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WHERE ERICKSONIAN LEGEND MEETS SCIENTIFIC METHOD: 
A Comment on Matthews¹

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Milton Erickson was not particularly well-known and exerted little influence during the greater part of his working life. Late in his life, however, the publication of Haley's (1973) Uncommon Therapy gave birth to the Ericksonian legend. Erickson's fame and influence swiftly grew, as Matthews so aptly describes, and now everyone has an opinion about Erickson. Although most of the Ericksonian literature has been laudatory—beatifying, even—there is a range of opinion about him. He has been variously described as a genius (Zeig, 1985) and as a scoundrel (Gibson, 1984). Perhaps the only profound analysis of Erickson's work was done by Ernest Hilgard (1984), who expressed respectful interest in Erickson's clinical skill even as he raised critical questions about Erickson's ideas.

Does "Ericksonian hypnosis" have something uniquely valuable to offer? Let me first lament the term Ericksonian hypnosis. Unfortunately, hypnosis refers both to a set of techniques as well as a psychological condition, and this dual meaning leads to confusion. Ericksonian hypnosis is a particularly vexing case of this potential misunderstanding of terms, because it leads to the widespread misunderstanding that Ericksonian hypnosis refers to a psychological phenomenon different from that denoted by the mere word hypnosis. I have contended previously (Barber, 1988) that this confusion is heightened by a lack of understanding of hypnotic process among most Ericksonians, the consequence of a misguided emphasis on "naturalistic" technique. This emphasis has led to a generation of clinicians largely unfamiliar with classical hypnotic phenomena (e.g., analgesia, amnesia, and automatic response to posthypnotic suggestion, to name the most obvious) and who have, instead, focused narrowly upon the elaboration of technique. Matthews discusses two common examples, indirection and storytelling, and reports that neither of these techniques has any necessary relationship to effective hypnotic treatment.

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agree, and I would further contend that neither is adequate to the facilitation of effective hypnotic induction.

This misemphasis has led, I believe, to the relatively superficial training (hypnotic and otherwise) of many clinicians who call themselves "Ericksonian" and are largely naive about both the work of clinicians other than Erickson and relevant hypnosis research. Furthermore, I think this narrow clinical training lends itself to an uncritical acceptance of Ericksonian lore. As Matthews persuasively demonstrates, most claims for the superiority of the "Ericksonian approach" (a term never used by Erickson, of course) have never been submitted to systematic investigation. However, some have, and it is this line of research that Matthews explores.

I fully agree with Matthews that the underlying Ericksonian assumptions can and ought to be tested, in spite of resistance. Just as early Freudians asserted that psychoanalytic theory was not amenable to scientific investigation, so, too, contemporary Ericksonians have asserted that their methods are not amenable to such scrutiny. (Erickson himself attempted such research. See Haley, 1967.) Although most of us would agree that a meaningful exploration of complex ideas requires great care and subtlety, it can be done. We need only look to cognitive psychology for the illuminating application of increasingly sophisticated methods for examining subtle and complex phenomena—memory, social influence, and dissociation, to name but three examples—as well as to the innovative hypnotic research of McConkey and his colleagues (Barnier & McConkey, 1996, 1997, 1999; Bryant, Barnier, Mallard, & Tibbits, 1999; McConkey, Wende, & Barnier, 1999).

Now, to the three assumptions that Matthews says underlie Ericksonian claims:

Hypnosis is an altered state of consciousness with markers that are distinguishable from the waking state. The state-nonstate controversy has persisted throughout the history of hypnosis, and the assumption that the hypnotic experience involves an alteration in consciousness is not unique to Erickson nor Ericksonians. Ernest Hilgard (1986) has provided us with a lucid theory based upon this premise. In fact, over many years, controversy about this "dissociation" assumption has become one of the enduring debates between scholars in this area. (For a recent discussion, see Kihlstrom, 1998; Kirsch & Lynn, 1998.) I think Matthews's statement of this assumption overreaches, however, when he adds the requirement for physiological "markers that are distinguishable from the waking state" (p. 420). The assumption that there must be physiological markers of an altered state of consciousness may be Matthews's own; I am not familiar with such an assertion from Erickson, his promoters, nor others.

\[3\] I use the term "Ericksonian" to distinguish those who have developed models or hypotheses based on their interpretation of Erickson from the assertions of Erickson, himself, who shunned such model-making. It may prove useful to distinguish Erickson and his claims from those of "Ericksonians."
who find value in a state theory. Moreover, in my view, identifying such markers adds little to our psychological understanding of hypnotic processes.

