# Defining Mental Disorder

## Jerome Wakefield and His Critics



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# 7 Doing without "Disorder" in the Study of Psychopathology

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Jerome Wakefield has made major contributions to thinking about psychiatric classification, contributions that have important ramifications for the practice of psychiatry and related disciplines.[1] I argue in this chapter that those contributions are independent of his own views about the nature of psychopathology. Wakefield's picture of psychopathology on my view relies on dubious assumptions about the methods, aims, and abilities of philosophy; his account of psychopathology and the disciplines that study and treat it oversimplifies a complex reality. Nonetheless, these criticisms do not undermine Wakefield's contributions because those contributions do not depend on his specific account of psychopathology, and moreover, his oversimplifications point to a fruitful research agenda.

In section I, I outline some of what I take to be some of Wakefield's main contributions. Those contributions center on the recognition that the behaviors that get labeled as psychopathology are a heterogeneous lot, with important theoretical and practical consequences following. Section II argues that psychiatry does not need a foundation in a conceptual analysis of mental disorder and that searching for such an analysis rests on a mistaken philosophical project. Using evolutionary considerations to identify mental disorders is the topic of section III. Wakefield's actual points about the different behaviors that get labeled psychopathological are not actually supported by evolutionary accounts of mental disorders, and the prospects and uses for such accounts are limited. Section IV sketches an alternative pluralist view of psychopathology that makes the search for objective explanatory classifications of psychopathology paramount, a goal inspired by and consistent with Wakefield's insightful critique of psychiatric practice.

## I. Contributions

Wakefield's overriding concern is the proper application of psychiatric diagnostic categories. He (and his sometimes coauthor Horwitz) defends the view that some specific disorders are mistakenly applied. Unlike the antipsychiatry tradition, he does not argue that psychiatric classification and practice rest on a mistake tout court and/or

are entirely social constructions. Nor does he fall into the trap of arguing—as do some philosophers—that there is something about the nature of the mental or of folk psychological concepts that precludes the naturalistic study of psychopathology or renders current psychiatric classifications unscientific. Rather than making such blanket assessments, he argues case by case, looking at the application of specific diagnostic categories—a procedure that is a vast improvement over the practice of much antipsychiatry criticism. Wakefield finds that some alleged psychiatric disorders such as grief are not disorders at all. Yet his arguments for this blanket skepticism about particular disorders are compatible with a more limited skepticism about other psychiatric classifications such as depression. There are clear cases of behavior that are legitimately labeled as depressive disorders. Yet that is compatible with the psychiatric professions misapplying the diagnosis in some or even a great many cases and in a way that does not just rely on the measurement error that is unavoidable in the social behavioral sciences. Instead, some psychiatric categories are systematically misapplied in identifiable ways.

So contribution number 1 is promoting a healthy skepticism of psychiatric classification practices but one that does not throw the baby out with the bathwater. Contribution number 2 is supplying an imminently plausible justification for that skepticism and for the accompanying optimism about psychiatry's scientific possibilities. That justification is in the form of an empirical claim about the range of human behaviors. For some types of symptoms and behaviors, there are qualitative differences that we can reasonably identify and defend. The qualitative differences are between behavioral symptoms that result from more or less enduring abnormal psychological mechanisms or processes largely inside the individual and those that result from situational and time-bound circumstances that are part of the vicissitudes of life. Wakefield asserts that there are objective facts of the matter about which types of behavior are present and that we can sometimes make reasonable well-confirmed judgments about them. This claim is nontrivial. It goes against the popular grain of psychometric approaches to psychopathology that see psychopathology as a continuous and not categorical phenomenon and thus conclude that there is no nonarbitrary distinction between the normal and the pathological.[2] Contribution number 2 is important and interesting.

