

**WHY WE SHOULD BE REALISTS ABOUT PSYCHIATRIC DISORDERS – REPLY TO**

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## WHY WE SHOULD BE REALISTS ABOUT PSYCHIATRIC DISORDERS – REPLY TO PETER ZACHAR

### 1. Epistemic Modesty isn't a Reason to Reject Realism

Resistance to realism about psychiatric disorders is, I think, generated mainly by a reasonable epistemic modesty about how far we can claim to have reached definitive conclusions about the right ways to carve up the disorders. But the resistance is, I'll argue, a mistake. We ought to keep the epistemic modesty while insisting that a principal aim of psychiatry is to find the correct ways to carve up disorders, realistically understood.

The mistake I'll try to identify is moving from the epistemic claim – we don't know much for sure about how to classify the disorders – to a metaphysical one: there really isn't any more to the disorders than is revealed by our current diagnostic categories. In physics, a parallel mistake would have been to say, in 1800 or so, 'Well, we don't know much about the ways in which atoms are structured, and perhaps we never will', to concluding, 'there's no more to the reality about atoms than is revealed by our current tests'.

What's really untenable is to try to hold on to the idea of 'epistemic iteration' of our diagnostic classifications – broadly, the idea that the development of diagnostic classification should be cumulative - while abandoning a realist picture of what we're aiming to get at with those classifications. Epistemic iteration makes perfect sense if you think of it as a matter of improving our ways of getting onto a structure of disorders 'out there'. If you've made the mistake of abandoning realism because of modesty about our current level of knowledge, then you have no way of explaining the sense in which one set of diagnostic procedures might be

‘better’ than another. I think that the significance of the mistake is well brought out by Peter Zachar’s insightful discussion.

Incidentally, you might be concerned that I am putting questions in terms of ‘realism’ without saying much about what that is. As we’ll see, the big issues here, as I understand them at any rate, do not actually require any elaborate stage-setting. For present purposes, the central question is whether psychiatric diagnosis should be thought of as aiming to identify a causal structure that implicates the symptoms of a disorder, or if diagnostic procedures have no such interest in identifying particular causal structures.

## **2. Framework Assumptions in Validation**

In his illuminating paper, Zachar is concerned with the assessment of diagnostic classifications, how we determine whether a proposed change to a DSM category is an improvement. His key point is that any procedure for validating categories must itself depend on ‘non-empirical’ assumptions. The problem is to know what to make of disputes over which non-empirical assumptions to employ.

His example is the argument over the entry on Personality Disorders in DSM-5. Proponents of the ‘alternative model’ of personality disorders had wanted to transition from thinking in terms of categories and criteria (such as ‘grandiose sense of self-importance’), which are binary – either one falls into the category or one does not, either one meets the criterion or one does not – to thinking also in terms of dimensions, such as ‘degree of grandiosity’, measures that can be used to place the patient on a scale with many possible values.

The Scientific Review Committee (SRC) wanted to provide a framework for the validation of psychiatric classifications. Rather than a reliance on ‘expert clinical opinion’ there was to be an explicit methodology for determining validity. However, Zachar remarks, much to the ‘eventual chagrin’ of the proponents of the dimensional approach, the SRC framework was ‘an expansion of the famous Robins and Guze (1970) program for validating *diagnostic categories* [categories being two-valued]...’ (p. 10). Zachar’s key point is:

The irony is that this attempt to alter the culture to establish a more dominant role for scientific considerations ended up clashing with the scientifically minded proponents of dimensional models in the domain of personality disorder. The proponents of dimensional models readily agreed with the SRC that allowing changes to be made based on expert clinical opinion should cease, but according to them expert clinical opinion is what led to diagnostic categories rather than dimensions.

(p. 10)

The trouble is that framework for validation used by the SRC already presupposes that validation must proceed in terms of categories and criteria rather than dimensions. It would of course be possible to shift to a framework that proceeds in terms of dimensions. But that shift would not itself be scientifically grounded in terms of either framework.

