Informed Consent and Uninformed Clinical Practice: Dissociation, Hypnosis and False Memories

Steven Jay Lynn
State University of New York at Binghamton

My brief article (Lynn, 2001) on hidden observers and informed consent seems to have jangled a raw nerve among a group of commentators whose names are a virtual “who’s who” of the proponents of the use of hypnosis for memory recovery and of ego-state and dissociation-based therapies and theories. Dr. Kluft is right: “Proponents of paradigms often conduct themselves as if they were the agents of the manifest destiny of that paradigm, charged with sweeping away all that preceded or opposes their paradigm of choice (Kluft, 2001).” This is a cogent characterization of the commentators’ responses to my article. It therefore should come as no surprise to the reader that my suggestion to provide patients with informed consent regarding the metaphorical nature of hidden observers, inner advisors, ego states and the like, was rejected by the commentators.

Perhaps the central issue that separates my views from the commentators is whether a case can be made for the use of informed consent when ego state therapy, as Watkins describes it, or when memory recovery techniques are employed in a hypnotic context. I believe a strong case can be made for informed consent in these cases; the commentators disagree. Given the brevity of my response to my earlier article in which I expressed my views regarding informed consent, it seems appropriate to amplify my position in the context of addressing a question that Dr. Watkins asks: Does the “great risk of ‘pseudomemories and reifying metaphors’ of which Lynn warns, comes from his own practice or from theory?” At the same time, I will address Dr. Hammond et al.’s question of “why (I am) motivated to advocate for a new standard of care which involves obtaining informed consent before undertaking clinical hypnosis.”

But before I address these questions, it is worth noting that Dr. Watkins does not accurately represent my current beliefs about the risk of pseudomemories, and Dr. Hammond et al. do not accurately represent my current position about informed consent. With respect to Dr. Watkin’s comment, the truth is there are few data available regarding the rates of false memories across a variety of therapeutic contexts. This is the case because it is difficult to
corroborate patients’ memories, and therapists, as a rule, make no effort to do so. Accordingly, I have refrained from making statements about the precise rates of false memories in clinical contexts. Nor have I maintained that hypnosis carries a greater risk of false memories than other suggestive techniques. In fact, hypnosis has been compared with few other memory recovery techniques, so definitive statements in this regard are premature.

Contrary to the implication of Dr. Hammond et al.’s comments, I have not written that informed consent regarding hidden observers and false memories should be implemented before initiating any and all hypnotic interventions. It could be argued that there is no imperative to inform patients about false memory risk in many present oriented, behavioral, highly circumscribed, problem focused treatments of negative habits and pain conditions. After all, in such cases, there may be little or no need to delve extensively into the patient’s past. At the same time, it is important to be aware of the limitations and potential inaccuracies of self-reports with respect to all therapeutic endeavors, even when acquiring basic information such as the nature, duration, and specificity of identified problems. If and when the treatment focus shifts to memory recovery or review, or when suggestive and potentially leading procedures are used, such as those described by Dr. Watkins in his discussion of ego state therapy, I can find no justification for dispensing with informed consent procedures about the fallibility of human memory.

Dr. Hammond et al. ask whether I would “...go so far as to advocate for first obtaining informed consent on these issues before initiating any course of non-hypnotic psychotherapy.” I would advocate to first obtain informed consent if the therapeutic procedures involve memory recovery and/or the elicitation of ego states, regardless of whether the therapy was labeled as “hypnotic” or otherwise. This is because the same sociocognitive variables that engender false memories in the hypnotic context—expectations, attitudes, suggestions, imaginative involvement, event plausibility, and suggestibility—can engender false memories in nonhypnotic contexts (Lynn, Myers, & Malinoski, 1997b). A strong case can be made for the fact that when expectancies about the volume and veracity of recovered memories are controlled for, little or no differences between hypnotic and nonhypnotic treatments would be expected to emerge. Also, when highly suggestive procedures are implemented, regardless of whether they are administered in a hypnotic context or not, differences between hypnotic and nonhypnotic recall are often not apparent (see Lynn et al., 1997b). Accordingly, care must be taken across the gamut of psychotherapies to minimize suggestive interventions and inform patients where appropriate (Lynn, Lock, Myers, & Payne, 1997a; Lynn et al., 1997b). I am hopeful that Dr. Hammond et al.’s commentary signals an emerging consensus regarding the importance of minimizing suggestive influences in nonhypnotic as well as hypnotic therapies.

