Dr. Lynn (Lynn, 2001) has proposed a new standard of care for the practice of clinical hypnosis based on his particular theoretical position regarding dissociation and hypnosis, as well as on his newly expressed reservations about his previously published research (Lynn, Mare, Segal, & Sivec, 1994). In this regard, he has expressed particular concern about how clinicians may evaluate the accuracy of a patient’s memories or make interpretations about the causes/motivations underlying their publicly observable behavior. Lynn seems to worry that the issues are especially problematic for the practice of clinical hypnosis because they may have otherwise unintended deleterious repercussions. Consequently, he has advocated that practitioners of clinical hypnosis should now engage in obtaining informed consent from all patients before initiating a course of psychotherapy involving such issues.

We applaud Lynn for his courage in publicly announcing a change of viewpoint. Science is built on the backs of disproven theories, discarded hypotheses, and debunked premises. In this world of saving face, integrity and intellectual honesty are the first casualties. Lynn’s proclamation is all the more commendable because he essentially admits to conducting his practice in a manner that recently has attracted dozens of lawsuits against therapists. These cases, with which we disagree, argue that informed consent was always the standard of care, and therefore, its absence constitutes malpractice.

Lynn suggests that clinicians should engage in “a careful clinical assessment,” presumably for any evidence of suggestibility or dissociation, that would allow a patient the possibility of interpreting any mention of an “ego state” or “inner adviser” as being other than metaphoric. We agree that a careful clinical assessment should be done with all patients. However, given the relatively low overall base rate of dissociative disorders in the patient populations that most clinicians see, to suggest making such extra warnings and engaging in such additional assessment with all such patients seems extreme and excessive.

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We believe that Lynn’s proposal and the arguments he makes in support of it suffer from serious generalization problems. First, we acknowledge that there are always difficulties associated with evaluating the accuracy of anyone’s memory or interpreting the underlying causes/motivations of anyone’s behavior (including one’s own memories and actions). This is a problem not only in psychotherapy, but also in any social situation where human beings interact with one another.

Second, Lynn presents no new data or follow-up on the case studies in his prior report (Lynn et al., 1994) that directly support his implicit contention that memories obtained while in a state of hypnosis are necessarily more accurate or inaccurate than memories obtained in any other state of consciousness. Certainly, one can create false memories in hypnotized subjects. Bernheim cogently demonstrated this phenomenon over a hundred years ago (Scheflin & Frischholz, 1999). More recently, Loftus (1979) has presented a systematic program of research also demonstrating that it is possible to alter a person’s memory report for past events by a variety of means without any use of hypnosis. However, the magnitude of this memory alteration effect (whether hypnotic or nonhypnotic) has varied from study to study depending on the methodology employed to produce it and/or measure its effect. For example, Loftus (1979) has reported approximately 25% of subjects in one of her studies had their recollections about a target detail altered from what was present in the original stimulus event when using one kind of forced-choice recognition test. In contrast, McCloskey and Zaragoza (1985) used the same memory alteration procedure but found that less than 5% of their subjects had their memories altered when they used a different kind of forced-recognition test than that used by Loftus (1979). This illustrates another type of generalization problem, and one must be careful not to conclude that significant memory contamination effects reported in one experimental study would occur under different circumstances. Likewise, even statistically significant memory contamination effects like the 25% effect size reported by Loftus (1979) should not be overgeneralized to be expected universal effects which always take place in other situations (like a therapist’s office).

