

HYPNOSIS: A SESQUICENTENNIAL ESSAY¹

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Abstract: The present paper views Coe's (1992) reflections on the socio-political interests in clinical and experimental hypnosis against the background of Braid's *Neurypnology* of 1843. Topics considered are: the significance of the label "hypnosis"; the controversy over state; the tension between credulity and skepticism; the problem of dissociation and automaticity; current theoretical conflicts; and the relationships between practitioners and researchers.

"Where be these enemies? Capulet, Montague,
See what scourge is laid upon your hate,
That heaven finds means to kill your joys with love.
And I, for winking at your discords, too
Have lost a brace of kinsmen. All are punished.

A glooming peace this morning with it brings;
The sun, for sorrow, will not show his head.
Go hence to have more talk of these sad things.
Some shall be pardoned, and some punished;
For never was a story of more woe
Than this of Juliet and her Romeo [Escalus,
Prince of Verona, in Shakespeare's *Romeo and Juliet*]."

"Hypnosis: Wherefore Art Thou?" asks Coe (1992), in a provocative article echoing Juliet's plaintive cry and focusing our attention on the problems of identity and reference in hypnosis. Coe's paper is especially timely, as it is to appear in late 1992, on the eve of the 150th anniversary of the publication of Braid's (1843) *Neurypnology; or the Rationale of nervous sleep, considered in relation with animal magnetism, illustrated by numerous cases of its successful application in the relief and cure of disease* — the book that literally gave hypnosis its name, liberated the field from its roots in animal magnetism and mesmerism, and inaugurated the modern era of clinical and experimental hypnosis (for brief histories of hypnosis, see Pattie, 1967; Sarbin, 1963; Sheehan & Perry, 1977; Shor, 1979; for extensive extracts from Braid's work, see Tinterow, 1970; Waite, 1960).

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THE NAME OF THE THING

Braid (1843) knew that names matter. In the Genesis story, Adam and Eve received the right to name the animals; this may be regarded as a symbol of human power and dominion over nature (responsibility for stewardship apparently came later, after The Flood). So, too, by bestowing a new name on certain phenomena he encountered in his studies of animal magnetism and mesmerism, Braid hoped both to delineate a special domain of phenomena, communicate his understanding of their nature, to alter the perception of them by his colleagues and the public at large, and basically control the reception of his work. Thus, coining the term "hypnosis" was both a scientific and a political act.

It is useful to review just how Braid defined hypnosis, because in his work we can find the source of many controversies that remain current today. He wrote: (Braid, 1843, p. 94; see also Tinterow, 1970; emphasis in original):

By the term "Neuro-Hypnotism," then, is to be understood "nervous sleep;" [defined earlier as "a peculiar condition of the nervous system, induced by a fixed and abstracted attention of the mental and visual eye, on one object, not of an exciting nature"] and, for the sake of brevity, suppressing the prefix "neuro," by the terms —

HYPNOTIC, will be understood The state or condition of *nervous* sleep.

HYPNOTIZE, To induce *nervous* sleep.

HYPNOTIZED, One who has been put into the state of *nervous* sleep.

HYPNOTISM, *Nervous* sleep.

DEHYPNOTIZE, To restore from the state or condition of *nervous* sleep.

DEHYPNOTIZED, Restored from the state or condition of *nervous* sleep.

and

HYPNOTIST, One who practices Neuro-Hypnotism.

Such terms may not add much to our understanding, but then again the same thing could be said about any verbal label. What does the word "dog" add to our understanding of those highly variable domestic animals closely related to wolves? Words are symbols that stand for things. Braid did not have a particularly clear idea of what he was studying — hence all the attention given to the largely unsatisfactory definitions cited above. He only knew that he had discovered something interesting and useful in his observations of the mesmerist Lafontaine. He also realized, however, especially after his paper on the subject was rejected, that he needed a new label — one that would abolish any mesmeric connotations in his work. Of course he had a theory, too: that prolonged ocular fixation

paralyzed certain nervous centers, and his new name was certainly chosen with that theory in mind.