Where Do My Ideas Come From?

In answer to Dr. Watkins’s question, my beliefs about false memories, dissociation, and informed consent come from my research, clinical, and forensic experience, as well as the psychological literature.

My Research. I started doing hypnosis research in the early 1980s. In keeping with the clinical wisdom of the times, I fully expected that hypnosis would improve recall. Accordingly, I was surprised to discover that our initial memory studies showed that hypnosis had a negative impact on recall (Sweeney & Lynn, 1986) and that hypnotic age regression elicited childhood memories that could not be corroborated by parents (Nash, Drake, Wiley, Khalsa, & Lynn, 1986).
Since that time my colleagues and I have conducted many studies. In no study have hypnotized participants reported more accurate memories than nonhypnotized participants, without an accompanying trade-off of an increase in false reports. Furthermore, research in our laboratory and elsewhere has shown that hypnotized subjects are somewhat more likely to be more confident in the accuracy of their remembrances than nonhypnotized participants, although the “overconfidence effect,” when it is present, ranges from small to sizable in magnitude (see Lynn et al., 1997a; Lynn et al., 1997b). Our research has also shown that hypnotic procedures can: a) increase the rates of implausible memories above and beyond waking conditions (Marmelstein & Lynn, 1999), b) diminish the consistency of recall of emotional “flashbulb” memories associated with Princess Diana’s death (Krackow, Payne, Lynn, & Lock, 1998), and engender false recall of events that did not occur at the time suggested (Sivec & Lynn, 1996). Additionally, simple instructions that motivate participants to do their best to recall events yields recall comparable or superior to hypnotic recall, both in terms of sheer volume of events remembered and recall accuracy (see Lynn et al., 1997b). In short, the balance of the evidence provides no warrant for the use of hypnosis for purposes of memory recovery.

Clinical experience. In the late 1970s, I began working as a supervising psychologist (1978-1992) at a state mental hospital where a ward that housed “multiples” (as they were called by the staff, and now may be diagnosed as dissociative identity disorder: DID) was established under the supervision of a physician who became a well-known figure in the emerging “MPD movement.” The psychology staff had an opportunity to review tapes of hypnosis sessions conducted with DID patients in both individual and group contexts, and had an opportunity to work with the patients in a number of capacities.

The psychology staff were united in the belief that many of the techniques used, which involved eliciting different personalities and ego states, engendered false memories in patients who received considerable attention and social rewards for expressing distinct personalities in therapy sessions and on the ward. Members of the psychology staff noted that certain patients expressed strong doubts about whether they were really “multiples,” that some patients stated they felt pressure to bring forth memories to please the treating therapist, and that some patients seemed to decompensate in the process, while still other patients appeared to thrive on the attention they received and benefited from the staff’s caring and concern for their well being. The psychology staff expressed to the physician and the hospital administration the concern that the highly suggestive procedures and social rewards for certain patients to express distinct personalities were “creating multiples” and shaping their memories. All of this occurred before there was much if any awareness of “false memories” in the larger psychology community. I firmly believe that most of the DID patients at the state hospital would have benefited enormously from the implementation of informed consent procedures along the lines I have recommended.

In recent years I have treated a number of individuals who were diagnosed by previous therapists as DID patients. I also have had the opportunity to work with so-called retraction (Lynn, Stafford, Malinoski, & Pintar, 1997): patients who underwent suggestive therapeutic procedures, came to believe they had multiple identities, and then later came to disavow their DID diagnosis as they came to appreciate that their self-perceptions were shaped by their treating therapist. In each of these cases, I was impressed with the ill-advised tactics of the therapists, the use of hypnosis without proper safeguards and warnings about false memories, and the relentless search for hidden alters and dissociated memories. Unfortunately, no informed consent was given in any of these cases.
Forensic experience. Dr. Kluft wonders whether my contribution was driven by my role as a forensic expert. The truth is that over the years I have testified before a jury in only two medical malpractice cases, and in those cases I did not testify about whether the therapists’ clinical work measured up to the prevailing standard of professional care. However, in these cases, and other cases I am privy to, my impression was that the therapists were woefully ignorant of the scientific literature, and that it was their ignorance that was largely responsible for their legal tribulations. These therapists would have been in a far better position to defend their clinical practices (and reputations) had they provided their patients with the informed consent that the commentators eschew.

