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.

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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 on 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.

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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).

In contrast, Martin Heidegger (3) viewed intersubjectively tacit assumptions embedding scientific theorizing as a key that can unlock the cognitive straightjacket of pure science. Heidegger placed these implicit cultural biases at the remote limits of science's theoretical web, beyond its horizon of explicit cognizance under ordinary circumstances. However, recognition may erupt as a pragmatic necessity when sufficiently large breakdowns in the use of the web arise (3).

Popper would call such crises critical experimental falsifications. Quine, Duhem, and Davidson might term them massive empirical perturbations in the network. Heidegger framed them as golden opportunities to cast our gaze beyond theories as mere workaday tools. However, he also comprehended the price that such enhanced vistas exact.

In Heideggerian terms, violent assaults by the world of empirical practice on our culturally conditioned notions of abstract theorizing as a comfortable mode of human existence will decenter the scientific subject at his or her core. These jolts must generate ontic anxiety that cannot be anticipated in advance by the authoritative pronouncements of agenda-setting peer conferences or assuaged after the fact by the nostrum of consensual fiat.

Hence, one might justifiably ask, beyond mere science, can even meta-science, whose notion of progress is itself shot through with existential angst, contain the problem of anxiety?

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Dr. Alexopoulos Replies

To the Editor: Dr. Mender's letter offers additional support for my view that experimentally sound scientific findings have only relative value and underscores the importance of nonscientific processes. He points out that thinkers from two disparate traditions raise questions about the safety of experimentally supported conclusions.

Dr. Mender's first point is that the experimental method cannot adequately confirm or reject scientific hypotheses. This view has been articulated in the "Duhem-Quine thesis" and is based on the assumption that hypotheses cannot be tested in isolation from the theoretical network in which they belong (1, 2). Scientists do not subject an isolated hypothesis to testing but only a whole group of hypotheses. Thus, testing a hypothesis depends on its background assumptions. When the background assumptions are challenged, the observations that initially were used to justify a hypothesis become irrelevant. However, when a predicted event fails to occur, it is evident that something in the hypothesis needs to be changed, but nothing in the experiment indicates what the change should be. Consequently, any hypothesis can be safeguarded from falsity, so long as scientists are prepared to make appropriate adjustments to other parts of the theoretical framework. Therefore, critical experiments alone may be inadequate to change the zeitgeist of science.

Dr. Mender's second point emphasizes the power of culture in the scientific process. It is based on the existentialist view that embeddedness in a cultural context contributes to an inveterate tendency toward conventionalism and inauthenticity (3). Scientists are not immune to this problem. As they become initiated in the practices of the scientific community, they are inclined to drift along with the crowd, enacting stereotyped roles. The current competitive funding and publication system encourages conformity and compromises the scientists' ability to seize on and define their own scientific lives. Thus, to be authentic is not an expected consequence of the scientific process and requires action.

Paraphrasing Socrates's "εν οιδα οτι ουδεν οιδα," one can argue that there is no safety in science or anything else. Nonetheless, despite its limitations, science has offered the soundest approach to understanding and treating psychopathology. Most of the recent treatment advances in psychiatry have been based on science. Thus, my argument is not one of scientific nihilism but one that emphasizes the need for awareness of the nonscientific forces influencing the scientific process, including the scientific culture and its extremes, the evolving means of experimentation, and the social factors that promote scientific productivity. Because scientific criteria alone cannot define scientific priorities, this responsibility falls on investigators and policy makers.

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