But mostly, the term "hypnotism" served merely to label a group of phenomena. And so it does today. As Hilgard (1973) notes, the domain of hypnosis refers to "what happens when the hypnotist, with the consent of the subject, attempts to induce hypnosis through conventional procedures [p. 972]." The procedures vary considerably, but they all seem to have in common the focusing of attention. The suggestions vary considerably too, but they all seem to have in common imaginative experiences involving alterations in perception. Biofeedback and sensory deprivation are not hypnosis; one person's suggestion that another go to the opera or vote a straight Democratic ticket is not hypnotic. Other boundaries are less clear perhaps, as between hypnosis and progressive relaxation techniques or daydreaming, but we know from studies of categorization that unclear cases do not threaten the usefulness of a concept. Nobody looks at a coyote or dingo and wants to abandon the concept "dog." You get clarity when you focus on clear cases.

Nor is the concept of hypnosis threatened by individual differences among Ss in response to the hypnotist's suggestion. In fact, as Hilgard (1973) has argued, individual differences in response may be considered part and parcel of the domain of hypnosis: if everybody does it, maybe it is not hypnosis. Another potential delimiter has to do with the particular type of suggestions employed: the quality that Hull (1933) labelled "personal heterosuggestion" distinguishes what goes on in hypnosis from response to the progressive weights illusion, persuasive communication, and placebo tablets. Yet another has to do with phenomenal experience: subjective conviction in the reality of the suggested effects, and especially the experience of involuntariness in response to suggestions, serve to distinguish hypnosis from behavioral compliance; and, in the final analysis, they are what make hypnosis interesting. But more important in the present context, these features give the word "hypnosis" its meaning.

Of course, relatively few Ss who are exposed to hypnotic induction procedures meet all these criteria: a high level of positive response to suggestions, subjective conviction, experienced involuntariness, and the like. Accordingly, it might well be, as he suggests, that "true" hypnosis is vanishingly rare. This was Braid's point of view. In *The Physiology of Fascination and the Critics Criticised* (1855, pp. 370-371, emphasis in original), he writes:

I am well aware that, in correct phraseology, the term *hypnotism* ought to be restricted to the phenomena manifested in patients who actually pass into a state of sleep, and who remember nothing on awaking of what transpired during their sleep. All short of this is mere reverie, or dreaming, however provoked; and it, therefore, seems highly desirable to fix upon a terminology capable of accurately characterizing these latter modifications which result from hypnotic processes Let the

term *hypnotism* be restricted to those cases alone in which, by certain artificial procedures, oblivious sleep takes place, in which the subject has no remembrance on awaking of what occurred during his sleep, but of which he shall have the most perfect recollection on passing into a similar stage of hypnotism thereafter. In this mode, *hypnotism* will comprise only those cases in which what has hitherto been called the double-conscious state occurs; and let the term *hypnotic coma* denote that still *deeper* stage of the sleep in which the patient seems to be quite unconscious at the time of all external impressions, and devoid of voluntary power, and in whom no idea of what had been said or done by others during the said state of *hypnotic coma* can be remembered by the patient on awaking, or at any stage of *subsequent* hypnotic operations⁴

Even if genuine hypnosis were rare, practical and theoretical interest in hypnosis would not be diminished one iota. Schizophrenia affects only about 1% of the population, yet it is a very interesting and important topic for research. There are only a handful of patients with the amnesic syndrome available for study (and one of these, H. M., is absolutely unique in all the world), yet studies of these individuals have yielded great insights into memory. Since well before the publication of Braid's book, and for the 150 years since, what has attracted investigators to hypnosis is the feeling that we might learn something *new* from it.

THE "STATE" BUGABOO

Braid (1843) referred to hypnotism as a state, and a dominant theme in contemporary research on hypnosis is the claim that hypnosis is a special or altered state of consciousness. Indeed, the life of a hypnosis researcher might be made very much easier if hypnosis were a distinct state like sleep or drunkenness — if, for example, there were something physiological like the paralysis of nervous centers or something psychological like trance logic (Orne, 1959; for reviews of subsequent research, see McConkey, Bryant, Bibb, & Kihlstrom, 1991; Spanos, 1986) that could be used to determine whether someone is hypnotized or not. But physiological indices are not necessary for the study of private experience; modern cognitive science has produced a perfectly good psychology of imagination, even though the biological substrates of imagery are not at all clear.