The psychological literature. My opinions about hidden observers and the like, and informed consent, in particular, have also been shaped by the psychological literature. Contrary to what the commentators imply, I do not reject careful use of techniques like suggesting “inner advisors” and “hidden observers,” so long as patients harbor no misunderstandings regarding the metaphorical nature of the suggested division of consciousness, and memory recovery is not associated with the therapeutic intervention. Nor do I reject the term “unconscious,” as Dr. Hammond et al. state. To the contrary, much human activity is carried out with little or no conscious awareness. In a recent article (Kirsch & Lynn, 1999), I have argued that human action is triggered automatically and largely unconsciously at the moment of action. Moreover, I surely have no quarrel with the use of the term dissociation as a descriptive label (see Kirsch & Lynn, 1998), as evidenced by the fact that I have edited a book on dissociation (Lynn & Rhue, 1994) that clearly reflects my interest in fascinating dissociative phenomena. However, I am skeptical of the hypothesis that hidden observer phenomena or ego states are due to a division of consciousness into two or more concurrent parts (Kirsch & Lynn, 1998).

The basis for this skepticism can be gleaned from reading reviews of the literature I have written in collaboration with Dr. Irving Kirsch and other colleagues (Kirsch & Lynn, 1998, Lilienfeld, Lynn, Kirsch, Chaves, Sarbin, & Powell, 1999). In a nutshell, the research reviewed indicates that the “hidden observer” is actually a “flexible observer.” Depending on the instructions with which they are created, hidden observers report more or less pain than otherwise reported, they either do or do not reverse figures written on paper (89/98), and they either report their hypnotic experiences as effortful or effortless (Green, Page, & Handley, 2000; Kirsch & Lynn, 1998). These data render it unlikely that hidden observer reports or “ego states” reflect spontaneously occurring phenomena. Flexible observer studies indicate that the apparent divisions in consciousness can be produced by the instructions used to elicit the hidden observer reports.

Counter-arguments

Dr. Kluft says that my recommendations “...stem from one available model, which has been challenged for its adequacy in addressing all relevant research and clinical observations.” This, of course, is true. I am unaware of any model of human behavior that is capable of “addressing all relevant research and clinical observations.” Yet the sociocognitive model of hypnosis is supported by a wealth of empirical data (see Sherman & Lynn, 2000), whereas Dr. Kluft cites little or no research support for the counterarguments he endorses. For example, the evidence that Dr. Kluft presents to argue that, “It is not at all clear that the hidden observer phenomenon is no more than a metaphor” is to appeal to authorities of “impeccable credentials” who argue for a neodissociation theory. He also states that “...many conditions and phenomena involving ego states occur naturalistically. ” Yet he does not document the scientific basis for this claim nor give much attention to the studies critical of the idea that hidden observers are free standing phenomena.
Dr. Kluft’s primary way of discrediting laboratory studies is to state that it is “very dangerous” to generalize from laboratory studies to conclude that “longstanding alter personality phenomena can be created in a significant number of psychiatric patients from similar interventions.” Of course, Dr. Kluft may be correct in this regard. Ethical constraints on research do not permit the creation of full-blown dissociative identity disorder. Unlike researchers, however, clinicians do not have to submit their methods to an ethical review board before having to put them into practice. In the absence of such constraints, DID may well be created in clinical contexts, albeit at an unknown rate.

Is more research in clinical situations necessary, as Dr. Spiegel claims, before clinicians can justifiably add more caveats about the possibility that patients may misinterpret metaphors? I think not. If it is very easy to manipulate hidden observer reports in the laboratory, why would this not occur in a clinical context?

Relatedly, Dr. Hammond and his associates caution the reader against generalizing findings from the memory laboratory to the therapist’s office. I agree that caution is appropriate in this regard. However, whereas Hammond et al. seem to be unimpressed by the rates of false memories in the laboratory, such as a 25% effect size reported by Loftus (1979), I find these results to be quite impressive and alarming with respect to the ease by which false memory reports can be created.

Dr. Hammond and his colleagues state that, “only a tiny minority (of participants) 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.” Yet many of the studies Dr. Hammond et al cite to support this point indicate that participants’ reports show a high degree of sensitivity to experimental demands to the point that they readily alter their memory reports to conform to shifting situational cues. However, this general finding is completely consistent with the idea that memory is malleable and readily subject to social influence!

