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Commentary on Hypnosis in the Desensitization of Fears of Dying

Two Cases, One Good Therapist

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ABSTRACT

In my view, the most important similarity between Betty and Lawrence is that they had the same therapist, not that they had similar problems (Hamburg, 2006). This view raises several issues. First, are there any aspects of what Dr. Hamburg does that might be problematic for a less experienced and talented therapist. The one thing that clearly falls in this category is his use of the videotape metaphor for discovering memories during hypnotic age regression. This metaphor has created difficulties in the past and might best be avoided.

Second, Dr. Hamburg seems to rely more on personally invented procedures and clinical lore than on manualized, evidence-based techniques. In this paper, the evidence base for evidence-based procedures is examined in terms of what we know about effect sizes from research on clinical hypnosis. I suggest that the evidence base for most evidence-based procedures is, at best, weak.

Third, I speculate briefly about the effects of training clinical psychology graduate students in cognitive-behavioral therapy (CBT) by having them serve as therapists in randomized control trials (RCTs). In contrast to my previously stated (relatively positive) views (Karlin, 2002), I suggest that such training may not aid them in becoming thoughtful, creative, flexible therapists like Dr. Hamburg.

Key words: hypnotherapy; obsessive fear of death; videotape metaphor; evidence-based procedures; training in cognitive-behavioral therapy

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INTRODUCTION

Hamburg (2006) reports on two patients, Lawrence and Betty, who were unable to avoid thinking of their own deaths. Both patients were treated with hypnosis. In the one case in which follow up was possible, a letter from Betty, seven years after the end of treatment, showed the patient doing well. She also attributed her success to Dr. Hamburg's intervention.

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While both patients shared the death anxiety problem, the cases were quite different. For example, Lawrence, as best I can tell, had no significant comorbidity, while Betty had significant problems in other areas including depression severe enough to require long-term antidepressant medication.[1] Lawrence was seen three times, Betty was seen 17 times. Lawrence was highly hypnotizable, responding with unsuggested total amnesia for the hypnotic period. Betty seems more like the more usual moderately hypnotizable patient most often found in a typical hypnotherapy practice. Lawrence's hypnosis involved reinforcing his boundaries and noting the careful nature of his approach to life. Betty's hypnotic suggestion involved age regression and a form of re-parenting. Lawrence's fear was of death, of ending, of lack of existence. During treatment Betty feared dying of cancer.[2] In sum, aside from sharing a fear of death and being treated with hypnosis, there is little in their conditions or their treatment that closely joins these two cases.

From my point of view, what does join the cases more closely than the patients' fear of death is the honest, creative, gifted clinician who treated them. Start with the fact that Dr. Hamburg's report is honest. These are genuine case reports, not vest pocket amalgam patients, in which vignettes formed by several patients are joined together to produce illustrations of a technique or principle (cf. Erickson, 1980; Haley, 1973). At several junctures, Dr. Hamburg tells us that he was perplexed about how to proceed or that he felt therapy was failing. Such statements are unusual in reports of this kind. He also reports important mistakes. For example, assuming Lawrence's problems with thinking about death are similar to his own experiences, Dr. Hamburg forgets to ask Lawrence about external triggers of the fear. In fact, both the realization that he must correct this error and the resulting list of triggers are critical to ensuing treatment. Dr. Hamburg also reports incorrectly remembering critical information as having occurred during waking, when it happened in Lawrence's dream. Again such statements are unusual and give us faith in Dr. Hamburg's reporting of what went right.

As for Dr. Hamburg being creative and gifted, one only has to look at the content of his hypnotic suggestions. In Lawrence's case, the combination of suggestions for mild dissociation, imaginal exposure to triggers, and cognitive restructuring is quite interesting. There is also the suggestion about major change that he gives with each hypnotic intervention. Major change in response to a brief hypnotic intervention has a long history and is part of the cultural stereotype about hypnosis (cf. Ellenberger, 1970; Wolberg, 1948). Dr. Hamburg makes the possibility of swift, large changes explicit. Here is the "*generalized suggestion for rapid change"* that is used at the beginning of hypnosis.:

> Sometimes the psychological changes we experience in our lives happen slowly and gradually. For example, as we grow older we gradually become more

[1] In her follow-up letter seven years later, Betty states that she plans to take Lexapro forever. Most psychiatrists suggest such a course only when a patient has had three or more episodes of major depression.

