Revision of the Personality Disorder Model for DSM-5

TO THE EDITOR: The DSM-5 Personality and Personality Disorder Work Group welcomes input from research and clinical communities, as represented by the commentary by Jonathan Shedler, Ph.D. et al. (1), published in the September 2010 issue of the *Journal*. Addressing many of the authors' concerns, the work group has significantly changed its originally proposed new model of personality and personality disorder assessment and diagnosis, in response to comments posted on the American Psychiatric Association DSM-5 website.

The model is now considerably simpler. It consists of the following three dimensional components: a rating of levels of personality functioning, ratings of personality types, and independent ratings of personality trait domains and facets (i.e., disassociated from the types pending further research, as suggested by Shedler et al.). These three components are combined to yield criterion A (mild or greater impairment in personality functioning) and criterion B (*either* a good or very good match to a type *or* an extreme rating on one of the six trait domains) of the revised general criteria for a personality disorder. The model is flexible and focuses attention on personality psychopathology with increasing degrees of specificity, depending on a clinician's available time, information, and expertise. Thus, the level rating helps a clinician to determine whether or not a patient has a personality-related problem and, if so, how severe it is. The type rating allows for the characterization of personality problems according to broad descriptions. The trait ratings enable further description of the heterogeneity of any type by a patient-specific trait profile, if desired, and also describe patients who do not have a good match to any of the proposed types ("personality disorder trait-specified," formerly known as personality disorder not otherwise specified).

The rationales for the proposed changes are documented in literature reviews and have been summarized in forthcoming articles (e.g., 2). Serious problems with the current personality disorder diagnostic system are thoroughly documented in the literature. Thus, more than incremental change appears to be justified, just as serious problems with reliability justified precedent-setting changes from DSM-II to DSM-III. Although good scientific rationales exist for proposing the inclusion of five particular types related to current DSM-IV-TR personality disorder criteria, prototypes are now being considered for all 10 personality disorders, to ease the transition to DSM-5. The proposed trait structure model was derived carefully from existing trait models (e.g., Five Factor Model; Schedule for Nonadaptive and Adaptive Personality; Dimensional Assessment of Personality Pathology, etc.) to broadly cover pathological personality traits. The validity of the trait model is being assessed in a multiwave community survey.

The simplified and streamlined proposal will be tested in field trials for reliability and clinical utility. We anticipate further changes based on these results. We also look forward to continuing our dialogue with experts in the research and treatment of personality disorders, as well as with the clinical community, as DSM-5 progresses.

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ANDREW E. SKODOL, M.D.
Tucson, Ariz.

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Response to Skodol Letter

TO THE EDITOR: We are pleased that the DSM-5 Personality and Personality Disorder Work Group is considering feedback from the larger mental health community and invites continuing dialogue. We look forward to seeing the revised personality disorder proposal to which Dr. Skodol refers.

We reiterate our conviction that while dimensional trait models are useful as *research tools*, they are of limited relevance and utility in clinical practice. They should not be offered *in lieu of* clinically coherent personality prototypes, nor offered in a way that competes with or complicates the use of a prototype-based diagnostic system.

A good diagnostic system is like a good map in that it must accurately depict the territory. However, sometimes one requires a road map, sometimes a topographical map, and sometimes a political map. A mountaineer in a wilderness region will have little use for a highway map, regardless of its accuracy.

We do not consider the trait model map inherently better or worse than the personality prototype map. Rather, we believe it is the wrong map for clinical purposes. The two kinds of maps address completely different questions. Academic personality researchers ask questions about the *relationships among variables in a general population*. Dimensional trait models help answer such questions. Clinical practitioners need to understand the *interrelation of psychological processes in an individual patient*. Personality prototype models facilitate such understanding. Neither approach is more scientific than the other; it is a matter of what questions one is trying to answer. The primary purpose of DSM is the clinical diagnosis of patients. We therefore believe that the DSM-5 diagnostic system for personality disorders should be based on personality prototypes, not trait dimensions.

