

***Response to Commentaries on Metaphoric Tasks in Psychotherapy:
Case Studies of “Margie’s” Self-Image and “Amy’s” Pain***

**What Are Case Studies Good For?
A Response to Commentaries by McMullen and Karlin**

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ABSTRACT

I am grateful to Linda McMullen (2018) and Robert Karlin (2018), for their commentaries on my case studies of Margie and Amy (Hamburg, 2018). Although case studies do not permit strong claims regarding treatment efficacy, they allow strong claims for the plausibility that treatments are efficacious. From a pragmatic standpoint, that is sufficient to justify proposing the treatments to other practitioners to be tried and tested by them, thereby ultimately contributing to the sum total of psychotherapy craft knowledge. On the topic of the placebo effect, the perspectives of researchers and clinicians, based as they are on different kinds of knowledge, can differ to the point of irreconcilability. What have hitherto been characterized as non-specific contributors to treatment outcome might better be classified as specific factors yet to be identified.

Key words: treatment outcome; placebo effect; non-specifics in psychotherapy; craft knowledge; case studies; clinical case studies

I want to thank Linda McMullen (2018) and Robert Karlin (2018) for their thoughtful comments on my two cases (Hamburg, 2018), and for their kind words. For one who generally works in isolation, such affirmation is welcome most of all because it is reassuring. Both commentators identified issues and raised questions that warrant discussion.

MCMULLEN

As I read McMullen’s account of the variety of forms and functions taken by metaphor in the two cases, I found myself repeatedly thinking, “Yes, that’s right.” That agreement was not borne of any familiarity on my part with the current thinking on metaphor—I have none. It is striking to me that the mostly spontaneous and unpremeditated transactions of psychotherapy are pervaded by this multifaceted use of metaphor. I take this as testimony to the essential role that metaphor plays in human thought and communication. I can imagine that if I had been conversant with recent theoretical and empirical work on metaphor, I would have noticed additional points in the therapeutic interaction where metaphor might have been used fruitfully.

I agree with McMullen that the metaphoric task was conceptually the weakest link in Amy's treatment. When I thought of adding it to the hypnosis, I knew I was reaching but I felt it couldn't hurt. Amy evidently thought it was relatively unimportant. On most days during the months we worked together she faithfully listened to three hypnosis recordings, devoting about an hour and a half per day to it. Yet on most days she was unable to find a few minutes for fitting in practice of the tapping pattern. The tapping pattern might have fallen victim to the early success of the hypnosis in reducing her pain. That began far in advance of her gaining any mastery of the pattern. So then, why endure the difficulty and frustration of trying to learn it?

McMullen does a better job than I did in crystallizing my ambivalence concerning the strength of the claim that can be made for the importance of the puzzle task in Margie's therapy. (McMullen must have been reading the case report very closely indeed to see through my hedge!) Mindful of the epistemological limitations of case studies, I felt that only a relatively weak claim of "value" could be made. Nonetheless, and especially considering the salience of the puzzle task in Margie's memory, decades later, of our work together, I do believe that it was the pivotal event in her therapy and of great therapeutic power in her case. I do not have the evidence to make a strong claim to that effect, but I think that the detailed evidence presented in the case report does justify a related strong claim: that it is *plausible* that the puzzle task was the pivotal event in her therapy and had great therapeutic power—and therefore plausible that it could operate with similar power in other cases similar to Margie's. From a pragmatic standpoint, this plausibility claim is significant and I will return to it later.

KARLIN

Karlin allows that Amy's "brain may have somehow created an amplifier, so that ordinary sensation becomes amplified into pain" (2018, p. 341), but he leans toward the view that her pain was psychogenic. He relates, as cautionary tales, two early cases of his own in which, he now believes, he did not adequately consider the psychological functions of the pain in those clients. Many years ago, at the state mental hospital, I was asked to work with a non-psychotic, middle-aged woman who had been hospitalized because of an obsessive-compulsive disorder so severe that it had become crippling. At that point I was more purely behavioral than I am now and made little attempt at understanding the life-historical context or possible psychological function of her disorder. I just went ahead and treated it with the usual behavior therapy treatment: modeling, exposure, and response-prevention. The treatment worked and she was able to leave the hospital, but I suspect that I did not alleviate her psychological suffering and that she did not come to a good end. Although I still operate behaviorally much of the time, I don't wear those behavioral blinders anymore; and this is one of those early cases that cause me to cringe when I recollect it. We all have such cases.