Moreover, the search for singly necessary and jointly sufficient features of hypnosis has not just been futile; it has also been unnecessary. Most natural concepts do not possess such defining features (Smith & Medin, 1981). Accordingly, hypnosis may be construed as "a natural concept represented by a prototype or one or more exemplars consisting of features which are correlated with category membership [Kihlstrom, 1984,

⁴It is worth noting that Braid appears to have been the only investigator to have observed "hypnotic coma," apparently manifested as a lack of responsiveness during hypnosis, as well as a lack of memory afterwards. Braid appears to have been wrong to describe posthypnotic amnesia in terms that we now recognize as a form of state-dependent memory.

p. 151].” Some of these features have to do with induction procedures, such as focusing attention on some object or image; others have to do with overt behavior, such as response to suggestions; others have to do with subjective experience, such as conviction or involuntariness; and others have to do with physiological signs. Orne (1977) has pointed out that defining the presence of hypnosis is like making a diagnosis: no single feature is necessary, but the more characteristic features are present, the more certain we can be.

But what kind of state is hypnosis? Certainly, it is not a state that *causes* events to occur; rather, it is a state *in which* certain events occur — in particular, the kinds of experiences that characterize the domain of hypnosis. As Hilgard (1969) pointed out long ago, it is important to recognize that the hypnotic state is a classificatory label, not a causal explanation. It is a convenient shorthand for a social interaction in which one person, S, experiences subjectively compelling responses to suggestions offered by another person, the hypnotist, for imaginative alterations in perception and memory.

In any event, there should be no embarrassment about using the term “state” in a hypnotic context. According to *Webster’s Third New International Dictionary*, the word “state” means “a mode or condition of being . . . a condition of mind or temperament.” It goes on to note that “STATE, often interchangeable with CONDITION, may but does not always imply genuinely existent characteristics likely to be significant and enduring and discovered or announced after some analysis.” Phrases such as “state of knowledge,” “state of health,” “state of the Union,” and “state of mind” are part and parcel of common parlance. Hypnosis is a state in the same sense that these are states. People like Braid (1843), who thought there was more to the matter, appear to be wrong; but people who use the term in its ordinary-language sense are on solid ground. Like *hypnosis*, *state* is just a word.

If “state” is just a word, possessing no more than its ordinary-language meaning, then what of state reports? What does it mean for Ss to take a 0-10 scale of hypnotic depth and assign themselves a number on it? There is no reason to think that hypnotic depth, as indexed by depth ratings, somehow causes hypnotic responses to occur. That does not mean, however, that depth ratings or state reports have no interest value. Depth ratings have the same status as Ss’ self-ratings on any other psychological dimension — anxiety, depression, intelligence, neuroticism, etc.: they are judgments about themselves. As such, depth ratings provide a useful check on the effects of induction procedures, in the same way that self-ratings of happiness and sadness can serve as manipulation checks in studies of mood; and, as in other domains of social cognition, it is theoretically interesting to know precisely how Ss arrive at these evaluations.

Certainly there is no meter in the head that permits a direct read-out of hypnotic depth. Rather, these judgments are computed by Ss on the

basis of the information available to them, including their response to previous suggestions; this computation, in turn, is also influenced by the sorts of contextual factors discussed by Radtke (1989). The fact that depth ratings are computed, however, rather than read out by Ss does not make them uninteresting. They can have important implications for clinical work: discrepancies between the S's apparent level of response to test suggestions on the one hand, and his or her depth ratings on the other hand, may suggest that he or she is suffering some misconception about hypnosis that should be corrected before treatment proceeds. Hypnotic depth ratings also can have important implications for theory, in so far as they can serve as laboratory models for the study of self-evaluation in general.

CREDULITY AND SKEPTICISM ABOUT HYPNOSIS

The title page of the first edition of Braid's (1843, p. 67; see also Tinterow, 1970) book contains the following quotation from Dugald Stewart (1753-1828), who in turn was referring to Bishop Berkeley:

Unlimited scepticism [sic] is equally the child of imbecility as implicit credulity.

Braid himself started out as a skeptic, dismissing the demonstrations of the mesmerist Lafontaine; but he convinced himself that some phenomena, at least, were genuine when he was able to reproduce them in individuals whom he trusted; and certainly he believed the evidence of the cures he effected by suggestion. Ever since, as Shor (1979) noted, the fundamental problem in hypnosis research has been to navigate between the Scylla of caution and the Charybdis of conviction.

