

- Cheek, D. B. (1994). *Hypnosis: The application of ideomotor techniques*. Boston: Allyn & Bacon.
- Elliott, R. (1984). A discovery-oriented approach to significant change events in psychotherapy: Interpersonal process recall and comprehensive process analysis. In L. N. Rice & L. S. Greenberg (Eds.), *Patterns of change: Intensive analysis of psychotherapy process* (pp. 249–286). New York: Guilford.
- Elliott, R. (1986). Interpersonal process recall (IPR) as a psychotherapy process research method. In L. S. Greenberg & W. M. Pinsof (Eds.), *The psychotherapeutic process: A research handbook* (pp. 503–527). New York: Guilford.
- Erickson, M. H., & Rossi, E. L. (1980). Varieties of double bind. In E. L. Rossi (Ed.), *The collected papers of Milton H. Erickson: Vol. 1* (pp. 412–429). New York: Irvington.
- Hammond, D. C., Garver, R. B., Mutter, C. B., Crasilneck, H. B., Frischholz, E., Gravitz, M. A., Hibler, N. S., Olson, J., Schefflin, A., Spiegel, H., & Wester, W. (1995). *Clinical hypnosis and memory: Guidelines for clinicians and for forensic hypnosis*. Des Plaines, IL: American Society of Clinical Hypnosis.
- Johnson, M. K., Foley, M. A., Suengas, A. G., & Raye, C. L. (1988). Phenomenal characteristics of memories for perceived and imagined autobiographical events. *Journal of Experimental Psychology: General*, *117*, 371–376.
- Loftus, E. F. (1993). The reality of repressed memories. *American Psychologist*, *48*, 518–537.
- Loftus, E. F., & Ketcham, K. (1994). *The myth of repressed memory: False memories and allegations of sexual abuse*. New York: St. Martin's.
- Lynn, S. J., Rhue, J. W., Myers, B. P., & Weekes, J. R. (1994). Pseudomemory in hypnotized and simulating subjects. *International Journal of Clinical and Experimental Hypnosis*, *42*, 118–129.
- Ofshe, R., & Watters, E. (1994). *Making monsters: False memories, psychotherapy, and sexual hysteria*. New York: Scribner's.
- Schacter, D. L. (1996). *Searching for memory: The brain, the mind, and the past*. New York: Basic Books.
- Spanos, N. P., Menary, E., Gabora, N. J., DuBreuil, S. C., & Dewhirst, B. (1991). Secondary identity enactments during hypnotic past-life regression: A sociocognitive perspective. *Journal of Personality and Social Psychology*, *61*, 308–320.
- Spanos, N. P., Burgess, C. A., & Burgess, M. F. (1994). Past-life identities, UFO abductions, and satanic ritual abuse: The social construction of

- memories. *International Journal of Clinical and Experimental Hypnosis*, *42*, 433–446.
- Spiegel, D., & Schefflin, A. W. (1994). Dissociated or fabricated?: Psychiatric aspects of repressed memory in criminal and civil cases. *International Journal of Clinical and Experimental Hypnosis*, *42*, 411–432.
- Stevenson, I. (1994). A case of the psychotherapist's fallacy: Hypnotic regression to "previous lives." *American Journal of Clinical Hypnosis*, *36*, 188–193.
- Terr, L. (1994). *Unchained memories*. New York: Basic Books.
- Weiss, B. (1988). *Many lives, many masters*. New York: Simon & Schuster.
- Yapko, M. (1993, September/October). The seductions of memory. *The Family Therapy Networker*, pp. 31–37.
- Yapko, M. (1994). Suggestibility and repressed memories of abuse: Survey of psychotherapists' beliefs. *American Journal of Clinical Hypnosis*, *36*, 163–171.

Rejoinder

One Case Study, Many Conversations: An E-Mail Correspondence on Research

William J. Matthews and Douglas Flemons

Dear Douglas,

I have received and read your and Kristin Wright's paper, but I am unclear as to what meaning you place on it. I do not mean this disrespectfully. As you noted, Loftus, Khilstrom, Lynn, Spanos, and others have already conducted a considerable number of controlled research studies. Their work offers convincing empirical support for the notion that client/hypnotist expectancies and the demand characteristics of the context can create paramnesias that, once created, have a high likelihood of being believed. Your demonstration, while interesting and well done, is, as you appreciate, a single case study,

and it thus allows very little in the way of conclusions. You have confirmed what we already know empirically, and your method precludes refutability of your findings. Given these limitations, I cannot see, at this point, a reason to publish your work.