The original Lynn (et al., 1994) article admitted that it is not necessary to hypnotize a subject to produce, through demand characteristics, the reports of which Lynn speaks. A review of the literature on pseudomemories (Brown et al., 1998) quite clearly demonstrates that memory distortion may be as easily accomplished through social influence in a non-hypnotic context as in a hypnotic context. Therefore, one must question why hypnosis would be targeted for different treatment than psychotherapy interviews in general. The imposition of such a social psychological theoretical stance on psychotherapists in general would prohibit therapists from even mentioning the concept of “unconscious” ideas or an “unconscious mind”— concepts that are avoided within Dr. Lynn’s theoretical predilection. However, internal constructs related to dissociative conceptualizations are widely accepted by the largest proportion of those who practice clinical hypnosis routinely in patient care (Crasilneck & Hall, 1985; Erickson, 1980; Frederick & McNealy, 1999; Fromm & Brown, 1986; Gilligan, 1987; Hall, 1989; Hammond, 1990; Hilgard, 1986; Hunter, 1994; Lankton & Lankton, 1989; Rossi & Cheek, 1988; Watkins & Watkins, 1997; Watkins, 1992; Wright & Wright, 1987; Yapko, 1990), only a small portion of which involves working with memories or dissociative disorders. Most clinicians are not social psychologically oriented.

The general tenor of Lynn’s (2001) comments seems to be that of emphasizing the risk of false memories. However, a thorough and critical review of the research on memory distortion (Brown et al., 1998) finds that while memory distortion exists, it is much harder to create genuine false memories than false memory proponents would have everyone believe. Even research (e.g., Barnier & McConkey, 1992; Lynn et al., 1989, 1991, 1992; McConkey
et al., 1990; Murrey et al., 1992; Spanos & Bures, 1993-94; Spanos & McLean, 1986) that uses very strong suggestions and efforts to create pseudomemories in highly hypnotizable subjects finds that only a tiny minority are actually incapable of distinguishing between a suggested memory and the actual event that occurred, especially when they are debriefed away from the experimental context, after some period of time, when they believe the experiment has ended, and by someone other than the experimenter. Thus, the false memory reports found in research experiments are exactly that—verbal reports responding to experimenter demand characteristics, but which (in about 94% of even high hypnotizables) do not represent actual memory alterations at all. Lynn’s (2001) latest position also implies that it is easy to create dissociative identity disorder through suggestion and implication. The scientific base for this assumption was thoroughly explored, criticized and found to be completely unsupported by hard, empirical data (Brown, Frischholz, & Scheflin, 1999). While this evaluative review acknowledged the existence of uncontrolled studies that demonstrated in experimental subjects the creation of spurious dissociative psychopathology, these contextually influenced manifestations bore only a superficial resemblance to genuine, clinical dissociative presentations.

In his 1994 paper, Lynn described case studies where he himself used therapeutic procedures, which he is now questioning as potentially dangerous. Yet, he has presented no new follow-up data which indicate that any of these cases later manifested false memories or any other type of negative after-effect associated with the use of these psychotherapy techniques. Given this fact, and the literature cited, why then is he so concerned today that he is motivated to advocate for a new standard of care which involves obtaining informed consent before undertaking clinical hypnosis? He presents no compelling data against the use of hypnosis, and seemingly ignores the data showing that when hypnosis is used with caution (e.g., according to the standards of care proposed by the ASCH guidelines), it is no more dangerous than any other memory retrieval technique (Brown et al., 1998; Hammond et al., 1994; Scheflin & Shapiro, 1979).

The ASCH guidelines (Hammond et al., 1994), later expanded in Brown et al. (1998) have been well received and accepted in the professional scientific community. For example, Hammond et al. (1994) received the Society for Clinical and Experimental Hypnosis’ Arthur Shapiro Award for best book of the year. In addition, Brown et al. (1998) received the Guttmacher Award from the American Psychiatric Association and the American Academy of Psychiatry and the Law for Best Publication in Forensic Psychiatry of the Year, the Distinguished Merit Award from the International Society for the Study of Dissociation, and the Arthur Shapiro Award from the Society for Clinical and Experimental Hypnosis for the best book of the year.

Dr. Lynn has previously advocated that the ASCH guidelines are helpful but do not go far enough, even though he has not carefully and thoroughly followed and tested them in the laboratory or in real world settings. But, how could the use of a sterile informed consent (in the ambiguous, unspecified way proposed by Dr. Lynn) go beyond or be more effective than the therapeutic cautions already suggested by the ASCH guidelines? People take potent drugs even when one of the possible side effects of the drug is death—after giving informed consent. If the potential side effects of using hypnosis are far less than death, does Dr. Lynn think that clients will say, “No, Doc, don’t use it?”

