

---

## Against Scientism in Psychological Counselling and Therapy: A Response

---

Beth E. Haverkamp

*University of British Columbia*

---

Science is an art—it isn't a science.

—GOETHE

I welcomed the challenge that Martin's (1995, this issue) paper provided to my own presuppositions and view of research. However, the conclusions that we have each reached after considering the current state of affairs are quite different; I was not convinced by Martin's arguments that we have fallen prey to "scientism," or an overvaluing of science and the scientific methods. It is my belief that, far from overvaluing traditional scientific methods, we treat them with careless disregard and would benefit from their conscientious application. But, before noting my disagreement, I would like to discuss the final section of Martin's paper, which deals with the idea of psychological counselling and therapy (hereafter psychotherapy) as an area for moral reflection.

### *Moral Reflection in Psychotherapy*

A number of the ideas presented here were like a breath of fresh air, and widened my own perspective. As an academic who has written and taught in the area of ethics, I had never considered the near total absence of moral philosophic studies that Martin (1995) has noted. Given our field's early roots in philosophy, as reflected in the pioneering contributions of William James and renewed interest in his writings (e.g., Leary, 1992), the lack of attention to substantive ethics is remarkable.

The failure to consider moral philosophy is an area where I think Martin does make a case for scientism. Paradoxically, it may have been the early psychologists' knowledge of philosophy, and its centuries of attention to the biased and selective nature of human perception, that led to the suspicion of subjective experience. Euripides is credited with saying, "Among mortals second thoughts are wisest"; in some ways, the scientific method is an enshrinement of systematic procedures intended to guarantee "second thoughts."

Perhaps we have taken application of the scientific method too far. I agree with Martin's argument that an overreliance on the scientific method has limited our view of appropriate areas of inquiry and that this narrowing of the domain has resulted in overlooking a whole mode of reflection and examination. Our field is poorer as a result. On the other

hand, I welcomed his qualification that traditional science not be displaced, as that brings me to the portions of Martin's work where we do have different perspectives.

*Charges of scientism in psychotherapeutic research*

Martin (1995) charges that psychotherapeutic practitioners and researchers have overvalued science and have erroneously claimed a solid scientific base for the field, primarily via inappropriate claims of causation in research. In support of this claim, he recaps Eysenck's (1952) challenge to the effectiveness of psychotherapy but minimizes the subsequent findings that 75% of clients improve more than untreated control group members (Smith & Glass, 1977; Smith, Glass & Miller, 1980). Dobson's (1989) recent meta-analysis of cognitive therapy for depression has shown that clients receiving cognitive therapy improved more than 98% of untreated controls, than 70% of those receiving psychotropic medication, than 67% of those receiving behaviour therapy, and than 70% of those receiving other therapies (e.g., interpersonal, insight). Viewed from a consumer perspective, the probability of ending up in better shape than 75%-98% of non-treated control group members sounds pretty good.

The point of citing such research is not to reopen the question of whether psychotherapy is effective, but rather to raise a concern that Martin (1995) has selectively cited evidence to support his criticisms of "psychotherapy's aspirations to science" (p. 290). For example, in arguing that researchers are "expressing or assuming a very tight relationship between mental disorders and particular forms of psychotherapeutic intervention" (p. 291), he quotes Kendall and Clarkin's (1992) statement that ". . . professional treatment would be markedly simplified if each disorder had a set of non-overlapping symptoms" (p. 833). However, Kendall and Clarkin go on to say, "The aspiration is laudable. The diagnostic situation . . . remains much more complicated than preferred" (p. 833). I would argue that Kendall and Clarkin are emphasizing the opposite of a "tight" relationship between disorders and treatment and that their quotation identifies the limits of science, rather than providing an example of its overapplication and, that Martin's citation of it is imprecise.

*The value of felt experience*

Martin (1995) also contends that the overvaluing of science has occurred at the expense of other forms of knowledge, particularly the phenomenological. I value the contributions of "felt experience" as a source of knowledge but, at the same time, I believe that a scientific examination of the character of felt experience highlights its limitations and conversely, the value of the scientific method. As an example, consider the study and

experience of heart disease, where intuition from felt experience probably helped identify the detrimental effects of stress and the protective effects of social support and pet ownership. On the other hand, even if a smoker says “cigarettes help me relax,” science has convincingly demonstrated that this is one area where “felt experience” is not a trustworthy guide to healthy behaviour. The point to be noted is that the weaknesses or limitations of both methods do not invalidate the value of either.