A third contribution from Wakefield instantiates the above points by providing compelling assessments of various putative psychopathological categories. His work with Horwitz on depression is perhaps the most substantial. There the argument is that the application of the current *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-IV*) criteria for depression leads to enormous numbers of false positives. If the *DSM* criteria are applied without taking into account situational factors, then a large number of individuals will be classified as suffering from a psychiatric depressive disorder when they are in fact only experiencing predictable and normal problems in living. Depressive symptoms can, Wakefield thinks, indicate a real psychological disorder when the symptoms are not the result of normal reactions to problems in living, so he is not a

skeptic about the category itself, only its misapplication. For other putative disorders, however, he provides compelling evidence that no disorder whatsoever is picked out. For example, he argues convincingly that the criteria for the category "prolonged grief disorder," which was considered for the *DSM-5*, describes what are normal reactions to loss, with understandable individual variation in the intensity and duration of those reactions. This conclusion is supported by recent work on the grief process. Because the proposed criteria would label normal behavior pathological and there is no prospect for building in situational exclusions as there is with depression, Wakefield (2012) thinks that the category as it stands does not pick out a mental disorder and argues against its inclusion in the *DSM*. It was not included.

## II. Unneeded Grounds

All the points made above were done so without appeal to a key component in Wakefield's writings: the harmful dysfunction analysis of the concept of mental disorder. Wakefield thinks that much rests on getting the correct analysis of the concept of a mental disorder. So he says, "What do we mean when we say that a mental condition is a medical disorder rather than a normal form of human suffering or a problem in living? The status of psychiatry as a medical discipline depends on a persuasive answer to this question" (2007, 149). This assertion that the status of psychiatry depends on having a proper analysis of disorder is repeated frequently across his substantial corpus of writings.

Wakefield also believes that having a correct account of mental disorder *in general* is necessary for having correct accounts of *specific* disorders. We can identify the sets of symptoms that make a specific disorder by showing that they reflect a malfunction in what some psychological mechanism was designed to do by natural selection. Where symptoms do not reflect the failed functions of evolutionarily selected mechanisms, mental disorder does not exist.

On my view the fact that the contributions cited in the first section do not mention Wakefield's conceptual analysis is a virtue, for I do not think the harmful dysfunction analysis is needed or generally helpful. In this section, I explain why I do not think we need an analysis of the concept of mental disorder and in the next why evolutionary notions of disorder are not generally helpful in understanding specific psychopathologies.

My qualms about the project of providing an account of "mental disorder" have their roots in a Quinian naturalism about philosophy and science. That naturalism doubts that there are conceptual truths that can be tested by intuition about possible counterexamples and by reports on what we would say. My naturalism also leads to doubts that philosophical analysis of concepts can provide results that allow us to rule on the epistemic standing of scientific disciplines. These doubts are grounded in skepticism about the analytic/synthetic distinction and the coherence of substantial a priori

knowledge. They are reinforced by work in the history, philosophy, and social studies of science showing that successful science does not first start with getting a definition in terms of necessary and sufficient conditions and then proceeding; instead, science often muddles along with concepts that are undefined or that are prototypes (like most human concepts) or that have competing but useful definitions depending on the application (see Wilson's [2006] wonderful detailed account of concepts in the applied physical sciences). The concept of a gene still has not precise analytic definition (Moss 2004), and thermodynamics made great strides despite having an only partially explicated and not entirely coherent concept of temperature (Chang 2004).

Let me concretize these general results from philosophy and philosophy of science with some questions about Wakefield's project defining disorder. We should first ask, who gets to vote in the game of definition and counterexample that is to define "mental disorder"? We should then ask how we are to tally the results. Do the intuitions of those outside the psychiatric professions count as much as those inside? What do we do when reasonable people report different intuitions about defining disorder? (This is not just a hypothetical question—there are perfectly reasonable people who think that being a disorder is a matter of deviations from playing a standard role in complex systems.) We can also ask what we would do with a conceptual analysis produced by such methods. Suppose there was a consensus among all those who participated in public debates about defining "mental disorder" about the correct definition. Would we really want then to use that sociological fact to tell psychiatric researchers what they should be studying and how they should be studying it? I don't think so.

So my argument contra Wakefield is that the disciplines that study and treat psychopathology do not need and can get along without a philosophically satisfying (i.e., necessary and sufficient condition) definition of mental disorder. In this respect, I think they are no different from the rest of medicine. As I have argued elsewhere (Kincaid 2008), there are no clear individually necessary and jointly sufficient conditions for being a cancer, but that does not mean oncology is based on a mistake and that its "status as a medical discipline" is at stake.[3] Oncology, like the rest of medicine, studies and treats conditions that it can objectively and reliably identify and conditions where it can predict and alter the course of development in ways that people perceive to improve their lives. Psychiatry presumably has the same pretensions if unfortunately not the same prospects. This alternative picture of psychiatry freed from any deep ties to disease concepts will be elaborated in the final section of the chapter.

