Defining Mental Disorder

Jerome Wakefield and His Critics

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6 Do Clinicians Understand the Harmful Dysfunction Analysis of Mental Disorder? Reply to Leen De Vreese

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I thank Leen De Vreese for continuing, via her essay, our interaction begun in person some years ago during my very enjoyable visit to Leuven and for her thoughtful challenge to the viability for illuminating medical practice of my harmful dysfunction analysis (HDA) of medical, including mental, disorder. The HDA claims that "disorder" refers to "harmful dysfunction," where dysfunction is the failure of some feature to perform a natural function for which it is biologically designed by evolutionary processes and harm is judged in accordance with social values (First and Wakefield 2010, 2013; Spitzer 1997, 1999; Wakefield 1992a, 1992b, 1993, 1995, 1997a, 1997b, 1997c, 1997d, 1998, 1999a, 1999b, 1999c, 2000a, 2000b, 2001, 2006, 2007, 2009, 2011, 2014, 2016a, 2016b; Wakefield and First 2003, 2012). Like the classic answer to the question of how a musician gets to Carnegie Hall, De Vreese thinks that the way a philosopher gets to an account of disorder is *practice, practice, practice*—that is, immersion in the study of clinical practice in all its diversity and contextual anchoring. She argues for an analysis of "disorder" that "is grounded in practice rather than [conceptual] intuitions." Clinical judgments are based on the clinician's varying presuppositions, thus best understood within a pluralistic approach, she argues, whereas conceptual analysis arrogantly imposes one univocal meaning of "disorder" on clinical diversity and should be abandoned.

I agree that understanding clinical practice in all its rich detail independent of conceptual analysis is an interesting and important undertaking, albeit one that arguably might be better undertaken by social scientists than philosophers. However, rather than seeing the study of disorder judgments in practice as being *opposed* to an HDA-like conceptual analytic approach to "disorder," I understand the two approaches as importantly complementary given that the concept of disorder is one influence on clinical judgment. A symbiosis of the two is especially necessary for recognizing and correcting misdiagnosis, a crucial goal for which a grounded view that accepts clinical labeling at face value and simply tries to describe and understand it provides no help. Only an analysis of the concept of disorder provides a way to evaluate whether what a clinician judges in practice in a given context is correct or an instance of the

overpathologizing of normal distress or social deviance. In effect, De Vreese's uncritical practice-descriptive approach suggests that a disorder is whatever each psychiatrist chooses to label a disorder, which leaves the antipsychiatric critique of diagnosis unanswered. Elevating the clinician to a position of such privileged classificatory authority involves its own form of arrogance.

Before considering De Vreese's specific objections to conceptual analysis, an oddity of her argument—and consequently of my reply as well—needs to be mentioned. In undertaking to explain the methodological reasons for the HDA's failure, De Vreese does not argue but simply *presupposes* that the HDA in fact fails to explain disorder judgments and is subject to decisive counterexamples. However, De Vreese herself presents no such counterexamples. Instead, she in effect outsources the HDA's refutation to others, citing two sources for the HDA's refutation:

> It cannot be denied that Wakefield's approach has also been refuted in the literature on the basis of counterexamples demonstrating that people's intuitions are not always in accordance with the HDA. It is not the goal of this chapter to go into details about the shortcomings of, and counterexamples to, the HDA. These can be found in the literature (see, e.g., Cooper 2007; Schwartz 2007).

I consider De Vreese's methodological objections to conceptual analysis on their own merits, below. However, to fully answer De Vreese, I also need to address the two cited philosophers' critiques of the HDA that according to her "cannot be denied" and that form a presupposition of her methodological critique. Rather than going that far afield in this reply, I focus here on De Vreese's methodological claims and address Cooper's and Schwartz's proposed counterexamples to the HDA in a supplementary reply to Cooper in this volume.

