Mentalism, Mechanisms, and Medical Analogues: Reply to Wakefield (1998)

Arthur C. Houts
University of Memphis

William C. Follette
University of Nevada, Reno

J. C. Wakefield’s (1998) critique of W. C. Follette and A. C. Houts’s (1996) article is addressed by raising questions about (a) mentalism as a framework for studying psychopathology, (b) the nature of inferred mechanisms and the process of making such inferences, and (c) the accuracy of claiming that the Diagnostic and Statistical Manual of Mental Disorders (DSM; American Psychiatric Association, 1980, 1987, 1994) are about “the ways things can go wrong with the mind” (p. 846) and (b) the ways the mind works are too complex to be subsumed under some general theory. From (a) and (b), he concludes that it is desirable for the DSM listing of mental disorders to be noncommittal regarding theories about how the mind works. Presumably, many theories are needed depending on which mechanisms of the mind Wakefield and others infer to have gone wrong. We see many problems with Wakefield’s formulation of the DSM and with his more general approach, which he has labeled the harmful dysfunction analysis of mental disorders. In our response, we address three problems: mentalism, mechanisms, and medical analogues.

Mentalism

As behaviorists, we have been bemused by Wakefield’s generous use of mentalistic terms such as mind and mental dysfunction. As quoted above and in his other writings, Wakefield (1992a, 1992b, 1993, 1996, 1997a, 1997b, 1998) apparently believes that the behaviors that warrant a diagnosis are indicative of mental dysfunction. The behavior did not go wrong, the mind did. For Wakefield, behavior is just a manifestation (symptom) of mind. Such a formulation in mentalistic terms leads Wakefield to promote either a biological model of psychopathology or a pedestrian folk psychology.

Wakefield (1998) has a theory of mental disorder, and his theory is that for every genuine mental disorder there is an internal malfunction inside the organism. What sorts of things are inside the organism that can malfunction? Wakefield’s favorite examples illustrate the point. By analogy to the heart as pump and the automobile as conveyance, Wakefield (1998) implies that the mental breakdown must be due to some internal physical breakdown. Without a physiological anchor, mental mechanisms can be invented as needed.

On occasion, Wakefield (1998) has indeed offered purely mental mechanisms rather than physiological ones as what must have gone wrong or broken down. Such purely mental mechanisms are created de novo as needed. For example, he converts our example of depressive behaviors to a condition where “functioning of loss-response mechanisms are compromised” (Wakefield, 1998, p. 849). Elsewhere, he has inveighed that major depressive disorder can be distinguished from depressive behaviors because in the disorder there has been a dysfunction in “loss-reaction-regulating mechanisms, or sadness-generating mechanisms” (Wakefield, 1997a, p. 645). Where is the loss-response mechanism? Is it closer to the gain-response mechanism or nearer to the damage-response mechanism? And where are they? Is the sadness-generating mechanism located in the stomach or the throat? According to Wakefield’s formulation where something must have gone wrong inside the organism, such hypothetical mechanisms must be invoked or else his formulation cannot distinguish genuine disorder from normal behavioral variation. Where biological psychiatry cannot yet supply the physical mechanism, Wakefield seems content to freely invoke mental mechanisms.

Mechanisms

In his response to our article (Follette & Houts, 1996) and elsewhere, Wakefield (1998) claims that his definition of mental disorder as harmful dysfunction and the DSM definition of mental disorder provide an objective, scientific basis for discriminating true mental disorders from otherwise normal variations of human behavior. According to his definition, behaviors are symptoms of a mental disorder if, and only if, those behaviors are caused by a mechanism inside the organism and that mecha-
nism has malfunctioned. To establish that behaviors are a symptom of mental disorder, two requirements, one empirical and one normative, must be met: (a) The mechanism that produced the behaviors must be identified and shown to have caused the behaviors in question and (b) the mechanism must be shown to have malfunctioned or to have gone wrong. As Wakefield (1996, 1997a) has taken pains to point out on numerous occasions, absent these two requirements being met, the behaviors are not symptoms of a mental disorder. If these two requirements are not met, the behaviors, even if they appear in a DSM diagnostic criteria list, must be regarded as problems in living or some other designation that is distinct from mental disorder.

How does Wakefield (1998) suggest these conditions for mental disorder be established? Note that Requirement (a) is empirical and contains two parts: Identify a mechanism and demonstrate that the identified mechanism is the cause of behavior. As previously noted, Wakefield’s approach to the first part of Requirement (a) is to multiply mechanisms freely, to let a thousand mechanisms be inferred. He does not offer, nor does he appear to follow, any empirical constraints for deciding when there is evidence for the operation of some inferred mechanism.

Note that the second part of Requirement (a) requires that, to establish a mechanism as the causal explanation for some behavior, the causal role of the mechanism must be established independently from the behavior that the mechanism was invoked to explain in the first place. To do otherwise is to engage in circular, tautological reasoning. What Wakefield would have to show is that manipulation of some putative mechanism was both the necessary and sufficient cause of some behavior. Has Wakefield, or anyone else, provided independent confirmation for the causal role of some mechanism in the production of behavior that is relevant to some diagnostic category? We do not think so, but we are willing to be convinced. Investigators have been pursuing some promising hypotheses, such as chronic overreaction of the hypothalamic pituitary adrenal axis among those labeled with depressed, breathing dysregulation among those labeled with panic disorder, and dysregulation in the caudate nucleus among those labeled with obsessive–compulsive disorder. These are the types of things required for Wakefield’s hopes for the DSM and his own variation of the DSM to come true. They are not accomplished facts. Thus, by Wakefield’s own logic, much of what is now in the DSMs should be removed until such time as the required mechanisms are demonstrated to have been found.

