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Dr. Shea Replies

TO THE EDITOR: Dr. Reich raises some of the key questions that face the field of personality disorders, all of which may certainly benefit from more extensive discussion. Here we add some further thoughts on these issues in the spirit of extending such discussion.

It is true that semistructured interviews for DSM personality disorders do not agree well with each other, and it would be difficult to defend such interviews as gold standards for the diagnosis of personality disorders. However, this may be less the fault of the interviews themselves than the definitions of personality disorders that they set out to assess. The observation has frequently been made that the criteria sets include a mixture of behaviors, feelings, attitudes, and traits, with wide variability in complexity and level of inference (1). The limitations of assessment are likely to continue until the conceptualization and definition of personality disorders improve. As we commented in our article, clarifying the constructs that are presumed to underlie the observed manifestations (e.g., traits, dimensions, and genotypes) would allow more precise and, most likely, more reliable and valid assessments.

The influence of “state” on personality assessment is an important consideration. Here it is worth distinguishing between two meanings that have been ascribed to the axis I confound issue. One refers to a negative distortion in the individual’s perception and report of his or her usual self. This inflated report of lifetime psychopathology would result in a “false positive” personality disorder diagnosis. The influence of depressed mood on reports of personality disorders has been demonstrated on self-report measures, although the impact when semistructured interviews are used is less clear (2). A separate issue concerns mistaking a chronic axis I disorder as a personality disorder; hence, what appears to be a personality disorder “remission” is really remission of an axis I disorder. It is important here to not let our biases dictate our conclusions (if a “remission” occurs, must it be axis I?). Another explanation is that the domain of psychopathology that our current system divides into axis I and axis II disorders may include dimensions that underlie both (3).

If personality disorders were defined in terms of maladaptive traits, perhaps fluctuation over time in the manifestations of those traits would not be a surprise. That is, traits represent propensities to respond, feel, think, or behave in certain habitual ways. Individuals exhibiting a given trait may have differing amounts of such “propensities” or varying thresholds for manifesting the kinds of behaviors that define the trait. Those with low thresholds will appear more stably “personality disordered,” while those with relatively higher thresholds may show fewer or more intermittent manifestations, appearing less “stable.” Thus, perhaps rather than two disorders

(lasting and “stress-induced/state”), as Dr. Reich proposes, the difference may be more of degree. The latter is consistent with our findings and others’ (4) of high correlations of a number of criteria for each personality disorder across assessments points, despite a significant decrease in the mean level of criteria present.

Finally, we agree that it is likely that some of our findings are due to the influence of treatment (psychosocial and pharmacological). This influence is very difficult to estimate in a naturalistic study like the Collaborative Longitudinal Personality Disorders Study since the more severely ill patients tend to receive more treatment, resulting in a negative association between receiving treatment and doing well. Nonetheless, information regarding the range of outcomes for individuals with personality disorders in clinical study groups, receiving the types of treatments currently available, addresses a clinically relevant question that is important to clinicians and policy makers.

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Body Dysmorphic Disorder and Art Background

TO THE EDITOR: In their article, David Veale, M.D., and colleagues (1) reported the interesting finding that 20% of 100 patients with body dysmorphic disorder had a lifetime job (7%) or education (13%) in art or design. This was the case for only 4% of the patients with a major depressive episode, 3% of those with obsessive-compulsive disorder, and 0% of those with posttraumatic stress disorder. Dr. Veale et al. hypothesized that this finding may reflect an appreciation of esthetics in individuals with body dysmorphic disorder, which may contribute to the disorder’s development in some patients. An equally plausible explanation is that patients with body dysmorphic disorder tend to develop an interest in esthetics.

Because this intriguing issue has not otherwise been studied, to our knowledge, we determined the rate of employment as an artist for 146 consecutive participants (69% women, mean age=32.2 years, SD=11.4) in an ongoing prospective naturalistic study of the course of body dysmorphic disorder. Data on current occupation (during the past 6 months) was obtained by an investigator blind to any hypothesis about an association between occupation and a diagnosis of body dysmorphic disorder. The subjects were categorized as artists by using the definitions of Dr. Veale et al. (1). We also estimated the proportion of individuals in the United States currently employed as artists, based on the 2000 Occupational Employ-

ment Statistics survey (2). This survey collects nationwide occupational data (excluding self-employment) based on the government-wide Standard Occupational Classification system. Because this system does not report adequately detailed statistics for several of the artist categories used by Dr. Veale and colleagues, we made several assumptions to derive our estimate (for example, the Occupational Employment Statistics survey reports the number of postsecondary art, music, or drama teachers, which we divided by 3 to derive an estimate for art teachers). Because of insufficient detail, several teacher categories were excluded from our analyses, so our estimated rate of artists in the general population is likely an underestimate.