De Vreese's Methodological Objections to Conceptual Analysis

I now turn to De Vreese's criticisms of the conceptual analytic methodology I use to support the HDA. First, De Vreese argues that it is arrogant to claim that one can get *the* right answer to what a concept means, so one should remain more modestly pluralist. However, it is presumably not arrogant to be open to *the* most explanatory account, whether univocal or pluralist. One may wonder at the consistency of an avowed pluralist who, observing the complementary methods of grounded description of practice and conceptual analysis, sees a zero-sum competition and insists that *the* right method is the study of practice, thus endorsing a methodological monism (albeit one that embraces a pluralism across clinicians and situations) when one can perfectly well do both.

Second, De Vreese expresses impatience with the slow and contentious process of formulating and testing theories of classificatory judgments; "the whole debate about the 'right' definition of the concept is driven by a game of giving examples and

counterexamples"; "And so the story goes on: authors … keep arguing and counterarguing about the right definition of disorder." Granted, evaluating explanatory power and evidential support in a contentious field is often lengthy and tedious, but these "games" are the best available technique we have for gaining clarity and improving explanatory power over time. De Vreese offers no reason to think that theorizing about clinical practice rather than concepts would yield any less contentious or lengthy disputes. Indeed, one cannot help but wonder whether there is any area of philosophy that would remain standing if subjected to De Vreese's impatient gaze. One recalls here the joke: "Question: What is the difference between philosophers and Rottweilers? Answer: Rottweilers eventually let go."

Third, De Vreese argues that there are many types of disorders that do not fit one mold: "I will argue that disease is a multifaceted concept, for which a single definition making a straightforward dichotomy between disorder and nondisorder is not justifiable, not necessary, and not useful for practice." However, from the fact that disorders themselves are diverse, it does not follow that there is no univocal conceptual definition that subsumes them all. (On this issue, see also my reply to Zachar in this volume.) Great diversity exists among chairs (bean bag versus leather recliners), elements (helium versus gold), animals (snails versus chimpanzees), water (steam, ice, and liquid water), and stars (neutron stars versus red dwarf stars), yet one can give decent univocal accounts of the features that explain why something falls under each of these categories. De Vreese confuses multiplicity of *kinds of disorders* with the idea that there is no univocal *concept of disorder* that unites the multiplicity.

Fourth, De Vreese argues that individual clinicians' intuitions about how to classify conditions are anchored in the varying contexts in which they make those judgments, and this implies lack of a univocal concept, so a one-size-fits-all conceptual analysis is misguided and hopelessly prescriptive. Yet, precisely the opposite is true. A survey and analysis of the contextual features in the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM*) diagnostic criteria revealed that taking into account contextual variation when diagnosing superficially similar conditions is actually a strategy for distinguishing internal dysfunctions from normal responses and so determining whether a condition satisfies the univocal concept of disorder (Wakefield and First 2012). The contextual information provides information about whether core features of the univocal disorder concept hold in that context. For example, intense chronic anxiety usually implies an anxiety disorder *unless* there is a chronic contextual threat that explains such anxiety as a normal response. The HDA thus potentially explains *why* varying contexts yield the varying judgments they do.

Perhaps De Vreese can be interpreted here as claiming more strongly that the *intuitive judgments of category membership of one and the same condition in one and the same context* vary, thus casting doubt on the existence of an evidential base of shared classificatory judgments of disorder and nondisorder that are needed for a conceptual analysis.