If memory reports can be so readily influenced in the laboratory, why would this not happen in the clinic? And even if patients initially were not fully confident in the veracity a particular memory report, might they not, over time, become more highly confident that a suggested event actually occurred in a clinical situation? The therapist’s potential to exert social influence on a help-seeking, eager to please, vulnerable patient is probably much greater than the experimenter’s influence on a subject participating in a one shot experiment for a small amount of money or course credit. Accordingly, waiting for more definitive clinical research before implementing cautions warranted by an already substantive body of empirical studies would only serve to withhold valuable information from patients.

The relevance of research findings. Dr. Kluft takes a different position and maintains that it is desirable to inform clinical practice with research findings “...only if the research findings are clear, demonstrate ecological validity, and have not been used as springboards for generalizations.” But if this is true, shouldn’t we impose similar limitations on informing clinical practice from findings obtained from the clinic? Would we have any clinical innovations if we insisted on a similar standard? Would it be appropriate for any clinician to adopt many of the sensible ASCH guidelines for using hypnosis that Dr. Hammond et al favor, which were not empirically evaluated before they were promulgated?

The ASCH Guidelines. The American Society of Clinical Hypnosis (ASCH) has recognized the problem of false memories in psychotherapy and has made considerable efforts to educate clinicians in this regard by promoting guidelines for the use of hypnosis to recover memories
in psychotherapy. Although I have taken issue with specific aspects of the guidelines (Lynn et al., 1997b), I applaud their general thrust and intent. Indeed, the ASCH guidelines were the first, and perhaps only, attempt by a professional hypnosis organization to inform clinicians about potential problems associated with therapies that employ hypnosis and explore memories. It also bears mentioning that the guidelines advocate the use of informed consent, as did Scheflin and Shapiro (1989) some years earlier, and attempted to bridge the gap between empirical research and clinical practice.

Dr. Hammond and his colleagues assert that I “largely ignore 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.” However, the fact is that I have not “ignored” data; there are no data “out there” to ignore that directly pertain to this issue. Indeed, as far as I know, no published study has faithfully evaluated all of the ASCH guidelines in a clinical, much less a laboratory context. At the present time there is very little evidence regarding the efficacy of the informed consent procedures recommended. The small amount of research that has been conducted on various aspects of the ASCH guidelines, specifically prehypnotic warnings about the fallibility of warnings, has provided mixed results. However, no study has shown that implementing warnings favors hypnosis over nonhypnotic interventions in any respect. And to my knowledge, different memory recovery techniques have not been compared systematically with respect to the impact of the ASCH guidelines.

So it can be fairly said that, at the present time, there is little or no empirical support for (or against) the ASCH guidelines. However, it is clear that false memories can and do occur in hypnotic and nonhypnotic contexts, even when participants are warned about the possibility of such memories. So while informed consent provides patients with useful information and an informed choice regarding their treatment, warning patients about the unreliability of memory is no guarantee that false memories will not arise in hypnotic and nonhypnotic situations. Accordingly, it is imperative that clinicians not be lulled into a false sense of security even when patients are duly informed about false memory risk: As the ASCH guidelines stipulate, clinicians should be scrupulously cautious in their work with patients to avoid unduly suggestive influences in therapy.

The major support that Dr. Hammond et al. offer for the guidelines is to tout various awards their writings have received that contain the ASCH guidelines. However, in the interests of balance, it is worth noting that the book they refer to (Brown et al., 1998) has been sharply criticized in the scientific literature (McNally, 1998; Perry, 1998). Empirical evaluation of the ASCH guidelines is a priority and clearly preferable to claims of validation based on awards, critical acclaim or lack of it by special interest groups and societies, and the opinion of book reviewers.

Whereas the American Society of Clinical Hypnosis and the commentators are generally sanguine about the use of hypnosis for memory recovery, other professional organizations are less so. For example, the American Psychological Association-Division 17 (1995) guidelines state that hypnosis should be used to support and strengthen “self-care activities,” but should not be used for memory recovery, a conclusion also reached by the Canadian Psychiatric Association (1996). The American Psychological Association (1996) has recommended that hypnosis not be used for clients who are attempting to retrieve or confirm recollections of histories of abuse, and the American Medical Association (1994) has stated that hypnosis be used only for investigative purposes in forensic contexts. In my opinion, these recommendations are appropriate, given our current state of knowledge.
Is Ego State Therapy Effective and Not Suggestive?

