

2. Norcross JC (ed): *Psychotherapy Relationships That Work*. Oxford, UK, Oxford University Press, 2002
3. Wampold BE: *The Great Psychotherapy Debate: Models, Methods and Findings*. Mahwah, NJ, Lawrence Erlbaum Associates, 2001

BERNARD D. BEITMAN, M.D.
Columbia, Mo.

Dr. Cutler and Colleagues Reply

TO THE EDITOR: We are pleased to see continuing interest in psychotherapies among readers of the *Journal*.

In response to the comments of Drs. Sareen and Skakum on the use of transference in cognitive behavior therapy, we agree that cognitive behavior therapists can productively explore the influence of early relationships on patients' core beliefs and the impact of these core beliefs on the cognitive and affective processing of interactions with the therapist. In fact, our cognitive behavior therapy formulation provides several examples in which the patient's experience of interactions with the therapist and the association of this experience to his longstanding core beliefs might enter into the work of treatment. Therapists of different orientations may use similar concepts and even similar techniques in service of very different interventions. Yet despite the frequent usefulness of extending cognitive work to the therapist-patient relationship, we believe the writers' claim that "transference issues...must be an integral component of the complete management of every patient undergoing cognitive behavior therapy" overstates the case. For example, the focus of treatment for a patient participating in short-term cognitive behavior therapy for panic disorder would likely be on the patient's interpretation of somatic states, and the patient's relationship with the therapist might never enter into the foreground of treatment. All therapists should heed the transference, but not all may interpret it. Nonetheless, Drs. Sareen and Skakum's comments usefully underscore the importance of the therapeutic relationship across treatment approaches and the usefulness of comparing how different psychotherapies manage it.

We disagree with Dr. Beitman's contention that "the schools of therapy are illusory." Common factors are important, patient and therapist factors count, but meaningful differences exist among psychotherapeutic approaches, as any good therapist knows. Although these differences may not always matter, they often may. We agree that all psychotherapeutic treatments involve the power of the interaction, implicitly the transference to authority, unconscious communications, and the wish for relief. Technique in practice is always flexible; for example, every effective psychodynamic treatment involves varied noninterpretive interventions that deepen the process and reveal resistances. Constructs like common factors and patient variables require greater precision and more research. The three psychotherapies our case conference described would not necessarily have yielded identical results. Process research can disentangle the useful from the extraneous in particular therapies, but only within the context of outcome research of defined psychotherapies; i.e., which processes may mediate treatment outcomes. Process research does not currently support the conclusion that no significant differences exist among various psychotherapies. Nor should Dr. Beitman's letter validate muddy eclecticism. Research may

helpfully explore aspects of psychotherapy from neurobiological, process, and outcome vantage points, but it is surely premature to say that we should abandon 55% of our expectations to every patient's characteristics or that we can yet diagram the neurobiology of countertransference. We join Dr. Beitman in looking forward to the day when such a diagram may be possible.

JANIS L. CUTLER, M.D.
ADAM GOLDYNE, M.D.
JOHN C. MARKOWITZ, M.D.
MICHAEL J. DEVLIN, M.D.
ROBERT A. GLICK, M.D.
New York, N.Y.

A Simplistic Understanding of the Five-Factor Model

TO THE EDITOR: Jonathan Shedler, Ph.D., and Drew Westen, Ph.D. (1), argued that the five-factor model "omits key clinical constructs and may not capture the complexity of personality syndromes seen in clinical practice" (p. 1743). To demonstrate this, they constructed an abbreviated measure of the five-factor model using a small subset (30%) of the 200 items contained within the Shedler-Weston Assessment Procedure (SWAP-200). It was a foregone conclusion that the results of a factor analysis of 30% of the SWAP-200 items would not correspond to a factor analysis of the entire SWAP-200. In addition, one should ask whether their SWAP-200 items provided an adequate assessment of the five-factor model. They never attempted to validate their five-factor model measure, and a visual inspection of the items indicates inaccurate representation of the five-factor model. Finally, it is highly unlikely that their small set of SWAP-200 items would provide anything close to a reasonably comprehensive assessment of the five-factor model. In sum, there was little reason to expect that an incomplete and inadequate assessment of the five-factor model with a subset of the SWAP-200 would account for the variance within the entire SWAP-200.

Drs. Shedler and Westen suggested that the constructs assessed by the 12 SWAP-200 scales are outside of the realm of the five-factor model. They ignored many, many studies that indicated otherwise. For example, two other compelling dimensional models of personality disorder have been developed by Dr. Livesley (the Dimensional Assessment of Personality Pathology) and Dr. Clark (the Schedule for Nonadaptive and Adaptive Personality). A complete description of and references for these measures, as well as others, is provided by Widiger and Simonsen (2). The clinical constructs assessed by these scales include narcissism, identity problems, eccentric perceptions, affective lability, aggression, detachment, self-harm behaviors, and compulsivity that resemble closely the scales of the SWAP-200, and many studies have documented well that the constructs assessed by the Dimensional Assessment of Personality Pathology and the Schedule for Nonadaptive and Adaptive Personality are well within the realm of the five-factor model.

Drs. Shedler and Westen (1) derogatorily characterized the five-factor model as providing a simplistic lay description of personality. What was simplistic was their characterization of the five-factor model. The five-factor model is a rich dimensional model of general personality structure that has been used successfully in many areas of science and practice, in-

cluding (but not limited to) the study of temperament, gender differences, heritability, behavioral medicine, and aging. An integration of the DSM-IV personality disorder nomenclature with the five-factor model would go far in integrating DSM-IV with basic science research on personality structure (3). We regret that Drs. Shedler and Westen (1) argued instead for a distinct separation of our clinical understanding of personality disorders and basic science research on personality structure.