To support Matthews’s view that there are no markers (and, therefore, no alteration in consciousness), he cites Sarbin and Slagle’s (1979) report of failing to identify a single autonomic variable as an index of a hypnotic state. To be fair, we must concede that there need be no immutable physiological markers of an unusual state (e.g., being “smitten with love” is perhaps a good example). This notwithstanding, there have been some exciting developments in neuro-imaging studies, which examine not the “state” of hypnosis itself but the peculiar patterns of brain response following specific hypnotic suggestions. This fruitful line of research identifies particular brain activity involved in hypnotic processes, thereby yielding evidence of the neuropysiological substrates of hypnotic response (Kiernen, Dane, Phillips, & Price, 1995; Rainville, Carrier, Hofbauer, Bushnell, & Duncan, 1999; Rainville, Duncan, Price, Carrier, & Bushnell, 1997; Rainville, Hofbauer, et al., 1999). These reports reveal that the recent advent of brain imaging techniques, combined with a phenomenological paradigm, may soon reveal brain structures involved with the process of consciousness alteration itself (Rainville, Hofbauer, et al., 1999).

However, I believe that these attempts will not promote our understanding of hypnotic processes. For what will really help us understand the nature of hypnosis is not simply a matter of probing phenomenological reports alone or assaying the neurological substrate alone but examining how both these two processes covary with one another. A coherent psycho-neural model might then emerge (Price, 1996).

I fully agree with Matthews’s position about the unconscious. I have written elsewhere that attribution of some special wisdom to the unconscious is groundless (Barber, 1988). It is worth noting, however, that this issue is an example of the difference between theory and technique. Erickson was antagonistic to theory, referring to it as a “Procrustean bed.” Though he suggested to patients that they could trust the wisdom of their unconscious, he never published theoretical statements about the unconscious, wise or not. Subsequently, as Matthews observes, Ericksonians have transformed these clinical interventions into theoretical models, without benefit of empirical support. If these models are to be taken seriously, they must be empirically tested.

Hypnotizability of the subject/client is primarily a function of the hypnotist’s skill (i.e., utilization strategies) and less a function of the subject/client’s hypnotic ability. Matthews writes, “In the Ericksonian paradigm, essentially all individuals have the ability for hypnotic responding” (p. 421). Again, we ought to distinguish between Erickson and Ericksonians; although this statement is a distortion of Erickson’s position, it may accurately represent the position of Ericksonians. To the contrary, Erickson tended to characterize people as “good subjects” or otherwise, revealing his
recognition of the inherent differences in responsivity among people. (He himself was not a "good subject.") What may contribute to the enduring debate, however, is Erickson’s focus on technique for facilitating response, and these techniques have come to dominate the contemporary Ericksonian culture.

My own and others’ fascination with technique (Barber, 1977, 1980, 1989) was an exuberant reaction to the originality of Erickson’s inductions, compared with the relatively primitive induction techniques taught prior to Erickson’s influence.

Erickson described unusual, creative inductions that he claimed facilitated responsiveness in individuals who were not “good subjects.” There are also witnesses to Erickson’s prowess at accessing hypnotic responsiveness in “difficult” subjects. (See Wolberg’s Foreword in Zeig, 1985.) My earliest hypnosis research was heavily influenced by Erickson, and our pilot study (and subsequent data) led us to believe that an indirect, permissive technique would be more effective in producing hypnotic analgesia (Barber & Mayer, 1977). This leads us to Matthews’s third assumption.

The use of indirect hypnotic suggestion is, at least in some instances, more effective in producing hypnotic responses than is direct suggestion. Matthews includes reports that support his view that this assumption is unsupported (Matthews, Bennett, Bean, & Gallagher, 1985; Matthews & Isenberg, 1992; Matthews & Mosher, 1988) and concludes, with Lynn, Neufeld, and Maré (1993), “that differences between a wide variety of suggestions are either nonexistent or trivial in nature.” However, there is another side to this controversy, and the interested reader may conclude that the data not cited by Matthews yield a less clear picture.

For example, Barber & Mayer (1977) reported a far higher incidence of hypnotic analgesia (in response to indirect suggestion) than would be expected (in response to direct suggestion) from a sample of students (whose unmeasured hypnotic responsiveness can be assumed to be normally distributed). In addition to a clinical report (Barber, 1977), subsequent empirical investigations of the efficacy of indirect suggestion provided further evidence for the hypothesis that responsiveness, though usually stable, may become more or less accessible, depending upon both context and techniques (Alman & Carney, 1981; Diamond, 1984; Fricton & Roth, 1985; Gfeller, Lynn, & Pribble, 1987; Price & Barber, 1987).

My own conclusion, based on reviewing the published data, is that their meaning remains unclear. How much difference, if any, does technique make? I believe the definitive research has yet to be done.

CONCLUSION

Although I enthusiastically share Matthews’s attitude toward empirical support for the assumptions he explores, I prefer a restatement of his
conclusion in order to accommodate the credibility that Ericksonian assumptions have acquired through drama and repetition.

Because these assertions are based only upon impressive and dramatic clinical anecdotes about Erickson’s work, they should be considered to be hypotheses until empirical evidence is found to support them.

“Within the emerging field of consciousness research, there are huge philosophical and scientific doors opening up to the re-discovery of phenomenology and the experiential approach.” (Price, 1999, p. 9) I believe these issues are most likely to be resolved through experiments employing the phenomenological paradigms of Price and Barrell (1990) and Sheehan and McConkey (1982). Until that bright future, Matthews and I will share our pessimistic view of the current lack of rigor of things Ericksonian.

REFERENCES