I also find your concluding paragraph problematic. Why do you enclose the word *truth* in quotes? Are you taking an antirealism stance typical of postmodern therapists? The fallacy of such a position is highlighted when we confront issues of sexual abuse and recovered memory therapy. The client who believes she remembers being sexually abused in her family of origin is in a definite predicament. If her belief is correct (that is, if her memory is true), it will probably help her make significant changes to her relationships with other family members—appropriately so. However, if her belief is wrong (that is, if her memory is false), then the changes she may make in her family relationships could produce profound and irrevocable psychological damage. Without external corroboration of a memory, therapists should never encourage clients to act on what they *believe* to be true. Objective truth is essential if the client is to take prudent and therapeutic steps. Both the American Psychological and American Psychiatric Associations specifically warn their members against practicing so-called recovered memory therapies.

Sincerely,
Bill

Dear Bill,

Thanks for writing. I appreciate your frank comments and your willingness to talk about the issues. Not surprisingly, though, I disagree with you.

I believe Kristin's and my article *does* offer something unique, something that complements and adds to the work of Loftus et al. The paper introduces and explains how language contextually shapes cognitive process and how our implicit assumptions get packaged in the questions we ask. The demonstration and accompanying commentary illustrate and further nuance these ideas, and they provide a means for the "client" (Kristin) to have a voice. I know of no other article that has brought the issues of false memory alive in this way,

and I think Kristin's IPR commentary can help therapists better understand the potential phenomenological effect of their questions and statements. Kristin explains that she hadn't "attended to the presence of a man riding next to her" until the moment I asked her whether she was alone. She also points out that "the more D. F. questioned me, the more my awareness developed in focus and richness of detail." These comments clearly demonstrate the relationship between therapists' questions and clients' experiences.

You mention the problem of refutability. Are our theoretical ideas, clinical illustration, and IPR commentary refutable? No, not in the way the conclusions of a quantitative study can be. But qualitative research cannot and should not be judged by the same criteria used for quantitative research. Lincoln and Guba (1985), for example, discuss the various ways qualitative researchers can establish dependability, credibility, transferability, and confirmability, in contrast to reliability, internal and external validity, and objectivity.

Your criticism of our final paragraph stems, I think, from a misreading. Perhaps Kristin and I weren't clear enough. Our enclosing *truth* in quotation marks was not an instance of postmodern posturing; we simply wanted to underscore the fallibility of memory. We meant to say that therapists and clients should not go in search of an objective and certain truth, because they won't find it inside the client's memories. They may not even find it with "external" corroboration. A client recently asked me to help her remember whether her father had sexually abused her. Her sister believed herself to be a victim, and my client was hoping that hypnosis could confirm whether she too had been abused. But the sister's memory came to her with the "help" of a therapist, so could we reliably use it as an external benchmark to measure the veracity of whatever my client might recall? I thought not, and I told her as much.

You asked me about the meaning I place on the paper. I believe our work offers useful ideas about and illustrations of the relationship between a therapist's language practices and a client's experience. It also demonstrates the use of an interesting qualitative research method.

Yours,
Douglas

Dear Douglas,

I have not ruled out publication, but I am still not convinced. You carried out a one-shot case study—the very design that Campbell and Stanley (1963) considered the least reliable and valid. Such a design undermines the legitimacy of what you wish to claim and gives no certainty about what was happening in the situation. Kristin was a student who volunteered to participate in a hypnotic demonstration with you, her professor. You subsequently asked her to review a tape of the session and retrospectively to describe what she was thinking and feeling. What about the demand characteristics in these two activities? Would some other student have responded in a similar manner? Given the possible confounds, we can't be sure what was influencing/causing what. I did a similar study (Matthews & Langdell, 1984), a qualitative design in which we used 10 clinical cases. I could at least claim a consistent observable pattern of behavior over 10 subjects.

Ultimately, all research must be judged by the simple criterion of causality, or predictability, while ruling out alternative hypotheses. Qualitative research, in general, fares poorly when evaluated in this manner.

Your method (the one-shot case study) is shaky to start with, but even if we were to accept your conclusions, what have you said that hasn't already been demonstrated by the empirical researchers—Loftus (1993), Lynn (Lynn & Spanos, 1998), Khilstrom (1994, 1995), and so forth?

Sincerely,
Bill

Dear Bill,

Campbell and Stanley (1963) are great, aren't they? Their little book ranks up there with Strunk and White's (1979) *The Elements of Style* as a guide for clear thinking.