In Amy's case, almost 40 years after my state-hospital patient, I feel I did due diligence in searching for possible psychological functions of her pain. I couldn't find any. In view of the strange tricks our pain signaling system can play on us, e.g., phantom limb pain, I did not think it farfetched to assume that Amy's pain was neurological in origin, however obscure. I worked with Amy long enough so that if the pain had been serving some psychological function, we would have seen some kind of symptom substitution after it had been alleviated—but there was none.

Karlin devotes much of his commentary to discussion of what are variously termed (and indeed overlapping) contextual, non-specific, and placebo effects. He notes my difficulty in attributing Amy's improvement to any specific feature of the treatment. For me, though, this is not equivalent to the claim that the improvement was due to non-specific contextual effects. Just as psychogenesis is not the logically necessary default if a physical cause cannot be identified for experienced pain, so contextual, non-specific factors are not the necessary default if a specific aspect of treatment cannot be confidently identified as the cause of improvement. With respect to Amy, let me put it this way: the alleviation of her pain was caused by (a) the set of specific things we did in our meetings together, and (b) the things she did on her own—listening to the hypnosis recordings, perhaps even practicing the tapping pattern—as a result of those in-session events. And I will assert that if I had done a different set of things in our sessions, she would not have improved. What I am saying then is that a logically defensible alternative to attributing her improvement to non-specific effects is to attribute them to specific aspects of the treatment, difficult as it may be to identify which, or to explain exactly how they resulted in improvement.

Karlin asserts that “[t]he history of medicine and psychotherapy is the history of the placebo effect” (2018, p. 344). True as that may be with respect to the history of these pursuits, I am not sure it is so true now. For one thing it does not explain differential treatment outcomes. The hospital-based treatment centers for cystic fibrosis surveyed by Atul Gawande (2004) in that *New Yorker* article I cited were presumably outfitted equally well with the scientific and professional trappings conducive to the generation of positive patient expectations and a robust placebo effect, and they all conducted treatment according to the same protocol. Yet they had substantially different success rates. Gawande attributes the treatment variance to differences in the conscientiousness with which the protocol was administered. It could be argued, alternatively, that patients perceived this conscientiousness, were heartened by it, and therefore experienced an enhanced placebo effect. But this is farfetched. It is also a contradiction in terms, since the placebo effect is presumed to be non-specific and based on what healers are culturally identified as being rather than on the details what they do, which are necessarily specific.

Cases like Margie's and Amy's raise unanswerable challenges to the placebo effect. Margie had previously undergone two courses of psychotherapy from which, she emphatically said (at the time we met, and then at follow up decades later), she derived no benefit at all. I must presume that these professional clinicians possessed at least some of the professional regalia, and were able to muster some measure of non-specific therapeutic empathy and warmth, sufficient to yield at least a little placebo improvement—but there was no improvement.

Before Amy was sent to me she underwent a number of traditional and non-traditional medical therapies, including acupuncture. I believe that acupuncture is currently judged a more legitimate therapy, in the public mind, than hypnosis which still suffers under the stigma associated with hucksterism, people clucking like chickens, and *The Manchurian Candidate*. (And I have yet to see an accurate depiction of hypnosis on TV or in the movies.) If the placebo effect abides in treatments as a function of their cultural sanction, then why did Amy respond to the relatively suspect hypnosis rather than to acupuncture?