[2] As part of her treatment, in their seventh session, Dr. Hamburg has Betty imagine herself in the last stages of dying of heart disease. "She reported that she found this image to be 'profoundly comforting' " (Hamburg, 2006, this module).

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> mature and wiser...or as we get to know someone over time we realize that we like them more and more. But some psychological changes, sometimes big ones, can happen very quickly, almost instantly. Some of these very rapid psychological changes are unpleasant, like [in the case of a post-traumatic phobia] what you experienced some time ago. But some of these very rapid and very big changes are pleasant ones...they are changes we very much want. Falling in love is one of those pleasant changes that many people experience almost instantaneously. That's why we have the phrase, "love at first sight." Religious conversion is another huge psychological change that can happen to people in an instant. Something touches you and suddenly the whole world looks different...And the interesting thing is that we don't really do anything to make these big and pleasant changes happen to us....they just happen...and it's hard to say why...So if in these next weeks or even days or even hours you find yourself feeling much better than you have, you might wonder why...but it doesn't really matter...Was it this hypnosis, this tape? Maybe, maybe not...it really doesn't matter...all that really matters is that you feel better...and it really doesn't matter why because big positive changes like this happen all the time...they just happen (Hamburg, 2006, this module).

Notice that he tells patients that rapid change occurs in non-hypnotic situations and is unsurprising in response to hypnosis, thus normalizing the change. Moreover, he gives the suggestion in a very permissive way, so that if major change doesn't occur quickly, little or nothing is lost. This seems like a potentially very effective intervention. With wording changes to make it sound more like me, I am going to try it.

It would be unnecessarily repetitive for me to go over each case in detail. Instead, I will go over the one part of one intervention that has a history of becoming problematic in less experienced and talented hands. Then, I will discuss the fact that Dr. Hamburg seems to invent techniques rather than transplanting evidence supported ones. The fact that the vast majority of clinicians do the same thing is often taken as evidence of their intransigence and our lack of marketing skill. In examining this issue, I will explore the question of how good the evidence base is for evidence-based treatment. I will discuss what is becoming a noticeable undercurrent among a number of cognitive and behaviorally oriented academic clinicians: the suggestion that there may be less evidence for evidence-based treatments than we might like. Finally, I will ask whether having graduate students participate in the search for evidence by serving as therapists in RCTs is a good way to train them.

THE VIDEOTAPE METAPHOR

One of the techniques presented by Dr. Hamburg is problematic. During age regression with Betty, Dr. Hamburg uses a videotape metaphor for memory retrieval. He reports saying:

> Our memory is kind of like a videotape library. Each of our memories is on a separate videotape. All the tapes are there in the library. The only problem is they are all jumbled up, not arranged in any particular order. But our unconscious mind has a way of using special searching and sorting procedures to find videotapes that

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> we wouldn't be able to find otherwise. So now I'd like you to find the videotape that is labeled "My Earliest Memory" (Hamburg, 2006, this module).

Unfortunately, the videotape metaphor has a troubled history and can be quite dangerous in the hands of a clinician who is less talented and experienced than Dr. Hamburg.

The videotape recorder model of memory is still how memory is conceptualized by much of the public and many professionals. It suggests that we have records of all our experience in a storage area in the brain. The video tape model is largely about episodic, autobiographical memory, memory based on personal experience, but not learned skills or impersonal facts. In strong statements of this notion, even the most trifling details in our lives are thought to be retained in the storage area. The problem is to access the memories kept there. Recovery of long forgotten, accurate memories is part of the mythology of hypnosis. Somehow, the myth holds, hypnosis allows the hypnotist to help us reach into this storehouse of memories and retrieve the requested one.[3]

Since the work of Bartlett (1932), we have known that memory is a reconstructive process, not a reproductive one. Most reconstructions (memories) of past events are fairly good representations of those events. While details tend to fade over time, and material that makes the memory coherent may be added, if the information to be remembered is salient to begin with and occasionally rehearsed, the essence of a memory is likely to remain intact. Moreover, attempts to influence such memories also tend to change only details, not the essence of remembered events. (cf. Wells & Loftus, 1984).