Yes, if you view our paper from their perspective, you should probably dismiss it, since "[one-shot case] studies have such a total absence of control as to be of almost no scientific value" (Campbell

& Stanley, p. 6). And what is their perspective? Remember that they were writing about experimental design, where "variables are manipulated and their effects upon other variables observed" (Campbell & Stanley, p. 1). According to Moon and Trepper (1996), "Experimental researchers [have for a long time] incorrectly equated the case study with one of the weakest types of quasiexperimental designs: the one-shot case study. . . . Recently, however, there has been a revival of interest in case study research in several fields" (p. 393).

I am suggesting, then, that you not evaluate our work from an experimental-design perspective. Kirk and Miller (1986) point out that "hypothesis testing is not the only research activity in any scientific discipline. . . . Qualitative research[,] . . . in being intrinsically exploratory, . . . explicitly departs from certain strictures of the hypothetico-deductive model" (p. 17). Moon and Trepper (1996) explain that "informal case study research usually is descriptive and discovery-oriented" (p. 395). According to Patton (1990), "Case studies . . . become particularly useful where one needs to understand some special people, particular problem, or unique situation in great depth, and where one can identify cases rich in information—rich in the sense that a great deal can be learned from a few exemplars of the phenomenon in question" (p. 54).

My demonstration with Kristin was a unique situation. She is an articulate, thoughtful person who was open to the experience and interested in phenomenologically examining and theoretically exploring what happened to her. I have done similar "past-life" demonstrations with a number of other people (two of which are described at the end of the paper and which offer some degree of comparison), but these other participants weren't able—for whatever reason—to examine their experience both passionately *and* dispassionately. Recognizing Kristin's unique gifts, I suggested we team up to examine, via an IPR-informed method of investigation, what had happened and why.

Of course, case studies have limitations: "Without control, generalizability is limited. . . . Without control, replicability is difficult. And without control, . . . the validity of the findings comes into question. . . . [Therefore, case-study] researchers must be humble in their presentations, . . . and they must at all times communicate

the limitations of [their] approach" (Moon & Trepper, 1996, p. 400).

I believe we have properly acknowledged the limitations of our work, but we also haven't let such limitations keep us from acknowledging and prizing the unique circumstances that made our work possible. We have taken the opportunity to explore how "words do things and are used by speakers to achieve particular results" (Austin, 1962, cited in Gale, 1996).

You say that "ultimately all research must be judged by the simple criterion of causality, or predictability, while ruling out alternative hypotheses." Well, perhaps all *hypotheses* need ultimately to be judged this way, but as researchers, we need to be able to engage in both strict *and* loose thinking (Bateson, 1972). "From case reports we learn both propositional and experiential knowledge. . . . When the researcher's narrative provides opportunity for vicarious experience, readers extend their memories of happenings. Naturalistic, ethnographic case materials, to some extent, parallel actual experience, feeding into the most fundamental processes of awareness and understanding. . . . Case study researchers assist readers in the construction of knowledge" (Stake, 1994, p. 240).

You note that Loftus, Lynn, and Khilstrom have demonstrated empirically what Kristin and I demonstrate clinically—implying, then, that our research is not important because the testable hypotheses have already been formulated and tested. Denzin (1978) discussed the importance of triangulation of data for establishing the trustworthiness of research results. He differentiated four ways that triangulation can and should be done: using multiple and different sources, methods, investigators, and theories (cited in Lincoln & Guba, 1985, p. 305). So, minimally, we provide some triangulation for the empiricists' results: We look at false memory with a different method, we are different investigators, and we offer a different theory to explain the phenomenon. But I think we do much more than this. As I said earlier, we provide a first-hand account of the experience of a recipient of hypnosis, we include a detailed commentary by the hypnotist of how he was "doing things with words," and we tie these two commentaries together. Readers are thus given a theoretical template with which to follow the unfolding of the demonstration and the moment-to-moment relationship between

suggestion and experience. This should have direct relevance for clinicians, and it might even tickle the imagination of an experimental researcher or two.

Yours,
Douglas

Dear Douglas,

You are right—you did acknowledge limitations in your study. But this does not overcome or negate them. In my reading of your case example, Kristin did not seem to actually believe her experience. Yet even if she did believe it, I don't see what we get compared to the existing body of empirical research on this issue.

