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## COMMENTARIES

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### Science and Psychodynamics: From Arguments About Freud to Data

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Freud died over 60 years ago, and his work, like that of all influential thinkers, is a mixture of truth and falsehood. It is not difficult to pick out the most salient of the falsehoods, weave them together, and make a compelling case, for example, that Freud was ignorant of demand characteristics, preoccupied with sexuality, too sweeping in his claims, too quick to yield to confirmatory biases, and so forth—all of which are manifestly true. We suspect that someone who decided to spend his career reading every word Skinner, Piaget, Chomsky or, for that matter, Isaac Newton ever wrote could similarly find errors in logic, faulty assumptions, sweeping claims for which the evidence was clearly inadequate, methodological mistakes by standards that have evolved since, and so on.

Macmillan (this issue), like many of Freud's most passionate critics, cuts off Freud's arms and legs and then proclaims that Freud cannot move. If one throws out all data other than clinical data based on free association as Macmillan does in the first paragraph, dismisses reviews of scientific literature with epithets rather than careful refutation of evidence (as he does in a footnote, declaring a review published in *Psychological Bulletin*, which summarized hundreds of studies, a "mixture of bland generalization and anecdote"), and if one ignores anything written by psychoanalysts since 1939 as irrelevant, then one is well justified to wonder "what is left to psychoanalysis if the rule by which its data are gathered is not objective, if the methods by which those data are translated are indeterminate, and if the standards cannot be formulated for evaluating the constructions and histories into which they are forged" (Macmillan, this issue).

Like Frederick Crews (1993), who has similarly made a career out of attacking 1900 Freud and then concluding that all of psychoanalytic thought is bank-

rupt (a logical error as large as any Freud ever made, and likely a good example of the kind of motivated distortion Freud spent his career trying to understand), Macmillan has many strong logical arguments against some of Freud's logical (or illogical) arguments. When he ventures anywhere near science—which, ironically, is almost never—he makes precisely the kind of sweeping, ill informed statements for which he faults Freud. Consider the following assertions: "Nor is much attention paid [in any literature defending Freud] to the reliability of interpretation", "In no formal study of the reliability with which psychoanalysts interpret dreams has minimal agreement been obtained. Nor have analysts found it possible to agree on the manifestations of transference, on the essentials of an analytic process, or even on definitions of psychoanalysis;" and "It is not that psychoanalysts cannot be trained to analyze behavior or to use rules to assign its elements to categories. It is simply that ... [n]othing exists against which they can hone their skills as translators or interpreters" (Macmillan, this issue).

#### When Science Meets Assertion

Let us look at some of the data pertaining to Macmillan's assertions and see how they fare. Has any attention been paid to reliability and validity? Macmillan might want to read a recent meta-analysis comparing the Minnesota Multiphasic Personality Inventory to the Rorschach test, which found equivalent effect sizes for the two instruments (Hiller, Rosenthal, Bornstein, Berry, & Brunell-Neuleib, 1999). That article is not easily assailed as bland and anecdotal, and one of its authors, Robert Rosenthal, is not widely known as a rabid Freudian. Macmillan might also read

the empirical literature on object relations, in which investigators have coded psychotherapy hours, narratives, Thematic Apperception Test responses, and other projective responses, generally with reliabilities well above  $r = .80$ , and which have shown predictive validity with respect to a whole host of measures, ranging from diagnosis to interpersonal behavior (e.g., Blagys & Hilsenroth, 2000; Nigg, Lohr, Westen, Gold, & Silk, 1992; Stricker & Healey, 1990; Westen, Feit, & Zittel, 1999). He might take a look at 3 decades of work on attachment, a theory developed by the psychoanalyst John Bowlby, who Macmillan cites approvingly. Research in this area has produced impressive findings on the relation between variables such as the longitudinal course of interpersonal development and attachment status coded from behavioral observations of infants, projective stories told by children, and interpersonal narratives told by adults (e.g., Cassidy & Shaver, 1999). Or he might want to peruse the work of Jack Block (1978), one of the leading psychometricians in the history of the field, who somehow managed to develop a highly reliable *Q*-sort instrument for assessing personality processes that was highly influenced by psychoanalytic theory.

Is it true that psychoanalysts cannot agree on manifestations of transference? One can only maintain this claim if one ignores three decades of work by Lester Luborsky and his colleagues (Luborsky & Crits-Cristoph, 1998) and recent work by Susan Andersen, who has even experimentally demonstrated transference processes using subliminal presentation of stimuli (Glassman & Andersen, 1999). And have researchers failed to study the therapeutic process in psychodynamic psychotherapy? This would probably come as a surprise to Hans Strupp, who spent his professional career on that task (Strupp, 1993), and Enrico Jones, who has developed a highly reliable and valid *Q*-sort instrument for assessing the range of therapeutic orientations, including psychoanalytic (Ablon & Jones, 1998).

Finally, is it true that even if psychoanalysts were to get reliable in making inferences, "nothing exists against which they can hone their skills as translators or interpreters" (Macmillan, this issue)? Researchers have, in fact, validated a host of constructs and measures, including measures that code narratives or quantify clinical judgments, by comparing them against external criteria that allow researchers to determine whether these interpretations actually predict anything real (e.g., Siegel & Weinberger, 1998; Westen, Muderrisoglu, Fowler, Shedler, & Koren, 1997; Westen & Shedler, 2000). Macmillan might also consult studies using experimental methods, such as studies finding that subliminal presentation of verbal and nonverbal stimuli can influence behavior in highly specific ways (e.g., Hardaway, 1990; Weinberger, in press; Weinberger & Hardaway, 1990).