Dr. Watkins claims that psychological symptoms can be associated with “entities” that differ from the “overt personality” and have “a history of origin and development uniquely its own different from that of the primary personality.” Ego states “covertly influence the behavior, thinking and affects of the overt, conscious personalities.” Dr. Watkins contends that the “personality segments, or part persons” “act like DID alters, but seldom become manifest except under hypnosis with a trusted operator.”

The problem is that clinical practices based on the belief that difficulties in living can not be resolved until hidden entities and dissociated memories are uncovered by hypnosis are thought to increase risk of both false memory and the creation of dissociative identity disorder (see Lilienfeld et al., 1999; Spanos, 1996 for reviews). Beyond that, I am not familiar with any empirical science that supports Dr. Watkins’s conceptualization of personality.

Dr. Watkins correctly observes that patients are sensitive to posture, gestures, word accent and inflection, biases, and beliefs. However, Dr. Watkins also believes that “MPD alters seldom reveal themselves to skeptics.” He avers that the genuineness of the ego state depends on whether the individual who elicits it is sincere or not. And finally, Dr. Watkins claims that ego states can be suggested “but these are frequently artifacts, designed to please the therapist or researcher.”

In these comments, Dr. Watkins clearly acknowledges patients’ suggestibility and their exquisite sensitivity to social cues and demand characteristics. Accordingly, it is surprising that he can not concede the possibility that so-called MPD alters are suggested phenomena that “reveal themselves” to believers (and only to believers) precisely because their appearance is encouraged and their attributes later shaped by social influence, demand characteristics, and therapist beliefs and expectancies.

Dr. Watkins states that “Both therapists and researchers may influence human subjects even more through their covert attitudes, beliefs, and biases than their verbal suggestions. This influence involves unconscious resonance (Watkins, 1978) and transfersences, ignored in the laboratory.” Dr. Watkins may be correct in his claim about covert influences, however, I am not aware of a single study that supports his contention. My concern is that minimizing the role of verbal suggestions risks desensitizing therapists to the real risks of leading psychotherapeutic procedures.

Dr. Watkins reports an outcome study of patients treated by Helen Watkins with ego state therapy during an 18 year period. The patients reported their ego state treatment was superior to psychoanalysis. These anecdotal reports of patients apparently contacted by their therapist, sometimes years after their treatment, are highly suspect and likely to be compromised by any number of self-report and self-selection biases. There are many reasons why therapists have “satisfied customers,” independent of actual changes in their patients’ lives. Whereas ego state therapy may well be effective, controlled research using psychometrically sound assessment instruments administered by researchers independent of the treating therapist would be far more useful and convincing than anecdotal reports.

I am concerned that the techniques Dr. Watkins advocates can be blatantly leading. The technique that Dr. Watkins favors to “activate ego states and minimize demand characteristics” is to ask the patient: “Is there any part of Jane who knows what is causing her disturbance, but if there is no such separate part that’s OK?” My impression is that contrary to what Dr. Watkins claims, this procedure is rife with demand characteristics: it clearly implies that there may be a part or parts of Jane that cause her disturbance. Would
Jane talk about a “part” of herself if she were not asked about such a part? And wouldn’t it be gratifying to the therapist, as well as the patient, and an indication of progress in therapy if there were a “separate part” that could shed some light on Jane’s condition? Frankly, Dr. Watkins claim that he encounters dissociated “parts” of patients every day, while his colleague has “in 30 years of practice…never seen a multiple” does not surprise me.

*Are false memories necessarily “dangerous?”* Even if ego state therapy were to produce false memories at a higher rate than other therapeutic interventions, would the emergence of false memories necessarily be “dangerous?” The answer is “no.” Memories retrieved during hypnosis, or any psychotherapeutic technique for that matter, need not be “historically accurate” to have therapeutic value (Spence, 1982). I acknowledge that all memories produced in psychotherapy and other contexts are not necessarily accurate, that memory is not an unbiased and permanent record of events as they unfolded in the past, and that it is possible that change in psychotherapy may come about regardless of the historical truth of memories. However, as a rule, “narrative truth is no substitute for historical truth” (Kihlstrom, p. 23), and when patients place stock in false narratives, they may be diverted from confronting and resolving important issues in therapy (Lynn et al., 1997a). However, there is considerable documentation for the fact that the emergence of demonstrably false memories can have serious personal and interpersonal ramifications when patients come to believe, for example, that they have been sexually abused by a close relative when this is not the case (see Lynn & McConkey, 1998).