However, while we are quite good at remembering things that actually occurred, there are large periods of our lives for which we have no memory. Early childhood and sleep are two such times. Even in later childhood there are many gaps in memory for activities and experiences. Memory for these periods is vulnerable. If you prime research participants with believable information, at least 20% of normal adults will later remember, in detail, childhood events that never happened (cf. Loftus & Ketcham, 1994). Thus, periods for which we have no memory are like relatively uncluttered canvases on which any fantasy can be painted and subsequently recalled as a memory of real events (cf. Spanos, 1996; Yapko, 1994).

During the last two decades of the 20th century, almost all hypnosis research groups spent some time looking at hypnosis and memory. In part, the interest was created because courts had to wrestle with hypnotically influenced testimony. Further, reports of incest during early childhood increased in the 1980s and reached a peak in the mid-1990s (cf. McNally,

[3] The use of the videotape model of memory in the context of hypnosis is most closely related to the work of Reiser (1980), who taught it to police investigators with quite problematic results.

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2003).[4] Recovery of such memories, often aided by hypnosis or hypnotic-like procedures, became common and was treated as reflections of historically real events (cf. Perry, 2002). Both the forensic and clinical false memories that emerged were often quite destructive (cf. Karlin, 1997a; Karlin & Orne, 1996, 1997; McNally, 2003).

Hypnotic age regression can provide a setting in which "memories" that are entirely created from whole cloth may be suggested or emerge on their own. Note that age regression techniques may result in vivid, detailed portrayals of the past. If one gives appropriate suggestions to reasonably good hypnotic subjects, they will produce memories of their own birth, influential events that occurred while they were in the womb, and what occurred in their previous lives (cf. Spanos 1996). With appropriate instructions, one can also age *progress* people so that they describe their lives after rebirth a hundred years from now (cf. Yapko, 1994) Unfortunately, I can not age progress someone to next week, have them pick up the newspaper, and tell me the value of the Dow Industrial Index nor the price of oil. Fantasy has its limits.

However, those limits are not always obvious. The videotape metaphor suggests that what one gets during age regression is veridical memory for real events.[5] After all, what else might be recorded on videotape in the storage area? The notion that the patient is actively creating memories during age regression is seldom considered [6]. Moreover, age regression can result in vivid, detailed, emotion-laden portrayals of past events. There is also a tendency to fill in the gaps in memory ("confabulate") to create a coherent narrative. For example, Betty displays a number of such flaws and Hamburg treats the obvious confabulation with benign neglect, the best way to treat it. Unfortunately, given the pop psych literature and books like *The Courage to Heal* (Bass & Davis, 1988), with the wrong therapist this procedure can easily lead to false memories of childhood incest and to symptoms of a malignant version of multiple personality disorder (cf. McNally, 2003).[7] During the late 1980s and most of the 1990s, it did so frequently,

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[4] Some authors hold that trauma produces memories that are unlike any other kind of memory. In this view, memories of trauma, especially sexual trauma, are accurate, long-lasting, but often held behind amnesic barriers. (For such views see, for example, J. Freyd, 1996; Herman,1981; and Courtois, 1988. For a series of papers from both sides of the recovered memory/false memory issue, see, for example, Pezdek and Banks, 1996. For a view of the damage caused by this iatrogenic epidemic, see McNally, 2003.) I would note that during almost 30 years of a practice partly devoted to hypnosis, I have seen one person, an 11-year-old child, who was temporarily unable to remember a salient feature of a trauma, rather than unwilling to do so. Discussing it with Martin Orne, he said that in over 40 years of practice he had seen two such cases. Based on our experience, it would seem that such amnesia is possible, but when examined carefully, extremely rare.

[5] In fact, age regression usually yields a little more accurate memory combined both with more inaccurate memory and increased certainty about both (cf. Karlin, 1997b).

[6] Obviously, these *caveats* do not apply to Dr. Hamburg's work. Hamburg states that regression yields memories that may be true, confabulated, or false as he recounts Betty's memories during age regression.