Of the 146 subjects with body dysmorphic disorder, 1.4% (N=2) (95% confidence interval [CI], 0%–3.3%) were currently employed as artists. The estimated rate in the general population was 0.64% (753,283 of 117,006,770) (95% CI=0%–1.4%). Thus, the proportion of subjects with body dysmorphic disorder who were employed as artists was approximately twice that in the general population.

Our rate of 1.4% cannot be directly compared to the rate of Dr. Veale et al. of 20% because their rate includes education in art/design, which we did not assess. Nor is our rate directly comparable to the employment rate of 7% of Dr. Veale et al. because we assessed only current—not lifetime—occupation. Nonetheless, our rate is notably lower than that of Dr. Veale et al.; however, it is still higher than that in the general population and thus offers modest support for the hypothesis that body dysmorphic disorder is associated with employment as an artist. Our findings are based on a small group, however, and should be considered preliminary. Further research is needed to elucidate factors that may contribute to body dysmorphic disorder's development, as this important topic has received little investigation.

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Transcranial Magnetic Stimulation and Processing of Facial Threats

TO THE EDITOR: With great interest, we read the article by Ralph E. Hoffman, M.D., and Idil Cavus, M.D., Ph.D. (1), who discussed the results of a recent study of slow repetitive transcranial magnetic stimulation (rTMS) on the attentional processing of threatening facial expressions (2). They considered those results consistent with the findings of McCann et al. (3), who reported reductions in the symptoms of posttraumatic stress disorder (PTSD) after slow rTMS over the right prefrontal cortex. Since enhanced reactivity to a perceived threat is a central symptom of PTSD, the demonstration of reduced attention to threatening faces after slow rTMS over the right prefrontal cortex (2) would, according to Drs. Hoffman and Cavus, not only fit the valence model of approach- and with-

drawal-related emotion but also provide evidence concurring with the study by McCann et al. (3).

Although we, in fact, showed enhanced attention to threatening (angry) facial expressions after right prefrontal cortex rTMS (2), our finding is in accordance with the valence model. Threat can be displayed by fearful and angry facial expressions, but whereas fear is a withdrawal-related emotion associated with relatively more right-sided activity of the prefrontal cortex, anger is an approach-related emotion associated with relatively more left prefrontal cortex activity (4). The local inhibitory effects of slow rTMS of the right prefrontal cortex induce relatively more left prefrontal cortex activity and thus enhance attention to angry faces (2). Slow rTMS of the right prefrontal cortex should contrariwise reduce attention to fearful faces since there is clearly relatively less right prefrontal activity. In agreement, we recently demonstrated reduced attention to fearful faces in a placebo-controlled study of slow rTMS over the right prefrontal cortex (4). These findings concur with the reductions in fear-related PTSD symptoms by McCann et al. (3). Moreover, rTMS-EEG research indicates that the reductions in attention to fearful faces (4) are anxiolytically mediated and that the local inhibitory effects of right prefrontal cortex rTMS result in excitation of the left prefrontal cortex (5). Such left prefrontal cortex excitation after slow rTMS of the right prefrontal cortex is not counterintuitive but is defensibly due to reductions in transcallosal inhibition, a crucial mechanism in the valence model of emotion.

In sum, when discussing the motivational and neurobiological mechanisms by which slow rTMS over the prefrontal cortex reaches its effects on approach- and withdrawal-related emotion, the phenomenon of contralateral inhibition between the hemispheres and the exact nature of the emotional index of behavior should be taken into account.

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Drs. Hoffman and Cavus Reply

TO THE EDITOR: We regret that when discussing the TMS effects reported by d'Alfonso et al. (2000) as they might pertain to an rTMS case series of PTSD patients, we misrepresented the ef-