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Versions of disorders we aspire to explain - nominal, conventional, and factual features

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Abstract

Work on causation in psychopathology often emphasizes variation in the causes but variation in what is to be explained further complicates matters. Focusing on the protean nature of psychopathology, this chapter explores different ways that classificatory variation is generated. For example, choices about what features of disorders to foreground and background can produce variation. The chapter also examines, from the perspective of scientific conventionalism, how classificatory decisions made at choice points partly constitute what is classified, but not in the sense of making it up. In contrast to the view that conventions are neither true nor false and thus isolated from the domain of facts, the chapter argues that scientific conventions are implemented to promote the discovery of facts. Scientific conventions must also answer to conceptual and factual constraints. The chapter concludes by looking at how classificatory choices can produce different versions of a psychiatric which may also result in variations in causal models across those versions. In agreement with ideas articulated by Putnam, the chapter argues that we cannot divide the language of psychopathology into a part that describes disorders as they are in themselves and a part that contains our conceptual contributions to what we know about disorders.

Key words: classification, vagueness, scientific conventionalism, operational definition, open concepts, John Locke, Nelson Goodman

Causal relationships

As a descriptive term, causation refers to a variety of relationships between before and after. For instance, the cause, - or 'before' part - can be producing, sustaining, or changing. Let me illustrate each option with respect to anxiety. Events experienced as highly traumatic can *produce* stress and anxiety. Continual and intrusive memories of the traumatic event can then *sustain* the anxiety for longer periods of time or *change* mildly disruptive anxiety into something more debilitating.

Other kinds of causal relationships that have been discussed include causation being irreversible or reversible and distal or proximal (Krieger, 2008; Ross & Woodward, 2022). The loss of a romantic relationship might precipitate a depressive episode, which can be *reversed*

with the formation of a new relationship. Some would say that for a depressive episode to be an actual psychiatric disorder, there should be a degree of *irreversibility* where the depression persists even if the person finds a replacement relationship. Lauren Ross and Kenneth Kendler (ch ?) explore the distal versus proximal distinction. An example of a proximal cause is ingesting a psychedelic drug leading to the experience of hallucinations. Childhood adversity, such as physical and sexual abuse, is considered a distal cause of various disorders in adulthood.

These differences largely emphasize variation in the causes, but causal relationships are made complex not only by variation in the before part, but variation in the after. For instance, with *multifinality* the same risk profile can have many different outcomes (Nolen-Hoeksema & Watkins, 2011).

In classification theory, non-essentialist approaches emphasize variation in the after, or variation in what we aspire to explain. Variation in what we want to explain will be the primary focus of this chapter.

In what follows, adopting a nominalist perspective, I will review some current ideas about the protean or changeable nature of psychopathology. I will then discuss the interplay of decisions and discovery in classification by focusing on classificatory choices as understood from the perspective of scientific conventionalism. More specifically, I will examine the importance of classificatory choice points that cannot be settled once and for all by reference to facts alone, but that also promote the discovery of facts. The resulting disorders that we aspire to explain are not privileged or inevitably destined to be discovered, but neither are they mere theoretical fictions. When I return to causation, I will suggest that implementing diagnostic conventions generates different versions of what we want to study, and some of those versions might not be causally interchangeable.

Multiple options for lumping and splitting classifications

In the classification of psychopathology, diagnostic criteria can be categorized as sensitive, specific, and non-specific. A sensitive criterion is one which indicates the presence of a disorder. A specific criterion is one that distinguishes the disorder from other disorders. A non-specific (transdiagnostic) criterion occurs across disorders. Forbes et al. (2023) show that about 63% of the criteria in the DSM are disorder-specific and about 37% are disorder non-specific. Interestingly Forbes et al. report that of the top fifteen most non-specific criteria in the DSM, ten of them (or 67%) are DSM depression criteria.

Looking at major depressive disorders, the following might be considered prototypical depression criteria.

- Little pleasure in doing things
- Feeling down or hopeless
- Feeling bad about yourself
- Recurrent suicidal ideation

At one time or another, the following have been considered sensitive to depression, but they are also non-specific for depression.

Anxiety

Headaches

Gastrointestinal distress

Depersonalization

In addition, we could list a number of auxiliary psychiatric criteria that could be incorporated into a depression symptom¹ network. These might include

Identity instability

Disturbances in the sense of self

Fear of going ‘crazy’

Ideas of reference

Thus, the population of potential diagnostic criteria depression is larger than the circumscribed list of nine criteria included in the DSM.