The problem with this argument is that there is a remarkable degree of prima facie agreement about many conditions' normality or pathology among a large number of observers, providing an ample target for explanation. Robert Spitzer, the most eminent psychodiagnostician of the twentieth century and someone who, from his experience editing *DSM-III* and *DSM-III-R*, knew a thing or two about agreement and disagreement among psychiatrists and laypeople about diagnosis, put it this way:

> What is remarkable—and is in keeping with Wakefield's analysis of the problem—is the great degree of consensus that exists about whether particular psychological or physical conditions are or are not disordered in the absence of a definition of disorder in general. Neither physician, psychologist, nor the public have any problem in agreeing that childbirth (painful), being in love (overevaluation of the loved object), and normal grief (marked distress) are not disorders and that unprovoked panic attacks (dysfunction of the anxiety system), severe depression (dysfunction of mood regulation), and schizophrenia (dysfunction of reality testing and motivation) are disorders. (Spitzer 1999, 430)

Fifth, above all, De Vreese argues that individual clinicians' intuitions about how to classify conditions are based on their varying presuppositions about values and facts, so that analyzing the concept of disorder in a one-size-fits-all way is futile: "Why do we not just admit that presuppositions cannot be excluded and start in the development of an approach from the use of the concept in *practice*. Would this not be much better than acting as if one can start from scratch in making a traditional philosophical conceptual analysis on the basis of armchair intuitions?"

The problem here is that De Vreese's pluralist proposal simply accepts such divergent judgments based on varying presuppositions at face value, providing no understanding of why certain presupposed beliefs and values nonrandomly tend to yield certain judgments of disorder. The concept of disorder is an explanatory construct explaining these links. De Vreese's "presupposition" argument reveals a misunderstanding of the role of conceptual analysis in an account of practice. She writes as if a conceptual analysis by itself determines whether a certain condition is or is not a disorder, but in fact a conceptual analysis *explains why certain background presuppositions lead to the disorder judgments* that they do. The fact that different presuppositions lead people to different disorder judgments is consistent with there being one shared concept of disorder that mediates the relationship between presupposition and judgment.

Concepts by themselves do not determine classificatory judgments. You and a friend can totally agree on the meaning of the term "bachelor" and totally disagree about whether a specific individual you met at a party is or is not a bachelor based on different observations, beliefs, and inferences. Concepts only set the conditions under which something falls within a category, and one's beliefs about whether those conditions are satisfied provide the presuppositions that determine one's category judgment. The concept and the presuppositions are jointly necessary for explaining classificatory judgments.

A sixth objection by De Vreese is that the HD emerges from armchair intuitions and that such analyses "do not have practical implications." These claims are inaccurate. The HDA emerged from my attempts to grapple with clinical intuitions taken from clinical practice and empirical research results concerning conditions as varied as major depression and female orgasmic disorder (see Steeves Demazeux's chapter in this volume for a discussion of my early sexual disorder papers). It has since been applied to many disorders (e.g., Wakefield 2011b, 2016, 2019; Wakefield, Horwitz, and Schmitz 2005; Wakefield and Schmitz 2017a) and been the basis for two research programs, one on clinical judgment (e.g., Wakefield et al. 2002; Wakefield et al. 2006) and the other on epidemiological estimates of prevalence (e.g., Wakefield et al. 2007; Wakefield and Schmitz 2015, 2017b; Wakefield, Horwitz, and Lorenzo-Luaces 2017; Wakefield, Schmitz, and Baer 2010). As opposed to, for example, Boorse's biostatistical theory of disorder, which is generally cited by philosophers but not by clinicians and clinical researchers, the HDA is heavily cited in the mental health literature. In contrast, De Vreese fails to provide even one example of how the study of practice in the way she suggests yields fruitful philosophical or diagnostic understanding. (I comment below on the example of attention-deficit/hyperactivity disorder [ADHD] that she develops in another paper.) The reality is that the HDA is the most clinically anchored—and clinically cited—analysis of the concept of mental disorder of those currently available and has been extensively deployed by nonarmchair mental health clinicians and researchers in discussions of the validity of diagnosis and in debates over revisions to diagnostic criteria more than any other approach.

Finally, although De Vreese is generally allergic to the notion of "dysfunction," she suggests at one point that according to her pluralistic approach, *dimensions* of dysfunction and harm should replace the HDA's categories:

> One might in fact end up with three kinds of disorders: harm and dysfunction-related disorders (where you have a clear case of harmful dysfunction), primarily dysfunction-related disorders (where you have a clear dysfunction and vague reasons also to suspect harm), and primarily harm-related disorders (where you have a clear case of harm and vague reasons also to accept the presence of a dysfunctional mechanism). This would lead us to a pluralist definition, which is clearly in contradiction with Wakefield's goal of finding an overarching definition.