I want to answer two likely objections, given Wakefield's published arguments. One is that we are being incoherent if we talk about psychopathology without a worked-out account of the concept of disorder, and the other is that we need an account if we are to avoid a nihilist social constructivism about mental disorders.

Don't we need an analysis of disorder to talk about psychopathology? Nothing prevents us from using the term despite having no strict definition; if we could only

use terms strictly defined, we would be in big linguistic trouble. The question is then whether the disease label is doing any useful scientific work (it no doubt has ethical and social consequences). I think it may well be that generally it does not, and thus psychiatric research should get on without the concept of a disorder. If we can find compelling evidence of malfunctioning evolutionarily selected psychological mechanisms, then it would have a role in those cases. I also find it plausible that some behavior that gets labeled pathological involves a breakdown in the normal operation of cognitive and neurobiological systems, the idea promoted by competitors to Wakefield's analysis of function. Here the term "psychopathological" would also have a more delimited and defensible meaning. If we eschew the conceptual analysis project of defining mental disorder, we can allow such a restricted use of "psychopathology" without committing ourselves to the view that a breakdown in normal system functioning captures all and only the ways we use terms like "mental disorder" or "psychopathology." However, I would argue that finding malfunctions in evolutionary mechanisms or breakdown of roles in a complex system are just valuable means to the end of getting objective, explanatory classifications of behavior that psychiatry and related disciplines study and treat.

A second objection by Wakefield to my suggestion that we do not need a conceptual analysis of mental disorder is this: "a central goal of an analysis of 'mental disorder' is to clarify and reveal the degree of legitimacy in psychiatry's claims to be a truly medical discipline rather than, as antipsychiatrists and others have claimed, a social control institution masquerading as a medical discipline." In other words, we need a clear defensible definition of mental disorder if we are to hold off the social constructivist nihilists massing at the gate.

The gate is surely worth defending, although many useful insights have come from the social constructivist camp as I sure Wakefield would agree. Still we can defend the gate quite nicely without a definition of mental disorder. What we need to show is that the psychiatric-related disciplines can produce objective and explanatory categorizations of behavior, ideally ones that lead to successful treatments. Tying those classifications to mechanisms selected by natural selection would be one way to ground them, but surely not the only way as I will argue in more detail in section IV. For example, the Big Five personality classification system relies on reliable and pyschometrically validated measures; scores on those measures predict differences in behavior. Here psychological phenomena are classified in objectively grounded ways that refute pure social constructivist stories and do so without any tie to evolutionary functions. So it is quite possible to avoid social constructionist conclusions without the machinery of evolutionarily selected mental mechanisms.

I should finish my rejection of Wakefield's conceptual analysis project by noting that I am not denying that clarifying concepts in the study of psychopathology is a worthwhile effort or that philosophers can contribute to such clarification. My objections are

to the grander project of establishing the legitimacy of fields that study psychopathology by providing a conceptual analysis of "mental disorder."

## III. Evolutionary Foundations?

Suppose we drop Wakefield's conceptual analysis project. Is it nonetheless still true that Wakefield has shown that the best way or a good way to understand specific psychopathologies is in terms of the failure of evolutionarily selected psychological mechanisms? I consider this part of Wakefield's project in this section.

A look at Wakefield's various discussions of psychopathological concepts such as depression shows that an appeal to evolutionary functions plays little role. In exposing the false-positive rate in diagnoses of depression, Wakefield does not tell a plausible evolutionary story about the origins of the relevant psychological mechanisms and then go on to argue that those mechanisms are functioning properly in the cases where psychopathological concepts are being misapplied. Instead, he argues that the behavior in question is a normal and reasonable response to a life circumstance. Depressive symptoms in the case of a lost job or marital breakup are normal, understandable responses to life events and thus should not be classified as depressive disorders. What we need according to Wakefield are "judgments of proportionality—that is, whether the nature of a triggering stressor is capable of explaining, within the normal range of emotional processing, the severity of the resulting symptoms" (2012, 181). It is the appeal to the normal range, not the proper functioning of a mechanism produced by natural selection, that is doing the work here.