References

1. Shedler J, Westen D: Dimensions of personality pathology: an alternative to the five-factor model. *Am J Psychiatry* 2004; 161: 1743–1754
2. Widiger TA, Simonsen E: Alternative dimensional models of personality disorder: finding a common ground. *J Pers Disord* 2005; 19:110–130
3. First MB, Bell CB, Cuthbert B, Krystal JH, Malison R, Offord DR, Reiss D, Shea MT, Widiger TA, Wisner KL: Personality disorders and relational disorders: a research agenda for addressing crucial gaps in DSM, in *A Research Agenda for DSM-V*. Edited by Kupfer DJ, First MB, Regier DA. Washington, DC, American Psychiatric Association, 2002, pp 123–199

THOMAS A. WIDIGER, PH.D.
Lexington, Ky.
TIMOTHY J. TRULL, PH.D.
Columbia, Mo.

Drs. Shedler and Westen Reply

TO THE EDITOR: The five-factor model is based on the lexical hypothesis that anything meaningful about personality can be identified by studying the language people naturally use to describe one another. The question is, what language should we study?

If we want to apply the lexical hypothesis to clinical phenomena, we would do well to apply it to the concepts of expert clinicians, not just ratings by laypeople. Practitioners of other medical subdisciplines would not agree to restrict their diagnostic concepts to the everyday language used by their patients (e.g., headache, feeling queasy) and for good reason: Experts develop knowledge and understanding that laypeople do not share. One would not ask physicians to limit themselves to the diagnostic vocabulary of their patients unless one believed that they understood nothing more than laypeople about physiological processes. The same applies to clinical psychologists and psychiatrists and their understanding of mental processes.

Our use of an item set designed for experts allows us to assess constructs that are difficult to capture with self-report measures, however well constructed. For example, the SWAP-II addresses the clinically crucial concept of splitting (dichotomous thinking) in borderline patients with items such as, “When upset, has trouble perceiving both positive and negative qualities in the same person at the same time (e.g., may see others in black or white terms, shift suddenly from seeing someone as caring to seeing him/her as malevolent and intentionally hurtful, etc.)” It assesses subtle forms of thought disturbance that laypeople often overlook (e.g., “Tends to think in concrete terms and interpret things in overly literal ways; has limited ability to appreciate metaphor, analogy, or nuance”) and “Thought processes or speech tend to be circumstantial, vague, rambling, disgressive, etc. (e.g., it may be unclear whether he or she is being metaphorical or whether his or her

thinking is confused or peculiar”). It assesses defenses and coping strategies that are absent from the five-factor model entirely (e.g., “Tends to see own unacceptable feelings or impulses in other people instead of in himself/herself”).

Although the five-factor model is empirically elegant, its advocates have not convincingly addressed the question of clinical utility. The five-factor model has engendered little enthusiasm among clinicians, precisely, we suspect, for the reasons outlined here. Spitzer and colleagues (personal communication, December 2004) recently conducted a “nonpartisan” comparison of alternative proposals for axis II for DSM-V. They found that experienced psychiatrists and psychologists consistently rated the five-factor model *less* clinically useful than other diagnostic systems richer in clinical depth (including our system derived from the SWAP-200).

We do not, as Drs. Widiger and Trull assert, advocate “a distinct separation of our clinical understanding of personality disorders and basic science research.” On the contrary, we agree that such integration is essential. However, we do not believe the way to achieve this integration is by asking experts to talk and think like laypeople. If DSM-V is to be relevant to scientists and practitioners both, it will need to pay more attention than previous editions of the manual to clinical relevance and utility (1). Substituting the language of everyday conversation for the language of clinical discourse seems unlikely to achieve this goal.

Reference

1. First MB, Pincus HA, Levine JB, Williams JBW, Ustun B, Peele R: Clinical utility as a criterion for revising psychiatric diagnoses. *Am J Psychiatry* 2004; 161:946–954

JONATHAN SHEDLER, PH.D.
Denver, Colo.
DREW WESTEN, PH.D.
Atlanta, Ga.

The “Infallibility” of Psychopathology

TO THE EDITOR: The editorial by George S. Alexopoulos, M.D. (1), rightly pointed out the limitations of a strictly scientific approach to the understanding of mental illness. His reliance on the philosophy of science to illuminate the social context in which scientific theories of psychopathology rise and fall is admirable and, in its own restricted way, helpful. However, he failed to push his exploration as far as it can go.

As the editorial correctly asserted, Karl Popper’s view of science rigorously separates the experimental phase of the scientific process from social influences on theory formation. However, Dr. Alexopoulos did not mention the views of Willard V.O. Quine, Pierre Duhem, and Donald Davidson (2), who denied the adequacy of atomized scientific theorizing to deal with the question of empirical falsification. Quine, Duhem, and Davidson instead argued that theories of science exist not in isolation but, rather, are linked to each other through a web of belief. The rich connectivity of this web ensures that any new experimental result, which Popper might deem a refutation of one specific theory, can also be seen as explained by the same theory if some other theory within the overall web of scientific belief is commensurately adjusted. Context is crucial here, although the conventionalism of Quine, Duhem, and Davidson does not identify social elements as fundamental contextual factors (2).