Rather than being "clearly in contradiction," the switch to dimensions is entirely consistent with the HDA. I have argued that dysfunction and harm are both fuzzy concepts, so of course there will be clear cases of dysfunction in which harm is debatable and clear cases of harm in which dysfunction is debatable. The HDA would predict, however, that the less clear the dysfunction or harm judgments, the less clear the disorder judgment, and in the extreme case of dysfunction with no harm or harm with no dysfunction, there would tend to be a nondisorder judgment. That is perhaps a prediction on which De Vreese and I would disagree and thus a useful test. (See my reply to De Block and Sholl in this volume, as well as Wakefield 2014, for examples of how lack of harm yields lack of disorder status.)

Why Haslam's (2002) "Kinds of Kinds" Fails to Support De Vreese's Disorder Pluralism

De Vreese (2014) states that her pluralistic anti-HDA view of uses of "disorder" is "based on the view of Nick Haslam (2002)" (37). I believe there are many who similarly interpreted Haslam's analysis in that paper to be in conflict with the HDA. Haslam is an exceptionally thoughtful scholar and one of the few researchers engaged in both empirical and conceptual exploration of the foundations of diagnosis. However, the Haslam paper referenced by De Vreese provides no legitimate rationale for rejecting the HDA in favor of pluralism about "disorder," for reasons I will now explain.

Haslam (2002) argues for a pluralistic taxonomy of types of disorder constructs. He appropriately laments the unjustifiably constricted ideological views that permeate psychiatry according to which all psychiatric categories must refer to one kind of construct, whether the neo-Kraepelinian's biological essentialism, Zachar's "practical kinds," currently fashionable dimensional constructs, or social constructivist notions: "Psychiatric disorders are presented as being uniformly of one kind. … The possibility that different structural models might capture different forms of psychopathology goes unrecognized" (2002, 209). Haslam reasonably argues that it is more likely that science will reveal various kinds of constructs underlying disorder categories and that there are probably a variety of kinds of constructs underlying current provisional categories. Haslam's corrective to dogmatic construct monism is to formulate a taxonomy of possible types of disorder constructs that offers "a pluralistic view of psychiatric classification … according to which psychiatric categories take a variety of structural forms …—non-kinds, practical kinds, fuzzy kinds, discrete kinds, and natural kinds" (2002, 203), organized as a hierarchy of progressively more structurally demanding types of constructs, analogous to the hierarchy of measurement scales from nominal to ratio, "with each successive structure meeting a requirement that the preceding one does not" (2002, 204). Haslam argues that our current set of psychiatric categories can be plausibly interpreted as encompassing constructs of these various forms, and he offers examples of psychiatric categories that may provisionally be understood as fitting each of the proposed types of constructs.

Regarding the relation between Haslam's paper and the HDA, nothing Haslam says in his paper about construct pluralism addresses, let alone answers, the question addressed by the HDA: what qualifies a category (of whatever kind of construct) as a category of psychiatric disorder? All of Haslam's types of constructs apply to normal as well as disordered conditions, so one needs an additional conceptual level of analysis not addressed in Haslam's paper to determine which constructs are disorders and which are not. Pluralism of disorder *constructs* does not imply pluralism of disorder *concepts*. Haslam argues only that various conditions already recognized independently as disorders fall into various different levels of his construct typology. He never argues for the absurdity that being an instance of one or another of his types of constructs

is sufficient for being a disorder because he understands that his various constructs can exist on both sides of the disorder-nondisorder divide. To use Haslam's analogy, the fact that something is measurable at one or another level of the typology of scales from nominal to ratio tells you nothing about whether it is a disorder or nondisorder because both disorder and nondisorder categories can be measurable in various ways. Haslam's pluralistic typology of constructs is neutral on what constitutes a disorder and thus provides no support whatever for De Vreese's anti-HDA position.