Decisions about which method to employ must necessarily take the client, and his or her ultimate welfare, into account. Our clients employ us (read: pay us money) to provide trustworthy information or experiences that have a reasonable chance of accomplishing their therapeutic goals. We have an ethical responsibility to decide when, or if, felt experience is a trustworthy guide to therapeutic decisions. The popularity of magazines such as *Consumer Reports* reflects the public’s appreciation of objective opinion as one component of “trustworthy.” Many of us have an intuitive recognition that a controlled test of multiple automobiles, rather than one person’s perception, is a better indicator of whether a particular model will be reliable. I have a concern that we not elevate the value of felt experience without acknowledging the risks of its application.

#### *Investigation of psychotherapeutic processes*

One of Martin’s (1995) arguments against “placing such a high value on science in psychotherapy” (p. 292) is his belief that it is near-impossible to establish causal claims in psychotherapy research; he also contends that much of the literature advances such claims. One aspect of Martin’s argument, based on the comparison with physical objects, is that psychotherapeutic phenomena cannot be studied in analogue or laboratory situations, and that the act of study is likely to fundamentally alter the process under observation. I disagree with the first statement, and would argue that Martin has overemphasized the difficulties with the latter.

First, let us consider the notion that psychotherapeutic processes cannot be investigated by traditional means. Martin (1995) argues that operational definitions, where “phenomena of interest are equated with elements of their physical display” (p. 292) are not useful. In making this statement, he seems to be limiting the broad area of process research to its most micro-level exemplars, where words or gestures are counted and coded, or is ignoring the long-established tradition of using self-report as an “observable.” For clients, the act of marking “true” on a paper-pencil depression inventory rests on a continuum with volunteering the statement, “I get pretty depressed sometimes” in a counselling interview. In both cases, we rely on the client to be an accurate, self-aware reporter of his or her inner experience. The client’s expression of that inner self becomes “observable data.” This is one reason we cannot fully disqualify

lab or analogue studies; the phenomena of interest is present in many cases. People still carry their representational systems “within.” The issue of generalizability is still to be addressed, because a lab will not have all the elements of a naturalistic setting, but it is worth noting some of the successes that psychologists have had in this regard.

Gottman’s (1994) programmatic research on marital stability addresses both of Martin’s (1995) contentions: that meaningful human processes cannot be studied in the laboratory and that such processes will be so altered that any information obtained will be irrelevant. Gottman invited couples to interact in a controlled laboratory setting, collected data on a range of physical and psychological “observables,” and was able to accurately predict marital status (separated, not separated) four years later for 90% of the couples in his study. Even more provocative (and positivistic!), the physiological data alone (heart beat, blood flow, perspiration, activity level) predicted changes in marital satisfaction three years later with 95% accuracy. This research targets the affect-laden, “meaning-rich” system of marriage through a very positivistic, micro-level quantitative analysis. Yes, there is more to be learned from the felt experience of each of the couples who volunteered, but the contribution made by traditional science needs to be acknowledged.

### *Causation and Causism*

The core of the first half of Martin’s (1995) paper offers a thoughtful discussion of causation and the difficulty our field has had in resisting inappropriate causal claims. That this is an enduring problem is evidenced by the regularity with which students are taught to say “correlation is not causation,” yet still produce papers saying, “the  $r = .62$  indicates that counsellor empathy produces client change.” However, when Martin says that “a great deal of [psychotherapeutic] literature and practice are scientific” (p. 300) because causal links are claimed or implied, I disagree with his interpretation of this phenomenon.

Where Martin (1995) believes that much of our research reflects inappropriate causal claims, and cites this as evidence of “scientism,” I contend that, rather than reflecting an overappreciation for science, it actually constitutes evidence for the opposite: a shoddy, careless attitude toward application of the scientific method. The problem seems to lie in the level of scientific understanding achieved or applied by some students, practitioners and academics.

Where Martin (1995) defines “scientism,” in part, as an effort to establish causal claims when none are possible, and attributes this to being uncritically enamored of science, Rosenthal (1994) casts the issue in ethical terms. He points to the inappropriate application of the scientific method and uses the term “causism” to denote “the tendency to imply a causal relationship where none has been established (i.e., where

the data do not support it)" (p. 128). To Rosenthal, bad science is bad ethics.