Zachar makes a similar point about the ‘Lack of a shared vision on the choice of standards for validating disorder concepts’ between proponents of the alternative model and the SRC. The alternative model was intended to satisfy a number of methodological goals, none of which were prioritised by the SRC. (For example, ‘Eliminate the non-informative diagnosis of

“personality disorder not otherwise specified”, ‘Increase the within-category homogeneity of personality disorder constructs ....’, ‘Eliminate the perception that there is a discrete threshold between disordered and non-disordered personality’, and ‘Comprehensively model the domain of personality pathology in an empirically based way ....’(p. 11).) Zachar, as I understand him, does not think of this difference as being open to empirical resolution, merely recording ‘the plurality of scientific and clinical goals that have been articulated in recent years’ (p. 12).

Zachar connects his point to a well-known passage from Kendler (1990):

Validation of a psychiatric disorder cannot, therefore, occur in a vacuum. While empirical data can provide answers, the most important questions to be asked derive from the construct of the psychiatric disorder under consideration. Therefore, one important limitation of a scientific nosology is that it cannot address the validity of a psychiatric disorder where there is disagreement about its proper construct. That is, data can only provide an answer if there is an agreement about what the question is.

This problem may be clarified by an example. Since the publication of DSM-III, there has been an ongoing debate about the proper construct for schizotypal personality disorder. Two positions have been articulated, which may be termed “familial” and “clinical.” The familial position defines schizotypal personality disorder as a nonpsychotic schizophrenialike syndrome that occurs commonly in relatives of schizophrenics and uncommonly in other individuals. The most valid criteria for this construct, then, would be those that best discriminate relatives of schizophrenics from relatives of controls.

The clinical construct for schizotypal personality disorder assumes that this syndrome should describe patients seen in the clinic who, despite the absence of classic symptoms of psychosis, nonetheless have substantial schizophrenialike symptoms. While this construct for schizotypal personality disorder less clearly dictates a single key validator, it suggests that one important test for proposed criteria would be their reliability vis-à-vis a “gold standard” clinician with extensive clinical experience in the assessment and treatment of such cases.

Thus, these two constructs for schizotypal personality disorder require two different hierarchies of validators. In fact, the clinical and research literature suggests that the familial and clinical constructs of schizotypal personality disorder do not define the same syndrome. Moreover, empirical data cannot determine which of the two is better, for this decision is essentially a value judgment as to which construct of the disorder is more conceptually appealing.

(Kendler (1990), 970)

The bombshell in this passage is in the last sentence. Zachar (p. 11) describes the framework assumptions required in determining validation as ‘non-empirical’, while Kendler here describes them as ‘essentially a value judgment’ relating to what is most ‘conceptually appealing’. But if this picture is correct, on which psychiatry as requires framework assumptions that cannot themselves be empirically grounded, then the prospects for a scientific psychiatry seem bleak. The use of data and experiment, statistical analysis and clinical testing, seem like distractions from the main point, that the procedures depend on assumptions that are themselves incapable of being established scientifically.

### 3. Doing Without Non-Empirical Framework Assumptions

The idea of a science as depending on framework assumptions that are not themselves empirically testable will be familiar to most philosophers, whether or not they know anything about psychiatry. But it's not the only way to analyze the situation, whether in other sciences or in psychiatry. It's true that in any context of scientific testing, we can make a distinction between the propositions that are being taken for granted in this context and those that are currently up for assessment. But that doesn't mean that there are any propositions that can't be established or refuted by empirical methods. There may not be any non-empirical propositions, certainly none with the substance of 'personality disorders are best assessed using dimensional terms in diagnosis', or 'schizophrenia is fundamentally a genetic disorder' (cf. Zachar and Kendler 2007 for sympathy with this point).