[7] The videotape model of memory also encouraged a malignant form of multiple personality disorder, when patients with Category B personality disorders adopted a dramatic role similar to that portrayed in Schrieber's book, *Sybil* (1973). In this view, most multiple personalities were formed at the hands of incestuous parents, many of whom were also members of an international satanic cult that, among other things, ate babies. Interestingly, Herbert

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to the detriment of patients and their families. Additionally, in the hands of the wrong prosecutor, the videotape metaphor can lead to major miscarriages of justice (cf. Karlin, 1997a; Karlin, De Filippo, & E. Orne, 2003). During the 1980s and 90s, a good deal of professional time was spent explaining that to courts and broken families. We did not succeed in getting the message across at least some of the time.[8]

Alternatively, as long as the memories are relatively innocuous and the therapist expects to hear fictive accounts of the past (as Hamburg does), no harm and some good may be done. Additionally, if the memories obtained during hypnotic age regression are used as symbolic representations of events and relationships, a good deal of information can be gained and narratives constructively altered. That is what happens in Betty's case. But, there are real dangers in these procedures. I think age regressing people to a specific time in their lives without presenting the videotape model of memory avoids some potential iatrogenic problems.[9]

HYPNOSIS AND THE EVIDENCE BASE FOR EVIDENCE-BASED, PSYCHOTHERAPY

Lawrence and Betty were both seen some time ago. However, it is clear that Dr. Hamburg invents his interventions instead of transplanting evidence-based techniques. Research-supported principles certainly play a part in his work, but he doesn't turn to randomized control trials (RCTs) or manuals for his treatment protocols. Why?

In fact, there are probably many reasons. However, his behavior raised two questions for me. First, if people like Dr. Hamburg are seemingly ignoring specific evidence-based techniques, are they right? How solid is the evidence base for evidence-based techniques? Second, as I am involved in the clinical training of graduate students, this led me to ask whether training graduate students for psychotherapy, at least in part, by having them be therapists in manualized RCTs is a good way to get more people in the field with the creativity and flexibility displayed by Dr. Hamburg?

Spiegel of Columbia University, the one expert on hypnosis and dissociative states who examined Sybil, declared that she had a borderline personality disorder rather than multiple personality disorder. The publisher, for purely commercial reasons, insisted that Sybil be billed as a "multiple." Commercially, he was quite right. Incidentally, I strongly prefer the term "Multiple Personality Disorder" to "Dissociative Identity Disorder," the DSM-IV term. "Dissociative Identity Disorder" assumes that the basis of the psychopathology is a dissociative process, not a means of getting attention from others and avoiding blame for one's behavior.

[8] Through a series of decisions starting in the late 1970s, 25 states exclude, as a matter of law, testimony that has been influenced by hypnosis. In most other states there are major limitations placed on such testimony. Yet there are people who are still in jail, more that two decades later, whose convictions for murder were based on highly questionable, hypnotically influenced memories (cf. Karlin, De Filippo, &E. Orne, 2003).

[9] It may also help to provide the patient with information given informally about how the mind often produces symbols, not historical facts, when we dig into its deeper recesses.

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About thirty-five years ago, I was caught up in the behavioral/psychoanalytic wars. Having chosen the wrong graduate program, I was at Yale, most of whose clinical faculty and all of whose external supervisors were strongly psychoanalytically oriented. When Arnold Lazarus arrived to spend two years as a visiting professor, I felt as if I had returned to Kansas after too long a stay in an intellectual Oz. My first exposure to hypnosis was under his instruction during my clinical internship in 1971-72. When Lazarus moved to Rutgers University, I happily went with him.

I became intrigued by hypnosis in the mid-1970s. Hypnosis research was very exciting then. The major figures in those days included Ted Barber, Milton Erickson, Erica Fromm, Jack and Josie Hilgard, Martin and Emily Orne, and Ted Sarbin. We walked with giants.

At that stage of my career, I was very interested in differentiating the specific versus non-specific effects of psychotherapy. That was one reason hypnosis research appealed to me. Between 1959 and 1967, Hilgard and his colleagues published a series of hypnotizability scales, standardized inductions followed by sets of 9 to 12 hypnotic suggestions (Hilgard, 1965; Weitzenhoffer & Hilgard, 1959, 1962, 1967). A participant's hypnotizability was simply the sum of items passed on each scale, with passing determined by specific behavioral criteria. Scales ranged from those laden with suggestions for responses that could be explained in terms of simple motor compliance to those that demanded the ability to hallucinate and/or undergo (temporary) personality changes in response to hypnotic suggestion. The "gold standard" was a relatively balanced scale, the Stanford Hypnotic Susceptibility Scale: Form C (Weitzenhoffer & Hilgard, 1962).