Patton (1990) and Moon and Trepper (1996) offer some interesting ideas about case studies, but they say nothing (at least in the passages you quote) about how to overcome the weaknesses articulated by Campbell and Stanley (1963). Increased intensity (i.e., richness, etc.) of the case material certainly does not overcome the design problems. If anything (as amply demonstrated by Freud) intense case studies *increase* confirmatory bias. At best, a case study, particularly if it is repeated with other cases, should give rise to a refutable hypothesis, testable by an appropriate experimental design. If you were to do a series of demonstrations that resulted in similar experiences for the subjects, the cumulative effect would, in my opinion, provide stronger support for some of your assertions.

At this point, Douglas, I can't see publishing the article, but I am still pondering, and I appreciate your arguments.

Sincerely,
Bill

Dear Bill,

Since you like my arguments, allow me to give you a few more. As I mentioned in an earlier post, Lincoln and Guba (1985) provide guidelines for how researchers can ensure the trustworthiness of their studies. Some of the ways they suggest doing this involve the following.

1. Member checking: going back to the people involved in the study and asking them for their assessments of your results.

Kristin and I provided member checking for each other, but we also had students in the class read our paper and provide feedback. They helped to determine that we weren't just "making it up."

Near the end of the manuscript, we discuss two of my other demonstrations. Before sending our paper to you, I sent a draft of my descriptions of them to the two individuals involved. I subsequently went through two rounds of revisions with the woman who heard bagpipes, until she said, "Yes, that accurately reflects my experience." The woman who thought me not "New-Agey" enough similarly helped me refine my description, though it took only one draft until she thought I had it right.

2. Peer debriefing: "exposing oneself to a disinterested peer in a manner paralleling an analytic session and for the purpose of exploring aspects of the inquiry that might otherwise remain only implicit within the inquirer's mind" (Lincoln & Guba, 1985, p. 308).

We had a number of colleagues read and critique the paper, our ideas, and our conclusions.

3. The audit trail: keeping track of decisions and choices made at each stage of the inquiry so that subsequent researchers can evaluate what the authors did.

We maintained a record of our choices and decisions in what Lincoln and Guba call a reflexive journal.

4. Thick description: I'm sure you're familiar with Geertz's term.

You make reference to Freud's in-depth case studies as an example of how increased levels of description don't take care of (and perhaps even exacerbate) the problem of confirmatory bias. Note, though, that Freud didn't study transcriptions of audiotaped *interactions*. He stayed inside his head, giving *his version* of what happened and his theoretical spin on it. We do nothing of the sort. The data we draw from can be checked against the tape, the interaction can be followed by reading the transcript, and our conclusions can be evaluated according to the criteria above. You make the point

that acknowledging the limitations of the study does not overcome or negate them. True, but there are limitations in all studies. If you get great reliability, you do it at the expense of contextual relevance. Yet how many editors say, "I can't publish this because error variance was controlled for too successfully?"

You say you still don't see how our study can measure up to the existing empirical research on the issue of false memory. Empirical research allows you to compare abstractions within and between groups. This can be useful, of course. But it, too, is limited. Even Campbell and Stanley (1963) point out that if we decide that only data generated via the Solomon Four-Group design is worth our attention, we will miss a hell of a lot. As Leibniz put it, "Those pure mathematicians and physicists, who are ignorant of and despise all other forms of knowledge, are wrong" (in Grafton, 1997, p. 63). I think we have to give credit to readers, letting them decide (after we have forthrightly explained the limitations of our work) how much or little stock to put into the studies they read.

So what do you get from our study? You get the client's perspective, transcribed verbatim; you get the hypnotist's ideas and his techniques; you get to see the interaction between the hypnotist and the client, and you get a theoretical discussion that ties it all together. I consider the package valuable and important. If you don't, I'm sure there is nothing more at this point that I can say to convince you. I can't make you think like a qualitative researcher.

I agree with your recommendation regarding my including additional case studies as a way of providing stronger support for our assertions. However, further investigations of this sort may well be impossible to conduct, which is why I think this study, with all its faults, represents something singularly valuable. Let me explain a little.

I bend over backwards to treat clients and demonstration participants (and students, etc.) with respect. I have performed various "past-life" memory-creating demonstrations over the past five years, and they have consistently provided the participants and the observers with great opportunities for learning. However, the person who ends up experiencing a vivid "memory" typically feels put on the spot. Kristin was unique because she could go through the experience and then listen to me talk, without getting insulted or upset, about how I contrived, with language, to determine her experience.