If Macmillan wants to look at some data specifically relevant to a classical Freudian view, McClelland and Pilon (1983) presented longitudinal data showing that adult motivation was correlated with childhood toilet training and parental permissiveness of sex and aggression (also see McClelland, Koestner, & Weinberger, 1989; Weinberger & McClelland, 1990), or the work of Fisher and Greenberg (1996), two psychoanalyst researchers who have exhaustively—and critically—reviewed the state of the empirical literature on specifically Freudian hypotheses for over thirty years. And if he wants to keep abreast of developments in psychoanalytic research more generally, Joseph Masling began editing a series that reviewed psychoanalytic research back in 1983 (recent volumes were co-edited with Robert Bornstein).

### Macmillan's Reasoning

Macmillan has chosen to ignore the data and argue about its origins, an unusual position in either science or philosophy. From a psychodynamic standpoint, we could readily write a paper castigating attribution theorists for the extraordinary assumption that attributions are conscious (and hence accessible by self-report) and for their failure to address motivated attributions; or attitude theorists for failing to address implicit processes and attitudinal ambivalence. Both of these critiques would have been well founded in the 1980s (when one of us made precisely such critiques; see Westen, 1985, 1991), but neither would be of any relevance today, except, perhaps, as historical pieces showing how far these bodies of literature have come in the last decade. Nor can we imagine that any editor would be interested in a review that said "We focus on Heider as an exemplar of attribution theory and ignore subsequent work," with a passing, disparaging reference to Dan Gilbert's work in a footnote.

But suppose we accept Macmillan's goal, whatever its value, of critiquing the nineteenth-century origins of a theory and ignoring what has followed. Macmillan's major argument is that traditional clinical data collected on the psychoanalytic couch are seriously flawed. If this were as far as he went, he would not engender much disagreement from us. He would also not be saying anything particularly novel. Each of us has made similar points (Weinberger, 2000; Westen, 1991). In fact, the case for the flawed nature of psychoanalytic clinical data was made decisively more than fifteen years ago by Adolf Grunbaum (1984), who Macmillan, oddly, does not cite in his article. Macmillan asserts that the flaws of data collected through free association are fatal to the psychoanalytic enterprise. Grunbaum came to a different conclusion, similar to what we have argued above: He suggested taking a scientific attitude toward psychoanalytic hy-

potheses and conducting independent, non-clinical investigations of psychoanalytic tenets. In his view, these tenets can and should be tested, and psychoanalysis can stand, fall, or be amended based on the outcomes of such studies, just as other theories can.

Macmillan also holds Freud, who was born in 1859, to contemporary scientific standards, pointing to flaws in Freud's thinking that are only clear from the vantage point of late-20th-century science. Consider the following: "[Freud] never granted a role to expectations and demand characteristics in his own work on hysteria. Nor did he see that such unconscious influences could be included in a deterministic framework." Demand characteristics were introduced to the psychological literature in 1962 by Orne. Experimenter expectancy effects were first discussed in any detail by Rosenthal in 1966. And expectancy is only beginning to be understood (Kirsch, 1999). All of these developments took place decades after Freud's death.

Even if Macmillan's criticism is restricted to the vaguer concept of "suggestion" available in Freud's time, it is still taken out of its historical context. The method of intensively questioning individuals and of introspection were considered perfectly respectable and even scientific in the late 19th and early 20th century (Heidbredder, 1933). They were used by Charcot, Bernheim, Liebault, and Janet, all of whom were studying the same kinds of disorders as was Freud. And when their findings were shown to be due to suggestion, as was the case with Charcot, the method was not blamed; rather, care to shield the patient from investigator hypotheses was enhanced. Even rigorously scientific investigators like Wundt and Titchener employed a disciplined form of introspection. The subjects in their experiments knew the hypotheses of their mentors, who were aware of suggestion but ignorant of demand characteristics and experimenter expectancy. The data yielded by such methods could prove to be incorrect, as was the case for Charcot and for Titchener. On the other hand, results could also prove useful, as was the case for Piaget, who also used this kind of methodology (Flavell, 1963).

It was not the use of introspection or association (free or otherwise) that doomed these schools of thought. It was the collection of further data that showed that they were wrong. Charcot was disproved by Janet (through the same kind of method; Ellenberger, 1970). Wundt and Titchener were supplanted by the functionalists and disconfirmed by the Gestalt psychologists (Heidbredder, 1933). Nothing could be more "scientific" than the behaviorism of Watson. It contained no introspection or suggestion of any sort. Nonetheless, much of it was wrong. And it was proven wrong by the data collected by later investigators, beginning with Tolman's (1932) work on latent learning and culminating in the cognitive

revolution (Baars, 1986), not by proving that the method was flawed.

Macmillan's criticism of Freud's positivism is equally inappropriate if Freud's work is understood in its historical context. It is true that Freud was an unabashed positivist and determinist. So were the vast majority of his contemporaries such as Helmholtz, Brucke, and Pavlov. Einstein was a strict determinist, declaring, in opposition to Heisenberg's uncertainty principle, that God does not play dice with the universe. No one seems to throw out the theory of relativity because of this error.

## Conclusions

Psychodynamic propositions do not rise and fall with textual analyses of Freud's arguments. They rise and fall with evidence. If Freud can be faulted for failing to follow what we now consider proper scientific procedure in testing his hypotheses (a failure, we might note, common to many of his contemporaries), this criticism should be equally applied to his contemporary critics, who have much less excuse for doing so. The time for philosophical and *ad hominem* critiques of Freud is long over.

## Note

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