In Rosenthal's (1994) view, the appearance of causism has no necessary implications for the choice of a scientific paradigm, but is more a question of ethics and education. As he says,

If a perpetrator of causism is unaware of the causism, its presence simply reflects poor scientific training. If the perpetrator is aware of the causism, it reflects blatantly unethical misrepresentation and deception. Poor quality of research makes for poor quality of education as well . . . if [students] are required to participate in poor quality research, they are likely to acquire only misconceptions about the nature of science and of psychology . . . they have been poorly served educationally as part of having been misled scientifically." (p. 128)

The Canadian Guidance and Counselling Association (CGCA), in offering guidelines for research training, has recognized Rosenthal's (1994) claim that bad science, or uninformed science, is unethical. A review of case examples provided by the organization suggests that concern exists over the tendency of some graduate programs to defer to student preferences (and, I would say, anxieties) about quantitative research. Two case examples of unethical practice that were listed in the recent CGCA *Counselling Ethics Casebook* (Schultz, 1994) were: (1) programs that fail to include requirements for coursework in statistics and research methods because students complained about difficulty they were having; and (2) a faculty member who waived a program requirement that students take an advanced research course.

I believe that Martin's (1995) paper successfully challenges the quality of poor research that is conducted, but I do not believe this constitutes evidence for overvaluation of science. My primary concern about his paper is that an uncritical audience may adopt his arguments as support for an existing opinion that "I never liked statistics or quantitative research, now I can feel good about not liking them and not learning about them." I am certain that is not his intention.

I believe that Martin (1995) and I can find common ground in recommending that both traditional and newer, more subjective methodological approaches be taught and employed, and that researchers and practitioners be expected to develop an appreciation of the merits and limitations of each paradigm. Martin cites Schrag's (1992) point that many qualitative, nonpositivistic researchers continue to draw conclusions that "seem to assume some sort of causal relationships among the phenomena studied" (Martin, 1995, p. 299). The fact that phenomenological researchers also fall into a causal trap seems to reveal the very reasons that we should not abandon traditional empirical science. Causism, in a quantitative or a qualitative researcher, is not evidence of scientism, but of careless research, uncritical thinking, or poor training.

Stan Strong (1984) captures the value of traditional methods, alongside an appreciation of subjectivity, when he says:

Because no human can be objective, the edifice of knowledge rests not on objectivity, but on intersubjective agreement. Scientific knowledge is therefore an organic, evolving construance of reality. . . .

The purpose of science is to generate intersubjectively valid and pragmatically useful construances of reality . . . over the generations, constraints on the method of inquiry have evolved, mainly to counteract the basic human tendencies to interpret events within one's preconceptions and to act such as to create what one expects. (p. 472)

Because we so readily see what we hope and expect to see, I would agree with Strong that we need to acknowledge our fallibility yet continue to search for the best approximations to reality that we can generate. Scientific methodology is a primary tool in that quest but, our continuing challenge is to ensure that, when we "do science," we do so with all the care and critical thought that are its hallmarks.

### References

- Canadian Guidance and Counselling Association. (1989). *Guidelines for ethical behavior*. Ottawa: Author.
- Dobson, K. S. (1989). A meta-analysis of the efficacy of cognitive therapy for depression. *Journal of Consulting and Clinical Psychology, 57*, 424-29.
- Eysenck, H. J. (1952). The effects of psychotherapy: An evaluation. *Journal of Consulting Psychology, 16*, 319-24.
- Gottman, J. M. (1994). *What predicts divorce? The relationship between marital processes and marital outcomes*. Hillsdale, NJ: Lawrence Erlbaum.
- Kendall, P. C. & Clarkin, J. F. (1992). Introduction to special section: Comorbidity and treatment implications. *Journal of Consulting and Clinical Psychology, 60*, 833-34.
- Leary, D. E. (1992). William James and the art of human understanding. *American Psychologist, 47*, 152-60.
- Martin, J. (1995). Against scientism in psychological counselling and therapy. *Canadian Journal of Counselling, 29*(4), 287-307.
- Rosenthal, R. (1994). Science and ethics in conducting, analyzing and reporting psychological research. *Psychological Science, 5*, 127-34.
- Schultz, W. E. (Ed.). (1994). *Counselling ethics casebook*. Ottawa: Canadian Guidance and Counselling Association.
- Smith, M. L. & Glass, G. V. (1977). Meta-analysis of psychotherapy outcome studies. *American Psychologist, 32*, 752-60.
- Smith, M. L., Glass, G. V. & Miller, T. I. (1980). *The benefits of psychotherapy*. Baltimore: Johns Hopkins University Press.

### About the Author

Beth Haverkamp is Associate Professor of Counselling Psychology at the University of British Columbia. She has conducted research on perceptual processes in counselling and teaches courses in research methodology, psychological testing, and ethics.

Address correspondence to: Beth Haverkamp, The University of British Columbia, Faculty of Education, Department of Counselling Psychology, 2125 Main Mall, Vancouver, B.C., V6T 1Z4.