Hypnotizability proved to be a remarkably stable, with test-retest correlations over 15 to 25 years of about .70 (Piccione, Hilgard, & Zimbardo, 1989). Thus, measured hypnotizability is about as stable as are multi-task measures of IQ. This stability combined with the inability of many researchers to find any personality correlates of hypnotizability (despite many attempts) led a number of hypnosis researchers, including me, to conclude that hypnotizability was a cognitive dimension, not an interpersonal one. Like an IQ test, the participant had to be willing enough to try to do each task in order for measurement to be valid, but willingness acted as a gatekeeper of, rather than reason for, hypnotizability scores (cf. Karlin, Hill, & Messer, 2005).

Given the ability to measure hypnotizability, a stable trait, it was relatively easy to differentiate the specific and non-specific effects of hypnosis. In this view, clinical effects that correlated with hypnotizability were seen as effects specific to hypnosis. Clinical effects that were uncorrelated with hypnotizability were non-specific effects involving factors common to most or all socially validated healing interventions. Non-specific in this context does not mean that these factors cannot be specified. Indeed, I am about to specify several of them and previous authors have been listing them for many years (cf. Fish, 1973; Frank, 1961; Hubble, Duncan, & Miller, 1999). Rather, these factors are deemed non-specific because they are common to all therapies and are not seen as basic and specific to any one treatment. These effects include (a) providing the role of patient to the sufferer, (b) forming an alliance between the patient (who is

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committed to getting as well as possible as soon as possible) and an authority figure, and (c) a patient's participation in healing rituals that are (in Orne's words) "often painful, humiliating and expensive" (Orne & Frankel, 1975). In addition, treatments for complaints more vague than a broken arm often require learning specific skills or taking potions of some sort. The hope or remoralization engendered by the treatment, whatever the treatment is, leads to increased willingness to attempt to think about and/or act in ways that solve problems (Frank, 1961). Further, the supportive nature of the doctor–patient relationship (and of other relationships enhanced as part of the healing rite) produces more positive expectations, a sense of self-efficacy, and further willingness to try to deal with avoided challenges.

This notion that the specific effects of hypnosis were those correlated with standardized hypnotizability scores (while non-specific effects were not), was most clearly stated by the late Kenneth Bowers (cf. Bowers & Kelly, 1979). This view became prevalent by the 1980s among hypnosis researchers and is supported by the evidence. For example, in a recent meta-analytic report on hypnotic effects on anxiety and stress, I reported that hypnosis had about an effect size of about .6 in the treatment of anxiety. This correlation is independent of hypnotizability and can be attributed to the non-specific effects of hypnosis as a healing ritual. Alternatively, hypnosis had about a .5, .8, or 1.2 effect size when used on acute pain, depending on whether a person was not hypnotizable (or barely hypnotizable), moderately hypnotizable, or highly hypnotizable, respectively (Karlin, in press). Highly hypnotizable research participants and patients are able to hallucinate the absence of pain to some degree, while the other two groups are not (cf. Karlin, Morgan, & Goldstein, 1980; McGlashen, Evans, & Orne, 1969; Raz, 2004). The changes associated with those with low hypnotizability scores (effect size about .5) and most of the effects for moderates (effect size of about .8) can be attributed to the non-specific effects of hypnosis. It is also interesting that simply calling a procedure "hypnosis" adds about half a standard deviation to the effectiveness of some cognitive behavior therapy techniques (Kirsch, Montgomery, & Saperstein, 1995). Making a placebo appear stronger strengthens its effects (cf. Evans, 1974).

This is yet another case where the effects of non-specific factors appear to be somewhere between .5 and .8. Unfortunately in terms of the theoretical orientation with which I identify, after the initial excitement wears off, these are the effect sizes associated with most cognitive-behavioral therapy (CBT) interventions for adults (cf. Wampold, 2001). Of course, there are exceptions. In terms of anxiety disorders, the work of Heimberg and his colleagues on social phobias usually has larger effect sizes (cf. Heimberg & Becker, 2002). Additionally, the only thing, including medications, that really works for OCD is exposure with response prevention (cf. Foa, Liebowitz, Kozak, Davies, Campeas, Franklin, et al., 2005). In fact, anything that can be treated with exposure (e.g., phobias) is clearly treatable with cognitive-behavioral therapy. Further, stuttering can be helped using a variety of behavioral techniques (Finn, 2003). These exceptions aside, when one removes the cloak of non-specific effects from most of the evidence base for evidence-based psychotherapy, one is left with a nearly naked emperor.