*Can DID be easily created?* I have argued that procedures like the ones Dr. Watkins recommends illustrate the need for informed consent. Yet would techniques derived from Watkins’s ego state therapy necessarily create dissociative identity disorder? I think not. Dr. Kluft implies that my contribution has “taken leaps from the notion that a certain outcome is possible, to the thought that it is probable, to the conviction that it will occur at a high degree of frequency, to the certainty that it has occurred or will occur in a given case.” And Dr. Hammond and his associates state that my “latest position also implies that it is easy to create dissociative identity disorder through suggestion and implication.”

In actuality, I have never claimed that it is easy to create iatrogenic dissociative identity disorder with ego state therapy, or any other therapy for that matter. Nevertheless, as is the case with medical procedures, providing patients with informed consent should not be reserved for only probable or certain negative outcomes; instead, informed consent should provide the patient with useful information and guide collaborative treatment decisions. Informed consent is particularly important when procedures that are suggestive or might place a patient at heightened risk are contemplated or implemented, even if the rate of negative effects is not large or is not known with precision.

*Are There Negative Effects of Informed Consent?*

Whereas several of the commentators maintain that I am excessively cautious in my call for informed consent, I believe their warnings about the negative effects of informed consent on therapy are excessive and unfounded. Watkins states that “...requiring a signed consent from the patient before applying these procedures...is a good (paranoid) way to tell the patient that you the therapist don’t have confidence in your technique, and you don’t trust him not to sue you.” Likewise, Dr. Spiegel implies that informed consent of the type I suggested would “have a chilling effect on patients’ willingness to enter or continue in therapy.” Dr. Kluft also uses the term “chilling effect” to describe the potential effects of warnings, along the lines I described, on “...a patient with as yet undeclared ego state phenomena.” Kluft wonders “whether the aggressive expansion of informed consent into psychotherapy in general and hypnosis in particular goes beyond what is reasonable and
creates a situation that violates the Hippocratic axiom of “primum non nocere,” that is, “firstly do no harm.” And finally, Dr. Hammond and his colleagues assert that consent forms can act as a “suggestion that the patient will get worse.”

Although the commentators make strong claims, they fail to provide strong evidence to buttress their opinions. Dr. Kluft could only identify one instance of a patient who was “scared out of therapy” in his own practice. Whereas Dr. Hammond et al. accuse my arguments of being “overly speculative,” neither they nor the other commentators support their rejection of informed consent with a scintilla of data to demonstrate that the risks of informed consent outweigh the benefits of providing patients with accurate information about potential treatments. The commentators would reject my recommendations for lack of clinical research support, while they marshal no research support at all to justify their negative feelings about informed consent.

Based on my experience, it is easy to integrate informed consent procedures into the flow of therapy in a seamless manner. Contrary to Dr. Hammond et al.’s avowal that informed consent compromises rapport with the patient, I have never found this to be the case. Patients appreciate the opportunity to lend their stamp of consent to the therapeutic procedures, and they feel empowered in the process. That said, I acknowledge that my positive observations are as anecdotal as Dr. Kluft’s reservations about informed consent. Speculation about the repercussion of informed consent are a poor substitute for research on the positive and negative concomitants of informed consent procedures.

**Concluding Comments**

Dr. Speigel and Dr. Kluft invited me to follow up on the subjects in the three studies I described and find out whether any of them misinterpreted the hidden observer instruction and developed an iatrogenic dissociative disorder. I thank them for their suggestion, and I offer my own: I invite the commentators, and other workers in the field, to conduct research on expectancies and the shaping influence of highly suggestive therapeutic procedures in psychotherapy. Although ambitious, such studies could be done, even in the context of developing tailored treatments to suit individual patient’s needs. It is through a partnership between clinicians and researchers that thorny issues like those surrounding informed consent in hypnotic and nonhypnotic treatments will ultimately be resolved.

**References**