Interestingly, as we understand Lynn’s position, he claims that even the term “hypnosis” can have a suggestive effect. If this is true, why should the appearance of the term “hypnosis” on an informed consent form be less suggestive than if it were simply uttered verbally by the therapist? From a social cognitive perspective, the damage (social influence/expectancies created by the term “hypnosis”) may be done either orally or in writing.
A focus on the idea of informed consent is only the beginning point of a cogent analysis, not its conclusion. What matters is what the form says and how it is presented to the patient. Lawyers in recent cases advocating what has been called “the false memory” position have claimed that the informed consent form is invalid unless it identifies hypnosis as “dangerous,” “experimental,” and “unproven” by long-term outcome studies. Does Lynn advocate the use of such language? If not, even the forces in society that have been pushing most strenuously for informed consent when memory is refreshed with hypnosis will find Lynn’s form to be a failure because it does not properly “inform.”

Although Lynn is sensitive to the social cognitive effects of hypnosis, we believe that he has failed to address the social-cognitive effects of using informed consent forms. Recent studies have demonstrated that the placebo effect is far more powerful in medicine than previously believed (Talbot, 2000; Kirsch & Sapirstein, 1998). In fact, a significant element in healing is the expectation that the healer can cure. Rapport is essential to both healing and hypnosis. Informed consent forms do not promote rapport and their negative content acts as what has been called a “nocebo” (a suggestion that the patient will get worse) (Spiegel, 1997). Furthermore, the informed consent form’s negative communication is additionally untherapeutic because it conveys to the patient some danger in the use of a healing procedure and the fact that the patient agrees that the patient, not the therapist, is the source of the lack of cure—by consenting to the treatment, the patient absolves the therapist from legal responsibility. Thus, the patient gets a double dose of negative expectations.

In a masterful paper, Zeig (1985) suggested that while informed consent is valuable for physical medicine, it is counter-productive for mental medicine. Some of the ramifications of this argument are explored in Alexander & Scheflin (1998). In essence, the more you inform the patient about what you intend to do, the easier it is for the patient to create the defenses and resistances therapy needs to overcome. Furthermore, many hypnotic techniques are “indirect” and therefore will be unavailable if informed consent is a prerequisite to their use. If we shift from the clinic to the laboratory, informed consent eliminates studying the effects of suggestion because the subjects will have to be told what it is they will be expected to experience, thus contaminating the experiment.

It would be an error for the reader to conclude that our criticisms of Lynn’s paper are based upon an ideological disagreement with the use of informed consent forms. In fact, two of the authors (Hammond et al., 1994; Brown et al., 1998; Scheflin & Frischholz, 1999) urged a higher standard of care in dealing with memory issues. We believe that Dr. Hammond was the very first clinician outside of a forensic setting in 1992, following recommendations by Scheflin and Shapiro (1989), to begin routinely using an informed consent process and document whenever hypnotic exploration techniques were used clinically. A version of this informed consent document was later recommended to clinical hypnosis professionals (Hammond et al., 1994). Thus, we are not against the use of informed consent. However, the form and process we suggested was necessary in part because the law had forced the patient into an unholy choice between mental health and legal rights (Scheflin, 1993). Patients who were hypnotized might lose their ability to later testify in court because of judicial rulings against hypnosis (Scheflin & Shapiro, 1989). Informed consent was necessary to give the patient the protection he or she needed from the potential loss of legal rights.

In summary, we believe that Lynn’s arguments are underdeveloped and overly speculative. In fact, we think that they constitute an indictment of almost all of psychotherapy which often involves clinical evaluation of the accuracy of patient memories and hypothesizing about the underlying production, we see no reason why he needs to allege that these issues should be especially problematic for practitioners of clinical hypnosis as opposed to
psychotherapy in general. We wonder whether he would go so far as to advocate for first obtaining informed consent on these issues before initiating any course of nonhypnotic psychotherapy or possibly in other types of potentially suggestive social interactions? We see no reason to change the current standard of care for the practice of hypnotic or nonhypnotic psychotherapy at this time.

References


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