Two concerns I have about Haslam's analysis are worth brief mention. The first is that he tends to take an overly blinkered psychometrician's view. For example, Haslam's discussion of his example of depression as a dimensional construct is problematic because the symptom-severity continuum on which depressive disorder is commonly claimed to fall and which Haslam embraces as the framework for his discussion is in fact orthogonal to the defensible common distinction between normal versus disordered sadness, in which severity is one dimension and dysfunction is another. Thus, normal grief and other clear cases of normal sadness in response to life events can be more severe than some mild cases of pathological depression. A second and related concern is that Haslam emphasizes that any smoothly distributed dimensional continuum along which individuals differ by degrees of a given variable is excluded from being divided into a categorical kind and is thus a nonkind because "although a binary distinction can be imposed on such a continuum, its placement is purely arbitrary; there is no correct location where the line should be drawn, and any such line creates a discontinuity that is merely artificial" (2002, 204). Of course, if you assume that you are limited in your considerations to only the dimensional continuum and have no other facts at your disposal, then by definition, Haslam's point is correct. But in real life, nosologists and scientists are never that limited. There is always the question of whether there are ways of understanding superficially continuous distributions in terms of deeper processes so that there are theory-driven nonarbitrary cut-points. The reason for such an inference could be as abstract as a belief about which part of the dimension was likely responsible for the natural selection of the mechanisms generating the dimension versus which part was not. (See my reply to De Block and Sholl in this volume for an extended example of this sort of inferential division of a continuous symptom-severity dimension.)

The Example of Attention-Deficit/Hyperactivity Disorder (ADHD)

I now consider De Vreese's comments on ADHD, first in her chapter in this volume and then in another paper of hers (De Vreese 2014) to which she refers the reader that makes clearer her own approach to the concept of disorder. In her present chapter, De Vreese is skeptical that the conceptual meaning of "disorder" actually shapes clinical judgments: "Clearly, no single physician will change his or her mind on whether, for example, attention-deficit/hyperactivity disorder is a 'real' disease on the basis of a

conceptual analysis of disorder according to which it is not. Therefore, it seems better to recognize the diversity in the meanings we put on the concept disorder in practice." De Vreese's notion that clinicians are unaware of disparities between disorder diagnoses in practice and conceptually correct disorder attributions, or that they do not change their minds about disorder diagnoses when evidence conflicts with the concept of disorder, is dramatically out of touch with the realities of practice. In the United States, the need to use a diagnosis to justify insurance reimbursement means that clinicians are constantly confronted with conditions that they do not believe are disorders based on their conceptual understanding but that they diagnose as disorders anyway to justify treatment reimbursement. As *New York Times* articles have documented (e.g., Schwartz 2012), it is common for clinicians to diagnose school-challenged children with ADHD and prescribe medication whether or not they are believed to be disordered. I have asked New York psychiatrists who have exclusively ADHD practices what percentage of the children that they diagnose with ADHD do they believe are having difficulty in school but do not actually have a disorder, and invariably they estimate that more than half of their diagnosed patients are not really disordered. Clinicians are quite aware of disparities between a diagnosis they give *in practice* for a variety of pragmatic reasons versus whether a condition really satisfies the conceptual requirements for being a disorder.