I'll look a little further at how you might elaborate the picture of the 'non-empirical framework' in the next section. Right now, I want to briefly sketch a well-known alternative picture and how it might apply in psychiatry. Consider the development of models of the electron. In his famous exposition, Hilary Putnam suggested that what's going on as successive models of the electron are developed is this. Out there is a phenomenon, the electron and its various behaviors, that is causally affecting us. That's what Bohr was talking about when he said 'electron'. Successive models of the electron are attempting to provide ever better descriptions of 'that thing', the thing to which we're causally responding in framing those models. Now why shouldn't we have this picture of what is going on in psychiatry, with successive diagnostic

procedures for a particular disorder simply being intended to be better and better ways of getting on to ‘that thing’?

On this view, we should have no need for ‘non-empirical’ framework assumptions in establishing the validity of a diagnostic procedure for a disorder. We may have to make conjectural assumptions that we do not yet know how to test, just as physicists often do, but that is a long way from supposing that those assumptions are inherently ‘non-empirical’ or ‘subjective’. For example, current work on the rate of expansion of the universe depends on the assumptions that the rate of change of distance between the galaxies we can observe is a fair sample of the rate of change of distance between galaxies generally; I don’t know how you’d establish that, but that is not to say that the assumption is ‘non-empirical’ or ‘subjective’. The correct way to think of it is rather to say that with continued ingenuity we continue to find new ways of determining factual questions that we couldn’t settle before. (For example, further findings about the Big Bang might affect our as to whether we have a uniform rate of expansion throughout the universe.) Whether personality disorders are best assessed by a dimensional approach, or whether schizophrenia is fundamentally a genetic disorder, seem to be empirical questions, whether or not they can be readily settled by our present techniques.

Realism about psychiatric disorders seems sometimes to be identified with the idea that there is a single latent variable, ‘depression’, or ‘schizophrenia’, that’s causally presumed to be causing the principal symptoms. But of course realism as such has no commitment to that kind of essentialist structure. The idea of a disorder as a ‘homeostatic cluster’ of symptoms causally holding one another in place is just as much a realist picture of the disorder as is an essentialist analysis (cf. Borsboom and Cramer 2013, Kendler, Zachar and Craver 2011). And of course, these are only the simplest possible models of the causal structure of a disorder. Various



different causal models of disorders, with latent variables postulated at different locations, are possible (cf. Ahn and Kim 2008). And of course, a hypothesis about the causal structure of a disorder is not non-empirical. Such hypotheses are always in principle testable, and can be expected to have an impact on what method we use to validate a diagnostic procedure for the disorder in question.

#### **4. Operationalism in Psychiatry**

Let's look at operationalism, the relevant alternative to realism here. As Bridgman put it, operationalism is the view that 'we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations' (Bridgman 1927, 5). Applied to intelligence testing, for example, operationalism is the view that all that we mean by 'intelligence' is the ability to achieve a particular score on intelligence tests. On this view, it is quite difficult to see what could be meant by saying that one type of intelligence test is 'better than', or an improvement upon, another. Each type of test measures what it does perfectly well (namely, the ability to do well on that test). Operationalism is not a particularly convincing account of the aims of intelligence testing. People engaged in intelligence testing have generally taken themselves to be trying to measure some underlying magnitude that also plays a role in many important aspects of people's lives. So it has usually been thought to be possible to make progress in intelligence testing, by filtering out the impact on test scores of, for example, such irrelevant factors as local cultural knowledge, which are thought to be extraneous to the presumed underlying magnitude. Or again, improvement in intelligence testing can be

established by showing better correlation between success on the tests and other outcomes, such as professional or educational achievement, that cannot be simply accounted for by factors such as socio-economic status that are, again, thought to be extraneous to the presumed underlying magnitude.

Of course, you might question whether there is any such underlying magnitude, 'g', to be measured. But that isn't as yet an endorsement of operationalism. It is, rather, a challenge to the usual concept of intelligence: it accepts that the concept is intended to identify something that is causally responsible for the ability to do well on intelligence tests, but denies that there is any such thing. So the right thing to do would be to throw out the concept, rather than keeping it and giving an operationalist interpretation of it.