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Even the CBT evidence base we have is strongly tainted by allegiance effects along with all the other non-specific variables. Researchers study techniques they personally believe will be efficacious. They compare them to alternatives that no one wants to succeed and that are not as believable or structured as the new one. This is similar to product testing by Clairol in comparing its new shampoo to Brand X. Somehow, Clairol usually wins.

At best, efficacy and effectiveness trials emulate "nonblind" clinical drug trials. It is sometimes hard to remember how unconvincing such trials are. In fact, there have very few trials in which CBT techniques were compared to a pill placebo given by someone who believes he is administering an efficacious drug to at least some of the participants and who believes that psychopharmacology is important .[10]

There are exceptions to the lack of real evidence for CBT. Deconstruction studies have been enlightening, with behavioral activation for depression and unreinforced exposure for anxiety disorders being as good as more involved treatments (cf. Jacobson, Dobson, Truax, Addis, Koerner, et al., 1996). However, less encouragingly, in response to unflattering data from well designed studies (e.g., Elkin, Shea, Watkins, Imber, Sotsky, Collins, et al., 1989), we in the CBT field often find it acceptable to speculate that the CBT was not done well, especially if therapist adherence to the correct protocol was not measured. In this and other ways, CBT seems to have become a new orthodoxy, similar in arrogance and insularity to that found in psychoanalytic circles 40 to 50 years ago.

SOME DISQUIETING THOUGHTS ABOUT RCTS AND CLINICAL TRAINING IN CBT

Finally, I want to express concerns about one of the professional areas in which I am engaged, the training of CBT therapists. What do we in this field exchange for the seemingly fumbling and theoretically fuzzy work of therapists like Dr. Hamburg? Put another way, what are we teaching graduate students about doing cognitive-behavior therapy? I cannot speak for other programs, but our PhD program at Rutgers often sends students on internship after collecting data for or even completing their doctoral dissertation. In many cases, a great deal of their time is

[10] Incidentally, the model of a double-blind clinical drug trial is itself flawed in major ways. Drugs are almost never tested against an active placebo, something that causes side effects of any kind, never mind side effects that are in any way similar to those produced by the drug. Since both patients and doctors expect side effects, pretty soon the double-blind trial is not blind at all. That makes sense when one considers that drug development underlies a multi-billion dollar industry. The way "the game is played" is to compare a new medication to inactive placebos. This model is the result of the ongoing push/pull between the drug industry and government regulators. Drug trials are not meant to be intellectually satisfying experiments. From the point of view of drug developers, they are a necessary evil.. But we, as scientists, should not blindly accept in our own work the financially motivated flaws in the Federal Drug Administration drug testing system. Pharmaceutical companies have billions at stake. In our role as therapy researchers, we lacked that excuse, but many of us welcomed significant results favoring psychotherapy, especially dose dependent psychotherapy, even when the results were from studies with highly questionable methodologies (cf. Seligman, 1995).

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spent in the very randomized clinical trials (RCTs) I have been discussing. Such RCTs now routinely measure compliance by observing these students, who must and should present the techniques they are testing in a standardized way.

A few years ago, I was sanguine about such training procedures (cf. Karlin, 2002). I thought that we were seeing a true form of technology transfer and that participating in such trials would, at least, give students a familiarity with the clinical setting and something to do when they were on their own. I thought that spontaneity, rapport and flexibility would emerge as students became more experienced. I expected post-doctoral development similar to that reported by Hamburg in his own case, where he adopted hypnosis and Rogerian approaches as he learned their effectiveness in practice.

I am no longer so optimistic.[11] I think our students believe that the techniques they are testing are psychotherapy. In my view, they are not. They are techniques, procedures that are reasonably good at changing specific maladaptive thoughts and behaviors. In a similar vein, hypnotically oriented practitioners have long said that hypnosis is not a therapy, but a technique used in therapy. Therapy, in the broader sense, has to do with all the major ways in which a person's quality of life, function, and sense of meaning are disordered, not just which specific DSM-IV disorders are addressed.