This phenomenon goes well beyond ADHD. Many diagnoses such as adjustment disorder, depression, and anxiety categories are routinely applied to individuals in distress who, clinicians will tell you in private, are suffering from normal emotional reactions but will benefit from therapeutic support. Typically, reimbursement for treatment of marital problems, which is not itself reimbursable, is obtained by classifying the normal reactions of distress to marital discord as depressive or anxiety disorders in each partner. The category of substance abuse was not believed to be a category of genuine disorder by many nosologists and substance addiction specialists and it was rejected by the *International Classification of Diseases* (*ICD*), yet for decades, the pragmatic pressure to justify insurance reimbursement to help all those with substance use problems caused *DSM* committees to retain it as a diagnostic category (it was finally eliminated in *DSM-5*). Similarly, *DSM-5* shrank the category of autism spectrum disorder to eliminate some milder cases formerly classified as Asperger's disorder that critics argued are normal-range eccentricity rather than mental disorder, but the implications for special education funding were so controversial that *DSM-5* added a clause that overrides the revision and allows diagnosis of anyone who qualified for *a DSM-IV* Asperger's diagnosis. In the other direction, at the time that homosexuality per se was eliminated from *DSM-II*, many psychiatrists who voted for that momentous change in fact believed that homosexuality does represent a form of psychopathology but felt that this consideration was overridden by the need to help end the unjust oppression of homosexual individuals by declaring the condition a nondisorder. In sum, studying *diagnostic practice* is not quite the same as studying *disorder judgments*, and understanding the concept

of disorder is necessary to understand how diagnostic practice and conceptual understanding can diverge.

What about De Vreese's denial that the concept of disorder has the power to explain changes of mind about disorder status? A conceptual analysis alone would not convince someone to change their mind about a condition's disorder status, but that plus changes in beliefs about a condition that imply that the condition does not satisfy the conceptual requirements for disorder could cause a change. In this way, research casting doubt on the existence of dysfunction in the HDA's sense regularly changes clinicians' and researchers' views of ADHD's disorder status. For example, recent research showing that it is disproportionately the youngest children in a class that get diagnosed with ADHD (e.g., Elder 2010; Evans et al. 2010; Holland and Sayal 2019; Zoega et al. 2012) has convinced many experts that normal variation in developmental rate is being confused with dysfunction, so that these younger children are likely being misdiagnosed. The behavior is the same, and it does the same harm to the child's school performance, but if there is no dysfunction, it is inferred that there is no disorder. Recent findings in genetic research (Eisenberg et al. 2008) have similarly convinced experts that some children currently diagnosed with ADHD do not have a dysfunction and so are not disordered. For example, *New York Times* psychiatric reporter Richard Friedman introduced an article reporting his change of mind as follows:

> Recent neuroscience research shows that people with A.D.H.D. are actually hard-wired for novelty-seeking—a trait that had, until relatively recently, a distinct evolutionary advantage. Compared with the rest of us, they have sluggish and underfed brain reward circuits, so much of everyday life feels routine and understimulating. To compensate, they are drawn to new and exciting experiences and get famously impatient and restless with the regimented structure that characterizes our modern world. In short, people with A.D.H.D. may not have a disease, so much as a set of behavioral traits that don't match the expectations of our contemporary culture. (2014, 1)

De Vreese allows that both value and factual elements enter into the notion of disease but does not spell out her own view in her chapter in this volume. She is more explicit in a recent paper to which she refers the reader (De Vreese 2014), in which she asserts that a prototypical disease is a disvalued condition with an identified and medically manipulable cause:

> I argue that our use of the term is determined by two interacting factors. One of these is value-laden considerations about the (un)desirability of certain physiological and/or psychological states. The other is the discovery of bodily and/or psychological causes which are explanatorily relevant in view of possible medical interventions to prevent, cure, or at least improve the undesired state. (De Vreese 2014, 38)