The tension between skepticism and credulity in hypnosis remains high to this day. Sutcliffe (1960, 1961) devoted two classic papers to the problem (without referring to Stewart), contrasting two propositions: (a) that the mental processes instigated by hypnotic suggestion are identical to those instigated by the actual state of affairs suggested by the hypnotist; and (b) that the hypnotized S acts as if the world were as suggested by the hypnotist. Sutcliffe could find only one example of the credulous point of view, but examples of the skeptical point of view abound: hypnosis is always being confused with pretense and fakery. This is the clear message of Meeker and Barber's (1971) analysis of stage hypnosis, and Sarbin and Coe's (1972) recitation of the hypnosis episode from the autobiography of Mark Twain. Wagstaff (1981) maintains that hypnosis is nothing more than compliance; and while Spanos' (1986) position is somewhat more complex than this, his use of the concept of strategic self-presentation certainly lends an air of cunning and deceit to his story of what hypnotic Ss are doing.

Coe and Sarbin (Coe, 1978; Sarbin & Coe, 1972, 1979) have always tried to distinguish between hypnotic role-enactment and mere fakery.

For example, they have argued that people can become so involved in their roles that they lose themselves. Interestingly, Sutcliffe (1960, 1961) himself embraced a similar position: that hypnosis involves a quasi-delusional alteration in self-awareness (Kihlstrom & Hoyt, 1988). The idea is that hypnotized Ss lose or suspend awareness of the actual state of the world affairs, and so believe it is as suggested by the hypnotist. This altered state of consciousness — what else can one call it? — does not prevent information about the actual state of the world from being processed outside of awareness, and thus influencing Ss' behavior. At the same time, however, it also means that Ss are not merely acting *as if* — they believe that the world is as suggested by the hypnotist.

DISSOCIATION AND AUTOMATICITY

This situation, in turn, is exactly the kind of situation that dissociation theory tries to describe. Dissociation theory was tested by White and Shevach (1942) and found wanting, but as Hilgard (1977) and others have pointed out, their experiment actually tested an inappropriate version of dissociation theory. White and Shevach expected that dissociated mental processes would be so isolated from other ongoing mental life that the one stream of consciousness would not interfere with the other. It is not at all clear where they got this idea, since it has no basis in the classic statements of dissociation theory by Janet. For Janet, a dissociated stream of thought is isolated from conscious awareness, and from the phenomenal experience of agency and control, but he never suggested that the dissociation will extend to the matter of interference. Quite the contrary: from Janet's point of view, one of the hallmarks of hysteria was the manner in which dissociated mental contents intruded on conscious experience, thought, and action. These intrusions are a form of interference.

Janet's views aside, our contemporary understanding of cognitive processes gives plenty of reasons to expect that interference will occur between simultaneous streams of information processing, even if one of these streams is dissociated from conscious awareness. For one thing, the two streams may compete for the same output channel. If, for example, S has been given a posthypnotic suggestion to say "February" whenever a cue is given, and then is asked to read a passage aloud, the suggestion will obviously interfere with the execution of the manifest task. Even if the two streams terminate in different output channels, they still draw on a common fund of attentional resource capacity. Thus, as Hoyt (1990) has noted, if two tasks consume attentional resources, then performance of one may very well interfere with performance of the other. Interference will occur if the total requirements of the two tasks exceed available capacity; interference will not occur if the total requirements leave a surplus of capacity (perhaps because one task is innately automatic, or

has been routinized through extensive practice). These are facts about the human information-processing system, and there is no reason to believe that hypnosis per se alters these facts in any way.

Of course, responses to hypnotic suggestions are often accompanied by the experience of automaticity. This phenomenal experience, however, should not be confused with automaticity as a technical concept in cognitive psychology and human information processing. The experience of automaticity, like so much else about hypnosis, is illusory. Shor's (1979) discussion of this point is worth repeating at length:

A hypnotized subject is not a will-less automaton. The hypnotist does not crawl inside a subject's body and take control of his brain and muscles. Motivated behaviors are carried out by the hypnotized subject only because at some level he himself is motivated to carry them out. Although the hypnotic subject may look as if he is no longer in control of his own volitional activities — for example, he may behave as if he is unable to bend his hypnotically stiffened elbow — that is only because at some deeper level than is operative within the boundaries of consciousness, he is actively, deliberately, voluntarily keeping his elbow stiff while simultaneously orchestrating for himself the illusion that he is really trying his best to bend it. In this case the volition that the subject is aware of within consciousness is subordinated to the volition that the subject is unaware of beyond consciousness. That ideas and motivational strivings may be operative beyond the bounds of conscious awareness is not a new idea in psychology [p. 124].