The question of whether techniques shown to be efficacious in RCTs can be transferred to the clinic has received considerable attention. In general, the answer is a qualified "Yes." (cf. Shadish, Navarro, Matt, & Phillips, 2000).[12] But the parallel question, whether students who have been largely trained by being therapists in RCTs will do well as clinicians, has not been asked.

I, for one, am not encouraged. I think our students tend to see a structured, clinical, "SCID" interview (First, Spitzer, Gibbon, & Williams, 1997) for assessing an Axis I DSM-IV diagnosis as what they need to know about the patient. They see evidence-based procedures as how one treats a patient. Moreover, while there are exceptions, once a diagnosis is made, the

[11] One might dismiss the concerns I'm raising by noting that Rutgers students get good ratings on their internships, as if that demonstrated their competence. However, it is not clear how good those data are. Our students are bright, and hardworking young men and women from a highly selective, CBT oriented, clinical Ph.D. program. To determine the meaning of their positive ratings, one might examine the base rate for positive feedback among similar students on internship. I suspect that it is the rare student with the general characteristics of ours who gets poor ratings. That does not mean all is well. At a minimum, positive ratings mean that a student doesn't stand out from his or her peers as a poor colleague or clinician. Whether such ratings reflect careful, critical approval is another question. In any event, my concern is not that our students will be unacceptably poor therapists, but that they will be therapists with blinders, less good than they could be.

[12] My reading of the literature suggests that attempts to transport CBT into field settings have met with mixed success. Overall, obsessive compulsive disorder and phobias have yielded the best effectiveness and transportability trials for CBT. These are disorders treated with exposure, and exposure works. Far less success has been seen with other disorders, with even the outcome of CBT for depression giving one reason to pause (cf. Elkin et al., 1989).

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information flow in a CBT-based RCT is largely one way, with valued information from the patient involving compliance with the required homework regimen, the utility of the procedures they have been taught, and reported (hopefully reduced) symptoms. Thus, our CBT graduate students are learning how to treat specific disorders largely using psychoeducational rather than experiential approaches. They are not being trained, or not trained enough, to listen carefully, observe, be aware of nonverbal communication, discuss possible scenarios, and gently increase the understanding of their patients. These students are not being trained to be flexible and real, to model openness and receptivity, to interact with patients easily and spontaneously. They seem somewhat like new teachers busy with their lesson plans rather than interacting with, understanding, and helping the children with whom they are charged.

What they are doing may be enough with the rare, mono-symptomatic patient. But there are few patients like Lawrence and many like Betty. What about all the problems people have that don't emerge during a SCID? We are the only health professionals who actually spend hours with patients. We are the only ones likely to hear, eventually, about impulsive buying or chronic irritability, about the fact that every surface in the house is covered with papers to a depth of 6 inches, about alienation from children or parents, and so on.

Presenting problems are just that. Most of the time, I find that along with the presenting problem, I am treating people whose lives are not turning out as well as they could. With or without hypnosis, the presenting problem is often gone quickly and we go on to other things. For example, when patients with panic attacks come to me for therapy, we will establish that the panic attacks are not a physical threat of any kind nor is the patient going to lose control. Then, briefly, I will describe the multi-session approach to relaxation and exposure as a treatment developed in RCTs by David Barlow and his associates (e.g., Barlow & Craske, 1994); and I will note that it is there to be used if we need it. However, I will also suggest that panic attacks are like stubbing your toe. It hurts a lot for a short time, but, after a brief halt, one can go about one's business knowing that the pain will be gone quickly. Patients are encouraged to treat their panic attacks this way, sometimes in situations graduated for perceived stress, sometimes not. Meanwhile, we are off talking about other things, learning about their life, their feelings, the ways they get themselves in trouble, the narratives they create and so on (cf. Woolfolk, 1998).[13]

In sum, we ignore the Sam Hamburgs in our community at our peril. It is necessary to ground our work in the science of psychology. Otherwise, we will face more iatrogenic false memory and multiple personality destructive fads. However, we need to train students to treat people as well as disorders. Hamburg can be said to model doing both.

[13] In the last decade, I cannot remember any of my patients who needed to use Barlow's multi-session approach. These "stubbed toe" patients included one whose claustrophobia-related panic attacks were kept in remission despite a new job. The new job required him to occasionally visit the most secure parts of maximum security prisons (Karlin, 2002).

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