In terms of evidential support and explanatory power, De Vreese's account is at odds with the history of medical and psychiatric practice and fails both as a necessary and a

sufficient criterion for disease. Regarding necessity, the causes of even those conditions that are considered the clearest cases of disease are often entirely unknown and thus should not fall on the clear end of the perceived disease continuum according to De Vreese's account. Before Pasteur, no one knew what caused most infectious diseases, yet bubonic plague, smallpox, and cholera were prototypical cases of disease. Today, many diseases—for example, many of the most horrific neurological diseases—remain unknown as to etiology, yet are considered crystal-clear cases of disease. Moreover, there is not even one major mental disorder for which we have a good theory of causation, yet some of them, such as schizophrenia, are generally considered prototypical cases of mental disorder. It is clear that a disorder judgment can be based on a justified inference that there is a certain kind of cause—namely, a dysfunction—and no actual knowledge of the cause is needed. The same evidence goes against the necessity of De Vreese's manipulability requirement. Many cases of disease fall on the consensually clear end of the disease continuum but for which there is no known treatment. Indeed, due to unique financial incentives, an entire industry exists to find treatments for rare diseases for which there is no effective treatment.

Regarding sufficiency, De Vreese's account is subject to massive numbers of counterexamples because there are endless disvalued, causally understood, and medically manipulable conditions (e.g., pain in childbirth, grief, homeliness) that are predicted by her account to be high on "diseaseness" but are not considered disorders. Notably, all of the above disconfirmations disappear if one replaces the "known manipulable cause" criterion with a "dysfunction" criterion, whether the dysfunction is known or merely inferred to exist and whether the dysfunction is treatable and manipulable or not.

De Vreese considers the question, "Is attention deficit hyperactivity disorder (ADHD) in children a real disease?" (2014, 35). She observes that those who argue that ADHD is not a real disease "are convinced that these children's behavior merely demonstrates the differences in character that can always be found among children. Additionally, they argue, performance standards in our modern society are too demanding for the more active and impulsive children" (35). One would think that these rationales for refusing to label ADHD a disease would offer valuable clues to the concept. Instead, oddly ignoring the ground-level judgments and presuppositions that she claims to be privileged data, De Vreese dismisses such considerations, saying that they cannot "directly guarantee or refute the appropriateness of the disease label for ADHD" (35). The problem here for De Vreese's account is that both sides in these disputes agree that the disputed conditions are disvalued, internally caused, and medically manipulable (via well-tested medications), so her account predicts that everyone should agree that ADHD is a disorder. Yet, in fact, there is prominent and heated disagreement that her analysis can't explain and that she chooses to ignore. What does explain the two sides' differences in judgments is crystal clear from the literature: they disagree about whether the disputed conditions are in fact due to internal dysfunctions or rather result from normal-range internal variation mismatched to current social demands. This crucial

test of explanatory power between De Vreese's versus the HDA's perspective on ADHD falls squarely on the side of the HDA.

De Vreese also suggests a more pragmatic approach:

> All this does not answer the question of whether ADHD is *really* a disease. But according to the pragmatic framework developed here, that is a misguided question. It is not a matter of simply identifying it as a real or an unreal disease (whatever the latter might be). It is a matter of reflecting on why we actually are inclined to interpret ADHD as a real disease. What is the basis for this. (2014, 51)

This perspective in which we accept that ADHD is a disorder because some doctors label it so and simply explore why they so label it brings us back to a central problem with De Vreese's pragmatic approach; it has no conceptual place to stand to be critical of the practice it studies and so cannot address the deep problems that the analysis of "disorder" aims to address. One might wonder about drapetomania in the antebellum South, clitoral orgasm in Victorian England, and the Soviet political dissidents seeking liberty, all labeled as disorders. Given that medical treatment was available and effective in addressing these problematic conditions—runaway slaves could be medically sedated, women who experienced orgasms in Victorian England could have surgical clitoridectomies, and Soviet dissidents could be "cured" of their social protesting by psychiatric institutionalization and antipsychotic medication—De Vreese's position seems to entail that we take these conditions at face value and merely reflect on *why* they were considered diseases rather than subjecting them to conceptual scrutiny as to whether they were in fact real disorders. Her skepticism about the very notion of real versus bogus disorder implies that her view fails a crucial transcendental test of a fruitful analysis, namely, providing a basis for critiquing oppressive practice based on invalid application of the concept of disorder.

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