Expressed in terms of Hilgard's neodissociation theory of divided consciousness, we might suggest that when the cognitive control system that executes the response to a hypnotic suggestion is dissociated from conscious awareness, S will experience that response as automatic and nonvolitional. Again, however, that experience is illusory — obviously, there is *some* executive control involved in hypnotic responding, even if the hypnotized S does not experience it as such — and there is every reason to expect that the dissociated response will draw on available cognitive resources, and interfere with other mental activities.

Issues of interference and Janet's intentions aside, it should be noted that the concept of dissociation has a technical meaning in cognitive psychology, and that this meaning is relevant to hypnosis (Dunn & Kirsner, 1988). In cognitive psychology, the term "dissociation" refers simply to a situation where some state of manipulation has an effect on one dependent variable but not a second variable conceptually related to the first. Thus, to cite some familiar examples in cognitive psychology and cognitive neuropsychology, bilateral damage to the hippocampus and associated structures affects recall and recognition but not priming in lexical decision or word-stem completion; similarly, damage to the striate cortex impairs object-identification but not object-location. In addition to these single dissociations, there are also examples of double dissociations (in two forms, crossed and uncrossed), where two variables produce oppo-

site effects on the performance of two different tasks. For example, elaborative activity at the time of encoding has an effect on recall but not priming; on the other hand, a shift in the modality of presentation between study and test has an effect on priming but not recall. Finally, there are also reversed associations, in which there is a positive association between two tasks under one change of experimental conditions, and a reversed association under another change of conditions. For example, a global dyslexic performs poorly while reading nonsense and irregular words, while normals perform well on both tasks (a positive association); and surface dyslexics can read nonsense words but not irregular words, while phonological dyslexics can read irregular but not nonsense words (a negative association).

Dissociations of this form are quite familiar in hypnosis. For example, suggestions for hypnotic analgesia lead to dramatic reductions in perceived pain, but have variable effects on physiological responses to a pain stimulus, depending on the index chosen. As another example, suggestions for posthypnotic amnesia disrupt free recall, a measure of explicit memory, but not associative priming, a measure of implicit memory. In fact, it was the observation of just these sorts of dissociations — between subjective report and other indices of response — that led Sutcliffe (1960, 1961) to devise his third, delusional, alternative to credulity or skepticism. Interestingly, these sorts of dissociations have been observed by investigators from widely different theoretical persuasions (though their interpretations of these observations may differ). So it is possible to talk meaningfully about dissociations in hypnosis, without embracing dissociation (or neodissociation) theory. Still, the observed dissociations must be explained somehow, and the task of explaining dissociations and other phenomena of hypnosis has led to a prolonged conflict between theoretical positions.

It should be noted, however, that this controversy revolves around a quite different use of the term “dissociation” — one that is much closer to Janet’s original *desaggregation*, and having to do with the lack of integration between mental processes, and especially the inaccessibility of certain mental contents or processes to phenomenal awareness. It is this sense of dissociation that has caused all the trouble.

MANAGING AND RESOLVING THEORETICAL CONFLICTS

It seems likely that Braid (1843) coined his new term, “hypnosis,” in order to avoid the still-raging conflict about the nature and effects of animal magnetism. If so, he was not entirely successful, because he was quickly embroiled in disputes with two magnetists, Elliotson and Von Reichenbach, who claimed that he was merely promoting a disguised form of mesmerism. Braid soon revised his own theoretical account, shifting from a physiological theory focusing on the paralysis of nerve centers to a psychological theory emphasizing attention, imagination,

expectation, and personality. Braid died in 1860 without his work having received much recognition in his own country, but the phenomenon was picked up on the Continent by the great French neurologists Azam, Broca, and Charcot, who knew of Faria's work on artificial somnambulism and Liébeault's work with artificial sleep; at this point theoretical controversy about the nature of hypnosis began in earnest.

