was a good vehicle for the study of volition and the way that ideas, in the form of suggestions, were translated into action.

How well does Hull’s research hold up? In fact, few of his

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findings would fail to be confirmed today. Hull construed hypnosis as closely related to suggestibility, and that indeed is where all the action in hypnosis seems to lie. So-called "neutral" hypnosis, in the absence of suggestions for particular imaginative experiences, does not amount to much. But not all forms of suggestibility are the same. Hull showed that hypnotic susceptibility was correlated with direct or prestige suggestion, of the sort measured by the postural-sway test, but not with indirect suggestion, of the sort measured by the progressive-weights illusion. He found a small sex difference in hypnotizability, as well as the usual sort of developmental curve. Correlations between hypnosis and intelligence and other standard personality traits were essentially nonexistent, and those between mental illness and juvenile delinquency were negative. This is still pretty much the situation today, except that we now know that hypnotizability is marginally correlated with absorption, itself a facet of the dimension of openness to experience in the "Big Five" structure of personality. Perhaps in anticipation of his later learning theory, and the postulate that  $sH_R = 1 - 10^{a_N}$ , Hull argued that hypnotic suggestibility was a habit that improved asymptotically with practice. Today we would ask whether it is hypnotizability that increases with time, or just the speed with which people respond to a hypnotic induction procedure.

Much of Hull's book is devoted to examining the effects of hypnosis on various aspects of sensory and cognitive functions, including claims concerning the transcendence of "normal" voluntary capacities. He showed clearly that the induction of hypnosis alone had no effects on muscular strength or resistance to fatigue. However, he did believe that these aspects of human performance might be enhanced through special suggestions delivered in hypnosis. Hypnosis did not enhance sensory acuity—though participants believed it did. Moreover, although Hull showed clearly that hypnosis had no effect on learning or on the recovery of forgot-

ten memories from the recent past, he believed that hypnotic suggestions for hypermnesia might revive remote memories, such as those from early childhood. We now know that hypnotic hypermnesia is as illusory as hypnotic hyperesthesia, that the appearance of hypnotic performance enhancement was largely an artifact of subjects' holding back on nonhypnotic control tests, and that the appearance of hypermnesia is largely an artifact of subjects' lowering their response criteria. On the other hand, Hull's experiments convinced him that hypnotic suggestions for analgesia and anesthesia led to genuine reductions in tactile sensitivity and felt pain. He also discovered that suggestions for posthypnotic amnesia impaired recall but had little effect on alternative memory measures, such as savings in relearning and retroactive inhibition.

Beyond exploring particular aspects of the phenomenon, such as suggestibility and amnesia, Hull also critiqued extant theories of hypnosis and offered a general theory of his own. He thought that the findings of spared retroactive interference in posthypnotic amnesia disproved Janet's theory of dissociation. And even without benefit of the electroencephalogram, Hull concluded correctly that hypnosis was a state of cortical inhibition resembling sleep. For his own part, Hull thought that hypnotic relaxation suppressed subjects' internal symbolic processes, leaving them more open to the hypnotist's suggestions. In Hull's view, the induction of hypnosis markedly enhanced response to suggestion, over levels observed in the normal "waking" state. These conclusions were controversial then and remain so today. We now know, for example, that relaxation is not critical to hypnosis. But Hull was clear that the differences between hypnotic and nonhypnotic suggestibility were only quantitative, not qualitative, and nothing was accomplished in hypnosis that could also not be done, to some degree at least, in the normal waking state. Because of this continuity between hypnotic

and nonhypnotic suggestion, and because of the prominent role of individual differences in hypnotic suggestibility, Hull believed that the effects of hypnosis itself had been greatly exaggerated by his more clinically inclined predecessors.

In all of this, what is missing? First of all, there are no studies of any clinical applications of hypnosis. It is not as if Hull was uninterested in clinical application. Hull believed that suggestion played a prominent role in medicine, and he was impressed by evidence of suggested "psychosomatic" effects on bodily processes. He also believed that his experimental studies supported the use of hypnosis in surgery, despite the popularity of ether and other chemical anesthetics. Hull certainly believed that his studies would contribute to the appropriate and effective use of hypnosis by practitioners. But, ardent proponent of pure science that he was, Hull's primary research interests lay in the laboratory, not the clinic.