At times, Wakefield talks as if picking out mechanisms can be easily done even if we do not have a good evolutionary story. So he suggests that we know that the eye is designed for seeing and that we can know that without having any good idea about the design process. *Maybe* that is true for biological functions, but is that true of evolutionarily evolved mental mechanisms? I doubt it.

If we look at the work of biologists actually applying detailed evolutionary considerations to psychopathology, then we run into reasons to doubt that evolutionarily based dysfunction accounts fit with the kind of (reasonable) intuitive judgments that Wakefield wants to make about which symptoms constitute disorders and which do not. The problem is that there are plausible evolutionary stories where a wide range of behaviors that we are inclined to call disorders turn out to be the products of evolutionarily selected mechanisms. There are at least three potential facts supporting this conclusion: the likelihood that psychological mechanisms producing false positives would be fitness improving, the prospect that fitness-enhancing psychological mechanisms in the Pleistocene may be invoked in maladaptive ways in complex industrial societies in which they did not evolve, and the general fact that evolution produces traits with a

wide reaction norm, raising the prospect that extremes of human behavior that we call pathological are the distributional tails of normal traits.

Success under natural selection requires a trade-off between costly unnecessary responses to threats to reproduction and costly failures to detect such threats. Where the threat to reproductive success is death, it is not hard to imagine that natural selection would err on the side of false positives. A one strike and you are out threat would seemingly produce traits that produce lots of false positives in reacting to such threats, given the extreme consequences of a false negative.

What do such false positives have to do with psychopathology? Two of the most prototypical psychopathologies—various forms of severe anxiety and depression—have natural stories as being evolutionarily selected false positives. Depressive symptoms can be seen as promoting avoidance of, and helping to repair, the breaking of social bonds, something that no doubt had drastic consequences in our evolutionary history. The motivation to avoid depressive pain by behaving in ways to protect social bonds is fairly obvious. The repairing role that depression may play takes more storytelling (see McGuire et al. 1997), but there is compelling evidence from our primate cousins about some of that story in terms of restoring social bonds in changing dominance hierarchies. Similar narratives exist about extreme anxieties, where the threats are both social and environmental. Possible live and let live, laid-back ancestors may have fared badly in ancestral environments where threats to life were ever present and where the cost of social exclusion was deadly as well. In short, significant anxiety and depression may have just been the price our ancestors paid for reproductive success and the price we pay for being their ancestors.

Another just so story—one in principle compatible with the one just told—does not require widespread Pleistocene depression and anxiety but finds the roots of disorder in past evolved mechanisms that have to deal with the complexities of modern society. This is a route familiar in broad outlines from Freud in *Civilization and Its Discontents* (1962). The idea is that modern society provokes in abundance naturally selected mechanisms producing depressive and anxious systems. We can give this just so story a quite concrete guise by thinking, for example, about poverty and psychopathology. A few miles from where I live, there are over a million people cramped into wall-to-wall corrugated metal shacks without running water and sanitation. Most live on less than $2 a day. Probably 50% are unemployed. The murder rate is enormous and sexual assault pervasive. Wakefield argues that depressive symptoms in response to a job loss should not be counted as a disorder. However, what if the job loss is a repeated or a permanent part of life as it is for the residents of these townships and the response is permanent depressive symptoms? Should we deny that they have a depressive disorder? What about crippling anxiety in women subject to ongoing sexual abuse? Since in my view, "only" ethical and social considerations turn on what we *say,* nothing social scientific

is at issue in asking these questions and to that extent they can be ignored. Instead, the question is whether these conditions allow for objective, explanatory classifications, a question that makes no essential judgment about evolutionary considerations. But this is not Wakefield's view. Such examples should be a chance for clarification of his views.

A third evolutionary approach arguing that psychopathology is the standard functioning of naturally selected traits points out that biological traits can have a wide reaction norm. Seemingly normal traits in common environments can exhibit extreme deviations from the average, given subtle changes in the developmental environment. The claim thus would be that major depression is just the tail of the expression of a normal, presumably adaptive, trait of sadness. This is the kind of view advocated by the psychometric tradition that wants to treat psychopathology as a continuous trait and replace talk of disorders and psychopathology with talk of abnormal behavior.