This is not the place to review the entire history of this controversy; excellent accounts have been provided by Sarbin (1963), Pattie (1967), Sheehan and Perry (1977), and Shor (1979). Coe's (1992) paper gives some flavor of the present state of the field, arraying special state and special process theorists on one side against social-psychological or cognitive-behavioral views on the other. Unfortunately, such a division obscures at least as much as it reveals: there is much heterogeneity within each camp. Thus, some special state and special process theorists think that hypnosis involves dissociative processes, while others think that hypnosis involves regression in the service of the ego; still others invoke cortical inhibition. Similarly, some social-psychological and cognitive-behavioral theorists emphasize a debunking attitude; others, the claim that hypnotic effects could be produced in appropriately motivated and instructed Ss without resorting to hypnotic induction; others, the importance of prescriptive social roles; others, the role of expectancy; and still others, the role of misattribution processes and strategic self-presentation. To make things even more difficult to track, some within the social-psychological, cognitive-behavioral camp move unannounced from one position to another; and some within the special state, special process camp resolutely refuse to offer any theoretical statement at all.

Nevertheless, as scientists we are trained from the time we are sophomores to pick a theory, any theory, generate a hypothesis, and test it. If we are really doing our jobs, we are supposed to test our chosen hypothesis not against the null alternative, but rather against a competing hypothesis derived from a competing theory. Thus, a win for one hypothesis automatically counts as a loss for the other. This strategy of strong inference makes for great public appearances, but it may have limited applicability in our attempt to understand the nature of human experience, thought, and action. Humans are complicated creatures; human mind and behavior may demand correspondingly complex explanations — explanations that invoke both special and normal processes as appropriate. To take a couple of salient examples: theorists of personality and social interaction now agree that both traits and situations are important; even more important (and interesting) is the fact that trait and situation variables interact in generating behavior; and that behavior feeds back to alter both the person who executes it and the situation in which it occurs. In perception, the battle between ecological and constructivist views is being resolved by the recognition that both bottom-up and top-down processes are impor-

tant. In language acquisition, the same resolution is in sight in the dispute between rule-and-representation and connectionist theories.

Why should hypnosis be any different? Hypnosis is simultaneously a social interaction, between *S* and hypnotist interacting in a larger sociocultural context; and a state of cognitive change, involving basic mechanisms of perception, memory, and thought. Thus, any satisfactory theoretical account of hypnosis must invoke both social-psychological and cognitive-psychological constructs. Individual investigators may wish to focus their efforts in one direction or another, as their preferences dictate, but the ultimate goal must be a kind of synthesis out of which comprehensive understanding will emerge.

Such a view was in sight in the later work of Braid (1855), who discussed the role of both expectations and double consciousness. It reappeared in the work of White (1941), who conceptualized hypnosis as a state of both altered consciousness and heightened motivation; of Sutcliffe (1960, 1961), who rejected both credulity and skepticism; of Orne (1959, 1979), who clearly demonstrated the impact of demand characteristics while holding to the belief that there was something else going on as well; of Hilgard (1965, 1977), who wrote about the relevance of dissociation but underscored the importance of *S*'s interaction with the hypnotist; and finally of Sheehan and McConkey (1982), who attempted to balance cognitive changes with contextual background. These theorists do not belong in one camp or the other; most of the rest of us do not, either. As hypnosis researchers, we have exercised considerable theoretical and methodological ingenuity in testing our hypotheses; perhaps we need to exercise the same amount of ingenuity in taking each other's work seriously.

CLINICAL AND EXPERIMENTAL HYPNOSIS

Braid (1843) had a theory of hypnosis, but he was most interested in the great potential of hypnotism as a clinical technique (though he was at pains to reject the notion that it was a panacea). A great deal of Coe's (1992) paper is devoted to the differences that separate clinicians from experimentalists, and indeed there are many. What many clinicians call "hypnosis" is unrecognizable to experimentalists as such; then again, from the point of view of many practitioners, laboratory research is so dry and sterile that it is almost irrelevant to their needs. Many clinical hypnotists make exaggerated claims for hypnosis, either out of ignorance of the experimental literature or out of an attempt to increase the motivation of their patients for treatment. Many experimentalists ignore potentially useful insights from the clinic because they are based on uncontrolled observations and anecdotal evidence.