The woman who heard the bagpipes knew from previous classes she had had with me where I stood on the issue of past-life regression. Yet she was surprised, after our demonstration, when I started talking about what *I* had done to shape her "memory." If you remember, we say in the paper that she believed I had no influence on what happened to her. She still has no sense of my having influenced her "memory." Despite my best efforts at the time to be respectful, despite my carefully chosen words, she felt that I was discounting her experience. She believed she had gotten in touch with a genetically inherited memory that had been buried in her unconscious. The demonstration provided or confirmed for her an understanding of why she had never liked the sound of bagpipes. But my postdemonstration commentary directly contradicted her experience, and she felt betrayed. When I was writing the paper, I contacted her, and we had some long talks that eventually repaired (I think, I hope) our teacher-student relationship, and she gave me permission to describe what had happened. As I said earlier, she then, at my urging, critiqued my descriptions until I had, in her words, "got it right."

Sure, a study that described the results of 10 demonstrations would be an improvement. But the one detailed here (plus the two brief descriptions of demonstrations that are juxtaposed to it) offers a unique glimpse into a therapist-client relationship and the language practices and phenomenological experiences that go along with it. The readers are damn lucky to have at least that. No, we didn't use a Solomon Four-Group design, but our research *can* help readers think more clearly about what they do and the assumptions they hold. And if that isn't relevant, I don't know what is. Have you never learned something important from only one experience? Perhaps you can't put a "refutable" stamp of approval on it, but you *can* say, "My, that's worth paying attention to, particularly given what I know about this from other people."

One more point: Single case studies don't afford cross-case comparisons, but they do make possible, because of their intensive focus, *cross-instance* comparisons. We highlight language practices that produce similar effects *at different times* in the demonstration.

Yours,
Douglas

Dear Douglas,

While we have been writing back and forth over the past few weeks, I have been reading a piece by Schneider (1998) that argues for the revival of romanticism in psychology. Although I find much of his discussion to be a call for fuzzy thinking, he cogently defends the validity of multiple case studies for testing rival hypotheses. This idea, consonant with your notion of triangulation, has helped sway me in favor of your position. After thinking long and hard about your paper, John and I have decided to publish it, because we think, taken as a whole, the strengths outweigh the weaknesses. I think you did a good job in stating your case (no pun intended). As you said earlier, let's let the readers judge for themselves.

Sincerely,
Bill

REFERENCES

- Bateson, G. (1972). *Steps to an ecology of mind*. New York: Ballantine.
- Campbell, D. T., & Stanley, J. C. (1963). *Experimental and quasi-experimental designs for research*. Boston: Houghton Mifflin.
- Gale, J. (1996). Conversation analysis. In D. H. Sprenkle & S. M. Moon (Eds.), *Research methods in family therapy* (pp. 107-124). New York: Guilford.
- Grafton, A. (1997, November). Birth of the footnote. *Lingua Franca*, 7, 59-66.
- Kihlstrom, J. F. (1994). Exhumed memory. In S. J. Lynn & N. P. Spanos (Eds.), *Truth in memory* (in press). New York: Guilford.
- Kihlstrom, J. F. (1995). The trauma-memory argument and recovered memory therapy. In K. Pezdek & W. P. Banks (Eds.), *The recovered memory/false memory debate* (1998). San Diego, CA: Academic.
- Kirk, J., & Miller, M. L. (1986). *Reliability and validity in qualitative research*. Newbury Park, CA: Sage.
- Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Newbury Park, CA: Sage.
- Loftus, E. F. (1993). The reality of repressed memories. *American Psychologist*, 48, 518-537.
- Lynn, S. J., & Spanos, N. P. (Eds.). (1998). *Truth in memory*. New York: Guilford.

- Matthews, B., & Langdell (1984). What do clients think about the metaphors they receive? *American Journal of Clinical Hypnosis*, 31, 242-251.
- Moon, S., & Trepper, T. (1996). Case study research. In D. H. Sprenkle & S. M. Moon (Eds.), *Research methods in family therapy* (pp. 393-410). New York: Guilford.
- Patton, M. Q. (1990). *Qualitative evaluation and research methods*. Newbury Park, CA: Sage.
- Spanos, N. P., Burgess, C. A., & Burgess, M. F. (1994). Past-life identities, UFO abductions, and satanic ritual abuse: The social reconstruction of memories. *The International Journal of Clinical and Experimental Hypnosis*, 42, 433-446.
- Stake, R. E. (1994). Case studies. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (pp. 236-247). Thousand Oaks, CA: Sage.
- Strunk, W., & White, E. B. (1979). *The elements of style* (3rd ed.). New York: Macmillan.