Hull viewed hypnosis as a phenomenon of prestige suggestion, but his book betrays little interest in the relationship between hypnotist and subject, or in the wider sociocultural matrix in which hypnotic interactions take place. This may seem strange in someone who was later to become a leading figure of Yale University's Institute of Human Relations, a pioneering attempt to foster interdisciplinary social science. Then again, Yale hired Hull as a statistician, not as a social psychologist. And the fact of the matter is that social psychology was so poorly developed in the late 1920s and early 1930s that many introductory textbooks of the time did not even contain a separate chapter on the subject. Now we understand hypnosis as an interpersonal phenomenon that transpires between the hypnotist and the subject, as well as an altered state of consciousness that occurs in the mind of the individual, and that expectations and interpersonal influence play a significant role. But when he performed the bulk of his experiments Hull had no way of thinking about hypnosis except as a phenomenon of individual behavior.

Hull recognized that there were wide individual differences in hypnotizability, but his procedures for measuring them were somewhat impoverished. He did not invent the postural sway test for measuring suggestibility, but he did tend to rely on this single test for selecting subjects for his experiments (Schneck, 1972; Weitzenhoffer, 1972), and he even developed a mechanical device for objectively recording the degree of response. Today we recognize that proper assessment requires sampling from a wider domain of suggestions, including cognitive as well as motor items and, in each domain, items involving both positive and negative suggestions. Multi-item hypnotizability scales were introduced only after Hull had completed the major portion of his research program, culminating in the "Stanford" and "Harvard" scales that are the gold standard for hypnosis research today. It is interesting that the first of these scales was developed by Davis and Husband at Wisconsin, but apparently independently of Hull's group. Hull commented favorably on the Davis-Husband scale in his book, but it appeared too late for him to use it in his own research. Still, it is not clear that any of his experimental findings would be changed if he had.

Finally, there is little inquiry into the subjective experience of hypnotic subjects, as they respond to the hypnotist's suggestions. Today we understand that subjective experience is central to hypnosis. The point of hypnotic age regression is not to behave like a child; it is to feel like a child. Hull's preference for behavior over self-report was mandated by his goal of making psychology an objective science, but it led him to misinterpret some of his findings. He thought he had disproved Janet's dissociation theory of hypnosis by showing that posthypnotic amnesia did not affect retroactive inhibition (some investigators hold similar views today). When he found that posthypnotic amnesia did not reduce practice effects or savings in relearning, he concluded merely that the forgetting was "by no means complete" (Hull, 1933,

p. 138). And when he observed that hypnotic analgesia and anesthesia did not eliminate reflexive responses to stimuli, he concluded simply that the suggestions affected voluntary but not involuntary behavior. Now we understand that dissociation is not about interference between divided streams of consciousness, but rather about awareness of the contents of the dissociated stream. And we understand that retroactive inhibition, practice, and savings effects reflect a dissociation between explicit (conscious) and implicit (or unconscious) memory of the prior learning experience, just as psychophysiological variables such as heart-rate variability and galvanic skin response reflect the dissociation between explicit and implicit perception of the pain stimulus.

Hull was a methodological behaviorist, but he was not a radical like Watson or Skinner, who sought to abolish mentalistic thinking from psychology. For Hull, the task of psychology was not merely to describe behavior, but rather to describe the internal mental processes that caused voluntary behavior to occur. Volition, or will, was central to his view of behavior, and suggestion—hypnotic or not—was central to his analysis of volition. Hull embraced William James's theory of ideomotor action, and like James he thought that hypnosis was a particularly good place to study how ideas were translated into action. In hypnosis, Hull could trace the source of these ideas to the hypnotist's suggestions, which were then taken up as autosuggestions, but he is clear that the critical processes are internal, rather than external. As Triplet (1982) has noted, the theme of ideomotor action was carried into Hull's later theory of learning, which was really intended to be a theory of complex behavior of all sorts and which Hull was already developing before he finished the hypnosis book. Moreover, Williams (1953) cites a comment by the British sexologist Havelock Ellis, to the effect that someone should do for psychoanalysis what Hull had done for hypnosis, as the genesis of the inter-

disciplinary investigation of psychoanalysis pursued at the Institute for Human Relations that led to the development of social learning theory. Rather than being a topic Hull stumbled on for want of anything else to do, and later abandoned for the greener and safer pastures of learning theory, in this view the work on hypnosis was just one step on the way to a general theory of complex human behavior—a journey that began with the work on concept formation and ended with the theory of habit and drive.