Operationalism may seem more attractive for psychiatric disorders than for intelligence testing. No-one cares about the ability to do well on intelligence tests for its own sake, so the idea that this is all that the tests are intended to detect has little appeal. But in the case of disorders, the symptoms that are being used in diagnosis are characteristically of great significance in their own right. Given simply that someone has a particular collection of those diagnostic symptoms, before we make any attempt at diagnosis, you might say: 'Well, I'm not sure what, specifically, is wrong with this person, but it's quite evident that they need medical help'. So there is some kind of initial plausibility here that is entirely lacking in the case of intelligence testing, to the idea that the whole point of the diagnosis is simply to report what symptoms the patient has. And you might push the point by saying, well all we really care about here is making people better. The realist says:

What matters in diagnosis is identifying the causal structure, implicating the symptoms, that a patient has.

The operationalist sets aside any such concern with causal structures, and may say that all that matters is sorting patients into groups for which this or that treatment will work. Of course, on this approach, the diagnosis of patients may be expected to vary unpredictably over time. Patients which had traditionally been sorted into two separate groups, given two quite different types of treatment, may suddenly find themselves being sorted into the same group as new broad-spectrum drug treatments become available.

On this operationalist approach, you might still try to motivate something that looks like the epistemic iteration found in sciences that do concern themselves with causes and effects. You might argue for an incrementalist approach to change in diagnostic procedures, because of the enormous costs of retraining clinicians. Somewhat similarly, to adapt Kendler's (2012) example, a cartel of fashion houses might decide to go for only incremental changes in fashions, year on year, because of the costs associated with radical changes in fashion. So you could motivate something like incrementalism, without appealing to the idea of some causal structure 'out there' that you are trying to get onto.

I cannot help thinking, however, that to go down this line would be to abandon all that we hope for from a scientific psychiatry. A scientific psychiatry ought to be interested in causes and effects. In diagnosing disorders, a scientific psychiatry ought to be trying to identify the causal structures that generate and sustain disorders. To say that 'we know so little that we ought to confine ourselves to interpreting talk of disorders operationally' is to abandon this enterprise. It would mean accepting the status of treatments for disorders as potions or spells, that we have

found to work by trial and error, but into whose action we expect no insight. That is what it means to resist realism about psychiatric disorders, but I think that once it is made explicit there would be relatively few who would really endorse this.

An operationalist account of disorders would make sense of the idea that there are ‘non-empirical’ assumptions driving the use of one diagnostic procedure rather than another. If the diagnostic procedure is something carried out for its own sake, rather than to establish the presence of a causal structure out there, then differences between those who prefer different diagnostic procedures can only be ‘subjective’, rather than empirically grounded. What I am suggesting is that we ought to throw out this operationalism, and we should accept that methods of validation should themselves be empirically grounded.

### **5. Epistemic Iteration or Paradigm Shift?**

Peter Zachar expounds an line of thought in Kenneth Kendler’s (2012) work that lists three ‘barriers to successful [epistemic] iteration’ (p. 7). According to Zachar, this line of thought is consistent with resistance to realism: ‘Kendler’s own position was indefinite between advocating for such a realist vision based on scientific common sense and believing that realist views oversimplify psychiatric phenomena’ (p. 7). But I think it is very hard to know what is going on here unless ‘successful [epistemic] iteration’ is explained in terms of convergence on accurate identification of a particular disorder by different diagnostic procedures. To see this, let’s look at the three in turn:

- (1) The modified random walk: ‘A random walk is a sequence of discrete random movements. For epistemic iteration a random walk would correspond to a situation where there is no fixed and stable phenomenon to increasingly approximate. There can be change, but change would be non-systematic – often covering the same territory over again. Kendler’s example of a modified random walk is the changing length of women’s skirts from 1605 to 1936.’ (p. 7).

Put like this, the point seems to be that if there is a ‘fixed and stable’ phenomenon out there that our diagnostic criteria are trying to lock on to, then we ought to have incrementalism. If our diagnostic procedures are causally sensitive to the disorder itself then we will find ourselves converging on a way of identifying it.