At the same time, it is important to realize that unlike most other basic research topics in psychology, interest in hypnosis arose directly out of the clinic, and the bonds between experimentalists and clinicians have

always been extraordinarily strong. One of the distinctive features of the hypnosis community, as represented both in its journals and in its professional meetings, is the way clinicians and experimentalists pay attention to each other and try to learn from each other. This experience is not common outside of hypnosis: it regularly occurs in cognitive and clinical neuropsychology, but much of the rest of the field practices a strict separation between laboratory and clinic, between theory and application — much to its detriment. This experience is something for us to cherish, and celebrate, and nurture.

BUT WHO WILL BE OUR DUKE OF VERONA?

We have come a long way since 1842, when Braid's proposed "Practical Essay on the Curative Agency of Neuro-Hypnotism" was rejected by the medical section of the British Association. Hypnosis is a thriving field of inquiry, respectfully presented in textbooks, seriously taught in university departments, frequently used by clinical practitioners, and carefully investigated by laboratory scientists. The *International Journal of Clinical and Experimental Hypnosis* has evolved from the house-organ of a small professional society to one of the field's most highly respected periodicals. Other specialty journals have also established themselves. At the same time, hypnosis research now regularly appears in the foremost psychological journals — not just the *Journal of Abnormal Psychology*, whose door has always been open, but others as well. We have much to be proud of.

But we also have much to worry about, including the sorts of persisting divisions between clinicians and experimentalists, and between different theoretical viewpoints, that are the burden of Coe's (1992) essay. It sometimes seems that 1843, the first time the term "hypnotism" was used, was the last time that the field of hypnosis was united — and then only for a very short time. At the beginning and again at the end of his piece, Coe (1992) invokes *Romeo and Juliet*, and we are reminded that Shakespeare's play is not just a tragedy of two star-crossed lovers: at its core it is also a tragedy of two great houses battling over issues that have long been forgotten. This feud takes one promising young life after another, including some, like Mercutio and Paris, who are neither Capulets nor Montagues; and others, like Romeo and Juliet, who want only to be left alone to pursue their love, unhampered by old allegiances and conflicts; and of course it disrupts the peace and progress of the city of Verona. At the end, the families put aside their differences, but tragedy turns to comedy only at the graveside, after generations of waste. There is a lesson here for us hypnotists, if only we would pay attention and take it to heart.

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Hypnose: Ein hundertfünfzigjähriger Essay

John F. Kihlstrom

Abstrakt: Die vorliegende Abhandlung befaßt sich mit Coes (1992) Äußerungen über die sozial-politischen Interessen in klinischer und experimenteller Hypnose gegenüber dem Hintergrund von Braids *Neurypnology* von 1843. Die erwögenen Themen sind: die Bedeutung der Bezeichnung "Hypnose", die Kontroverse über Zustand, die Spannung zwischen Leichtgläubigkeit und Skeptizismus, das Problem der Dissoziation und des automatischen Zustands, gegenwärtige, theoretische Konflikte und die Beziehungen zwischen Praktikern und Forschern.

Hypnose: Essai cent cinquantaire

John F. Kihlstrom

Résumé: Le présent article contraste les réflexions de Coe (1992) sur les intérêts sociopolitiques en hypnose clinique et expérimentale, avec l'arrière plan provenant de la neurohypnologie (1843) de Braid. Les sujets considérés sont: la signification de l'étiquette "hypnose"; la controverse sur l'état; la tension entre la crédulité et le scepticisme; le problème de la dissociation et de l'automatisme, les conflits théoriques actuels et les relations entre praticiens et chercheurs.

Hipnosis: un ensayo de cientocincuenta años

John F. Kihlstrom

Resumen: Este artículo examina las reflexiones de Coe (1992) respecto de los intereses socio-políticos de la hipnosis clínica y experimental frente a la perspectiva de *Neurypnology* de Braid de 1843. Los tópicos considerados son: la significación del rótulo "hipnosis"; la controversia sobre el estado; la tensión entre credulidad y escepticismo; el problema de disociación y automatismo; los conflictos teóricos corrientes; finalmente, las relaciones entre clínicos e investigadores.