Hull was not the first to do experiments on hypnosis. William James, who generally loathed experimentation, loved to tinker with hypnosis, and James's colleagues at Harvard, Morton Prince and Boris Sidis, reported some interesting findings around the turn of the century. More to the point, P. C. Young completed an extensive series of experimental investigations for his 1923 doctoral dissertation at Harvard. Still, Hull's research was the first to use control groups, and it set the standard for future experimental work. Hull abandoned hypnosis research, but not before outlining 40 experiments on nonhypnotic suggestibility (Hull, 1929), and another 102 experiments on hypnosis (Hull, 1930a, b), for others to pursue. Hull had actually debated whether the latter paper should include 100 research projects or merely 99 (Hull, 1962, p. 823). In the time since, some of the studies Hull proposed have actually been carried out, not always with attribution; many of the others remain as interesting today as they were more than 70 years ago. Hull was correct to write that "the possibilities of research in hypnotism not only are not exhausted but that, comparatively speaking, they have as yet scarcely been touched" (Hull, 1930a, p. 200).

With so much left to do, why did Hull give up hypnosis? For one thing, some authorities at Yale's medical school were concerned about the dangers of hypnosis research (concerns we now know were invalid) and put severe restrictions on Hull's ability to recruit new subjects in New

Haven to replace the ones he left behind in Madison. The situation was worsened when one of Hull's subjects, described as mentally disturbed by Williams (1953), claimed to have been harmed in an experiment on hypnosis and memory. The resulting "stigma" was so serious that Hull regretted having brought the project to Yale to begin with: "I should have dropped it on leaving Madison, and never breathed a word of its existence on coming to Yale" (Hull, 1962, p. 852). The writing also went badly. Hull had originally intended to publish a 200-page book in 1929, but in 1931 he referred to "the terrible chapter on waking suggestion" (Hull, 1962, p. 848) and in 1933 to "the terrible book on hypnosis and suggestibility" (Hull, 1962, p. 849), which eventually contained more than 400 pages. Throughout this long period of gestation, Hull fretted that a rival might claim credit for "the new birth of scientific hypnosis" (Hull, 1962, p. 831; the competitor's name was unfortunately redacted from the published version of the idea books, but was possibly W. R. Wells, P. C. Young, George Estabrooks, or even Hull's former student, Milton Erickson; in any event, none of them produced a book to rival his). Hull's final assessment of the book is a mix of pride and regret:

I believe that it is an important contribution, that it may mark the beginning of a new epoch in that form of experimentation, and that it will be read and quoted for a long time, possibly a hundred years. At all events it probably will be read after the work of

those here at Yale who have thrown obstacles in the way of the experimental work upon which it is based, has long been forgotten. But even if all this should take place, I have paid a high price and would hardly do it again. (Hull, 1962, p. 852)

Whatever his troubles and regrets, though, Hull more than anyone else legitimized hypnosis as a subject for scientific research, and by extension as a technique for clinical application. Hull had ambitions to found a new school of psychology (Hull, 1962, p. 823), and to a considerable extent he succeeded, spawning generations of "neoHullian" and social learning theorists, including John Dollard, Neal Miller, Hobart Mowrer, Kenneth Spence, Abraham Amsel, and Frank Logan. Nevertheless, his formal theory of learning—weighed down as it was with all those postulates and formulas—fared poorly in the competition with Skinner's radical behaviorism, and it did not long outlive its author. By contrast, Hull's hypnosis book is still going strong, more than 70 years after its publication—something that a new generation of hypnosis researchers, like the several that went before, can read with awe and respect. □

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