To establish Requirement (b) — that the presumed mechanism has malfunctioned — some normative claim about functioning must be posited. As we noted in our original article (Follette & Houts, 1996), Wakefield has tried to circumvent the value issue by invoking evolutionary arguments for what constitutes proper functioning of some mechanism. Lillenfeld and Marino (1995) have provided an extensive critique of the adaptationist logic that underlies Wakefield’s appeals to selection and design as the basis for inferring when something has gone wrong. Wakefield’s attempts to extract normative conclusions from appeals to what nature must have intended fall on the same false ground as the numerous other attempts (e.g., eugenics and sociobiology) to smuggle in social conventions and values in a scientific Trojan horse. No amount of waving the banner of evolution and design by natural selection will cover up the fact that what makes behavior abnormal is a social judgment first and foremost. We may, and frequently do, subsequently seek to justify those judgments in terms of appeals to what nature intended. This strategy has worked for much of physical health because we are convinced by arguments, for example, that nature intended the heart to pump blood. We simply do not have, nor has Wakefield supplied, convincing arguments regarding what, for example, nature intended the caudate nucleus to do. The faith that we will ever have such convincing arguments regarding how the mind works is strongly doubted by leading evolutionary biologists (e.g., Gould, 1997; Mayr, 1997).

### Medical Analogues

Wakefield (1998) makes a rhetorical appeal to the success of physical medicine by claiming that what has transpired in the expansion of the modern DSMs is analogous to the progress of medicine in general. This is a familiar argument proffered by those who defend expansions of the DSM as consonant with scientific progress. The argument is as follows. (a) Who can argue that we have made scientific progress in medicine? (b) The DSM revisions are doing the same thing that revisions of medical diagnoses have done. (c) Therefore, we should be happy about the DSM revisions and praise those, too, as scientific progress. We do not believe that Wakefield has made a convincing case for (b).

Relying on numbers provided by Spitzer, Wakefield (1998) addresses our criticisms of the expansion of the DSMs by noting that codeable disorders in the International Classification of Diseases (ICD) sections on the circulatory and digestive systems have expanded comparably from the 8th edition of the ICD (ICD–8; World Health Organization [WHO], 1968) to the 10th edition of the ICD (ICD–10; WHO, 1992). Even a cursory firsthand inspection of the two ICDs shows that much of their expansion is accounted for by the fact that diseases listed under a single code in the ICD–8 merely received a separate code in ICD–10. For example (and there are many others), the ICD–8 (p. 176) contained only one code for chronic ischemic heart disease and listed multiple forms, whereas the ICD–10 (p. 482) provided nine codes, only one of which was not previously named in the ICD–8. Unlike many of the expansions of the DSMs, the ICD changes were not due to completely new disorders being added to subsequent editions of the nomenclature. It is highly misleading for Wakefield and Spitzer even to suggest comparability between the DSM expansions and the ICD changes.

In his response to our critique of the DSM as a research program that is not progressing, Wakefield (1998) claims that the DSM “is not a research program but a listing of the known ways that things can go wrong with psychological functioning” (p. 848). Part of his argument was that we were incorrect in treating the DSM as a program of research and therefore incorrect to apply standards of certain philosophical views to judge its relative progress. Yet, elsewhere Wakefield (1997a) has writ-

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Footnote: For example, “from perturbations in Behavior B, I infer that Mechanism A has caused B to change. How do I know that A caused B to change? Because B changed!” Such a sequence of explanation is clearly inadequate.
ten that "in sum DSM is much more than a list of disorders. It is a program to place the mental health professions on a firm conceptual footing after decades of virulent and often embarrassing criticism" (p. 637). We contend that it is useful and informative to look at the DSM changes as part of an overall program for organizing knowledge in the mental health field, and unlike Wakefield (1997a), we are not impressed with how well things have been going.

Wakefield (1998) defends the expansion of the DSMs by stating that "there is nothing ad hoc about adding categories to diagnostic manuals when we identify new ways that things can go wrong or when formerly ignored distinctions between breakdowns become important" (p. 848). Regarding the first claim, in what new ways did exactly what go wrong for the following DSM labels: frotteurism, identity problem, intermittent explosive disorder, kleptomania, mathematics disorder, nightmare disorder, pathological gambling, sexual disorder not otherwise specified, voyeurism, and unspecified mental disorder (nonpsychotic)? Without evidence for some failed mechanism having been newly discovered, the introduction of these disorders certainly appears to be ad hoc.

Regarding the second claim, what are the criteria for deciding when distinctions become important? The revised third edition of the DSM (American Psychiatric Association, 1987) introduced sleep–wake schedule disorder, frequently and changing type, and the fourth edition of the DSM (American Psychiatric Association, 1994) partitioned this into circadian rhythm sleep disorder, either jet lag type or shift work type. Is this an example of a formerly ignored distinction becoming important?

We do not think that Wakefield’s (1998) numerous attempts to salvage a workable definition of mental disorders from the DSM research program do much to make that program look more scientific and progressive. Indeed, by clarifying what would be required for a meaningful definition of mental disorders in the tradition of physical medicine, Wakefield highlights just how far short of those standards psychiatric medicine has fallen in the modern DSMs.

References


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