The placebo effect is a topic on which the perspectives of researchers and clinicians differ to the point of irreconcilability. Psychological phenomena look very different down here on the ground of quotidian clinical practice than they do from the rarified heights of randomized clinical trials and meta-analyses. In his discussion, Karlin references Kirsch, who has long and famously argued that anti-depressants are placebos (and whom I have argued against [Hamburg, 2000a]).¹ I am not competent to evaluate the adequacy of Kirsch's methodology and logic, but I will take it on faith that his methods are sound and lead ineluctably to the conclusion that anti-depressants are placebos. But in my practice I have seen that SSRI medications (like Prozac) which work just fine for severe depressive episodes in patients with a good, non-depressed, pre-morbid history, fail to effect any change or even worsen the depression of patients whose episode of severe depression is an exacerbation of a chronic-recurrent depressive disorder, or which emerges from a lifelong depressive temperament. In my experience, for these patients, bupropion-based medications (like Wellbutrin) are effective. If antidepressant medications are all a matter of smoke and mirrors, then why, for this chronically depressed group, does the magic work for one pill and not the other? Might it not be more reasonable to hypothesize that among the population of people currently suffering from a major depressive episode, there are two subgroups, one with a good premorbid history whose neurophysiology responds to a chemical that blocks the reuptake of serotonin, and one, with a poor premorbid history, whose neurophysiology responds to a chemical that blocks the reuptake of norepinephrine and secondarily dopamine?

I have seen this phenomenon enough times so that I can make my assertion about it with confidence. I *know* it. In my review (Hamburg, 2000b) of Fishman's seminal book (Fishman, 1999), I observed that clinicians and researchers both know things, but they know different things. They know what their work allows them to know. My work allows me to know things about the differential effects of anti-depressant drugs that Kirsch's work doesn't allow him to know. It is craft knowledge, deriving from the practice of the craft, the product of unsystematic but careful observation and uncontrolled but continual and unending experimentation in search of plausible cause-effect relationships. It is not traditional scientific knowledge but—as any expert knitter, cook, or plumber will attest—it is indisputably knowledge.

And this is where case studies come in. Craft knowledge is passed down in a variety of mediating interactions: formal teaching, apprenticeship, mentorship. But ultimately, it is all passed down in stories—whether over coffee with a colleague, or via the more formal vehicle of a written case study. As these stories are passed from one practitioner of the craft to another, they are transformed from personal knowledge into knowledge that is the property of the community of crafters. I tell Karlin a story about how I hypnotized somebody, and even if I can't scientifically prove that it works, it sounds plausible to him so he tries it, or something like it. And if it works for him, maybe he tells his students about it—and so the craft knowledge is disseminated. In my view, that, after all, is one of the projects of this *PCSP* journal: to facilitate the collection and dissemination of craft knowledge.

Since the very beginning, and continuing to the present day, I have looked to case studies for inspiration. I remember reading the case vignettes in *Behavior Therapy Techniques* (Wolpe

¹ Lest there be any misunderstanding, I am also against prescription privileges for psychologists.

& Lazarus, 1966) and excitedly fantasizing about how those techniques could be modified and extended to be useful for a variety of problems. Then almost thirty years later, I found inspiration for a hypnotic approach I used (and published in this *PCSP* journal [Hamburg, 2006]) in a case of Erickson's recounted by Haley (1973/1993) in *Uncommon Therapy*. My approach was so explicitly modeled on Erickson's that I sought permission to reprint his case as an appendix to mine.

Then just last year, I tried a therapeutic technique that I had encountered in a case report the year before. The patient was a 42 year-old, married, mother of a teen-aged daughter, well employed. Despite being attractive and of above average intelligence, she complained of low self-esteem and in particular of feeling easily overwhelmed in the face of untoward events or even large-seeming tasks, to the point of panic. The case was brief—only six sessions—straight CBT with the usual package of suggestions and interventions, and successful. My notes from the third session include, “Rx: Jigsaw puzzle, 1000 pieces, exactly 15 min per night.” The notes from the fourth read, “Puzzle gave her feeling of accomplishment. Thinking she had to do it for 15 min at a time made it easier.”

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