Neither of three pictures above is beyond doubt. However, they do suggest some of the kinds of questions we must answer if we want to ground judgments about psychopathology on the idea of properly functioning mental mechanisms. Pursuing Wakefield's general approach thus raises a variety of fruitful research questions, albeit difficult ones.

## IV. An Alternative Picture

The alternative approach I favor denies that we must have a clear conceptual analysis of disorder in order to understand the practice of psychopathology research or for that research to form a coherent scientific enterprise (Kincaid 2014). Instead, I think that the ideal for the sciences of psychopathology is to establish the existence of objective, explanatory classifications. Let me explain what I mean by this terminology and then discuss the role of evolutionary and other considerations in identifying these classifications.

"Objective" classifications, as I use the term, are ones that put individuals into classes based on real differences in facts about those individuals.[4] One important way to show that we have real differences is to provide evidence that they can be identified by evidentially independent means. "Explanatory" classifications, as I am using the term, are those that ground regularities and causal relations. We get evidence for regularities and causal relations by showing that our classifications of individuals allow us to make successful predictions about (1) the factors determining who comes to exhibit the behaviors that the classification picks out and (2) the outcomes that the classified individuals undergo.

So, for example, consider the case of major depression (Kincaid 2014). There are multiple sources of evidence that we can pick out a qualitatively distinct set of individuals based on their behavior broadly construed. Taxometric analyses (Ruscio et al. 2006) using diagnostic screens as indicators point to a dichotomous division of individuals into a depressive taxon and a distinct complement group. Tests on cognitive

tasks and neurobiological measures such as functional magnetic resonance imaging (fMRI) differences in activation and differences in cortisol secretion provide additional independent lines of evidence in addition to the taxometric analysis of responses to diagnostic screens. So there is evidence that distinct differences between individuals are being picked out by the classification of depression. That classification then allows successful identification of causes and consequences of depression. Belonging to the category is predicted by family history and histories of marital discord, food insecurity, and job stress. Belonging to the category predicts subsequent suicide attempts.

The fields studying psychopathology will be vindicated as scientific endeavors if they can produce for other domains of behavior results like the findings for depression. Such success would be entirely consistent with Wakefield's concern to avoid lumping together problems in living with major psychiatric conditions. It would not, however, require an account of the concept of mental disorder or that specific disorders be identified with evolutionary malfunctions.

On the general picture just sketched, evidence about evolutionary dysfunction would be a useful but not essential ingredient. Accounts of evolutionary function and dysfunction could, for example, motivate the kinds of predictors of psychopathology we should look for. However, none of the evidence I listed about the existence of an objective category picking out depression relied on evolutionary considerations or essentially assumed that the behavior constitutes a disorder in any substantial sense. I should also note that the evidence did not rely strongly on the assumptions that some of Wakefield's critics—those who claim that the true meaning of disorder is the breakdown of roles in complex systems—think essential. Some of the fMRI evidence might be interpreted in these terms, but at the present, the connections are still fairly speculative. As with evolutionary functions, functions in the sense of roles in complex psychological and neurobiological systems might well be of use in picking out the objective, explanatory classifications. Yet, fortunately, researchers can make progress without having anything like a full, well-confirmed theory of cognitive-neurobiological functioning.

## Notes

1. It is hard to know how much influence his contributions have had on practice, but it seems clear that their influence is unfortunately not as large as they should be (witness the dropping of the bereavement exclusion from the *DSM-5* criteria for major depressive disorder).

2. The conclusion is unnecessary. Psychopathology might be manifested in continuous rather than categorical traits that in no way are distributed across the entire population. Huntington's disease varies significantly in severity, but it is not the case that everybody has at least a little bit of it.

3. They commissioned a study on whether they should worry about whether obesity is a disease. They concluded that there might be ethical and social reasons to answer the question but that scientifically, the question was irrelevant (Allison et al. 2008).

4. I am setting the issues up here on the assumption that psychopathologies are categorical rather than dimensional. While I think that is true for some prime examples of psychopathology, it is not essential for there to be objective, explanatory categories; they might be dimensional in nature.

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