We strongly agree with Dr. Skodol's wish to select DSM-5 prototypes on "good scientific rationales." The prototypes used in DSM-5 should be those that have emerged empirically in research specifically conducted to develop personality prototypes, that have demonstrated their clinical value–not borrowed from DSM-IV to "ease the transition" to DSM-5 or to an eventual predetermined dimensional trait model for DSM-6.

The empirical literature on personality trait models developed independently of the clinical practice literature and

with little input from clinical practitioners. It reflects mainly the methods and concepts valued by academic researchers who do not interact with patients. It would be unfortunate if the official psychiatric diagnostic manual mirrored this bias, to the detriment of patient care.

JONATHAN SHEDLER, PH.D.
Denver, Colo.
AARON T. BECK, M.D.
Philadelphia, Pa.
PETER FONAGY, PH.D.
London, United Kingdom
GLEN O. GABBARD, M.D.
Houston, Tex.
OTTO KERNBERG, M.D.
White Plains, N.Y.
ROBERT MICHELS, M.D.
New York, N.Y.
DREW WESTEN, PH.D.
Atlanta, Ga.

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Disulfiram: An Anticraving Substance?

TO THE EDITOR: We read with interest the Treatment in Psychiatry article by Bankole A. Johnson, D.Sc., M.D. (1), published in the June 2010 issue of the *Journal*. Hereby, we would like to briefly comment on the author's statements regarding disulfiram.

Disulfiram has been used for more than 50 years as an aversion therapeutic agent in the treatment of alcohol dependence (2). However, categorizing disulfiram as a "psychological pill" does not encompass the whole potential of the substance, since emerging evidence suggests that it also possesses anticraving properties (3). Besides its well-known mechanism of action (i.e., aldehyde dehydrogenase inhibition), disulfiram also inhibits dopamine beta-hydroxylase, leading to an increase of dopamine concentrations while decreasing concentrations of norepinephrine in the brain (4). Since dopaminergic transmission in the ventral striatal reward system is suggested to play a key role in the development of addictive disorders and craving (5, 6) and reduced activation of this system has been shown in alcohol dependence (7), cocaine dependence (8), and nonsubstance-related addictions such as pathological gambling (9), disulfiram could be hypothesized as a common treatment option for these disorders. In cocaine dependence, disulfiram has already shown preliminary efficacy in reducing craving and relapse rates (10). Furthermore, in a patient with alcohol dependence and comorbid pathological gambling, treatment with disulfiram led to a significant reduction in alcohol craving and urges to gamble as well as to maintenance of abstinence from both alcohol and gambling for more than 12 months (11). In alcohol dependence, most of the clinical trials conducted with disulfiram have possessed significant methodological shortcomings and have not measured changes in alcohol craving under treatment (2). Therefore, to evaluate the potential of disulfiram as an anticraving agent also in alcohol dependence, further randomized controlled trials would be needed.

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CHRISTIAN A. MÜLLER, M.D.
ROMAN BANAS
Berlin, Germany

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Response to Müller and Banas Letter

TO THE EDITOR: I thank Drs. Müller and Banas for their interesting comments. Recently, I compiled an updated synopsis on the actions, effects, and efficacy of disulfiram in the treatment of alcohol dependence (1). Disulfiram can be an effective agent for treating alcohol dependence, but this is limited to populations where there is high compliance with adhering to the medication (2) or where subjects have been directly supervised (2, 3). Since the behavioral effects of monitoring alcohol consumption and direct supervision are quite powerful in helping to maintain abstinence, it is these elements that are associated with any potential efficacy for disulfiram as a treatment for alcohol dependence. As such, its effect as a "psychological pill" is the predominant mode of action.

Disulfiram is associated with an increase in acetaldehyde levels, which in and of itself appears to be reinforcing (4). While I concur with Drs. Müller and Banas that disulfiram has been shown to inhibit dopamine beta-hydroxylase