- (2) The nosological box canyon. ‘According to this view the diagnostic categories of the old DSM approach are so flawed that they are beyond improvement by gradual iteration. Instead what is needed is a paradigm shift.’ (p. 9).

Notice, though, that the correlative notions of ‘flawed’ and ‘improvement’ here seem simply to presuppose a realist picture of disorders. If we don’t have some conception of a disorder as ‘the causal structure out there that we are trying to detect’, then in what sense might our procedures be said to be ‘flawed’, or ‘beyond improvement’? The very analogy of the ‘box canyon’ suggests there is some destination we would like to get to, from which we are barred by our having wound up in this dead end.

- (3) Wobbly iteration. ‘In Chang’s formulation of epistemic iteration, scientists propose some standard of evaluation or a criterion for improvement – such as more precise measurement – and iterate toward that goal. Wobbly iteration is what would occur when the criteria/validators shift.’ (p. 11).

Here Zachar seems to be appealing to his picture of shift of validators as a fundamentally non-empirical matter. So ‘wobbly iteration’ is when there are these empirically ungrounded shifts in criteria/validators. Why should this kind of thing be a barrier to successful [epistemic] iteration? Well, so far as I can see, only because the empirically ungrounded nature of the shifts themselves means that they cannot be represented as a matter of us managing to home in, better than we have previously, on the causal structure we are trying to identify.

None of this addresses the details of the dispute over Personality Disorders that Zachar describes. I have focused instead on two ideas in his discussion: the idea that the difference over validators is non-empirical, and correlatively, the idea that we should hold off from endorsing realism about psychiatric disorders. I think these ideas can seem tempting, but as I’ve said, the price of embracing them is the abandonment of a scientific psychiatry.

## REFERENCES

Ahn, Woo-Kyoung and Nancy S. Kim. 2008. 'Causal Theories of Mental Disorder Concepts'. *Psychological Science Agenda*, 22, 3-8.

Borsboom, Denny and Angélique O.J. Cramer. 2013. 'Network Analysis: An Integrative Approach to the Structure of Psychopathology', *Annual Review of Clinical Psychology*, 9, 91–121.

Bridgman, Percy Williams. 1927. *The Logic of Modern Physics*. (New York: Macmillan).

Carnap, Rudolf. 1950. 'Empiricism, Semantics, and Ontology'. *Revue Internationale de Philosophie* 4, 40–50.

Chang, Hasok. 2004. *Inventing Temperature: Measurement and Scientific Progress*. (New York: Oxford University Press).

Kendler, Kenneth S. 1990. 'Toward a Scientific Psychiatric Nosology'. *Archives of General Psychiatry*, 47, 969-973.

Kendler, Kenneth S. 2012. 'Epistemic Iteration as a Historical Model for Psychiatric Nosology: Promises and Limitations'. In Kenneth S. Kendler & Josef Parnas (eds.), *Philosophical issues in Psychiatry II: Nosology*. Oxford: Oxford University Press.

Kendler, Kenneth. S., Peter Zachar and Carl Craver. 2011. 'What Kinds of Things are Psychiatric Disorders?', *Psychological Medicine*, 41, 1143-1150.

Putnam, Hilary. 1975. 'Explanation and Reference'. In his *Mind, Language and Reality* (Cambridge: Cambridge University Press).

Quine, W.V.O. 1953. 'Two Dogmas of Empiricism'. In his *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press).

Robins, E., & Guze, S. B. 1970. "Establishment of Diagnostic Validity in Psychiatric Illness: Its Application to Schizophrenia", *American Journal of Psychiatry*, 126, 983-986.

Zachar, Peter. 'Epistemic Iteration or Paradigm Shift: The Case of Personality Disorder'. This volume.

Zachar, Peter, and Kenneth S. Kendler. 2007. 'Psychiatric Disorders: A Conceptual Taxonomy'. *American Journal of Psychiatry*, 164, 557-565.