Van Os and Reininghaus (2016) discuss what they term the *extended psychosis phenotype*, by which they mean a collection of psychotic experiences that do not indicate the presence of a psychotic disorder. This implies that the domain of psychosis is broader than what is included in DSM criteria sets. By analogy, an *extended depression phenotype* could incorporate additional features beyond those listed in the DSM diagnostic criterion set.

In writing about emotions, Faucher and Tappolet (2008) distinguish an avocado-pear metaphor and a wax metaphor. In the U.S. avocado-pears are just called avocados. An avocado is a fruit with a hard pit at its center. Applied to psychiatric disorders, in the avocado metaphor, depressive disorder would possess a deep hard core that defines the stable nature of disorder, but individual differences can lead to slight variations in expression on the surface. For example, the pattern of depressive symptoms can vary across different precipitating stressful events (Keller et al., 2007).

Wax on the other hand is malleable all the way through. When warmed up it can take many different forms. It can cool down and maintain a specific shape, but also be re-warmed and morph again in larger and smaller ways. The wax metaphor suggests a more malleable depression concept. For instance, one can add and subtract symptoms to create different kinds of depression such as melancholia and post-partum depression, but also make the concept fluid enough to incorporate ‘masked depression,’ which includes inexplicable pain and impulsivity, but not sadness or anhedonia (Wender & Klein, 1981).

¹ To technically precise, symptoms are reported and signs are observed. This distinction is not always followed and I use symptoms loosely to refer to both.

One feature to which I would like to call attention is that in classifying and measuring, there is some *choice* regarding how we lump and split features. As suggested above, DSM descriptive definitions are partial representations of disorders – and in a hypothetical extended symptom network we can foreground and background different features. For example, with depression, we could choose to foreground anxiety and identity instability, which would result in an agitated, poor prognosis depression.

Let me also note that the population of potentially relevant diagnostic criteria should not be limited to those that happen to be classified in the DSM-ICD, which Berrios (2003) claims is largely based on the descriptive psychopathology of the late 19th century. Were the list of potential specific and non-specific criteria to become more extensive, the potential classificatory options available to select would expand. Furthermore, in addition to symptoms, other validating criteria such as past history, prognosis, risk factors and are also relevant considerations.

Nominalism and the protean nature of psychopathology

Previously, I have argued that a nominalist (versus an essentialist) view of emotion offers an informative analogy with psychopathology (Zachar, 2022) For instance, an emotion such as fear is constituted by characteristic precipitants, cognitions, behaviors, subjective feelings, and physiological responses. Basic emotion theorists contend that the patterning of cognitions, behaviors and feelings that define fear is the result of the activation of an affect program for fear (Ekman, 1992; Griffiths, 1997). In basic emotion theory, affect programs are the essences of emotions.

In contrast, according to the theory of psychological construction, there is no specialized affect program for fear (Barrett, 2006; Russell, 2003). Over time, different and varied patterns of cognitions, behaviors and feelings occur in our emotional lives. Adopting a nominalist view, psychological constructionists about emotion argue that occasionally a pattern occurs that resembles what a local language names fear, but many patterns occur that are not named.²

The nominalism of the psychological constructionists closely resembles the nominalism of John Locke. According to Locke, we do not have a specific name for the killing of an old man, but do for the killing of one's father, i.e., patricide. Locke claims that it would be impractical to specifically name every combination of features that can occur if we are not likely to use these names with some regularity. In addition, according to Locke, those combinations of features that by convention are assigned names *gain* some perceived privilege over those that are not.

Like the shifting components of emotions, different combinations of symptoms (and other validators) can ebb and flow throughout our lives in a protean or changeable manner (Caspi et al., 2020; Hartmann et al., 2021). Classification systems such as the DSM give some of them specific names – and perhaps the act of assigning specific names confers some perceived importance to those patterns. Many other patterns occur that are not named, but any actually

² One important difference between psychiatric disorders and emotions is that disorders are not typically transient – thus closer to moods than emotions.

occurring combination of features could be foregrounded and named if doing so was deemed useful.

Nominalism emphasizes both the view discussed in the last section that DSM definitions are partial representations of disorders and the view discussed in this section that many other symptom patterns occur that are not named. With that as a background framework, let us now turn to scientific conventionalism.

Psychiatric classification and scientific conventionalism

In the philosophy of science, conventionalism represents a rejection of a priori necessities, absolutes, and self-evident foundations. Scientific conventionalism holds that if what we discover is contingent on previous choice points, things can be otherwise – but not be any way we want them to be.

For example, in Euclidean geometry the three interior angles of a triangle add to 180 degrees – a logical necessity based on a postulate called the parallel postulate. Euclid proposed five postulates that were assumed to be true, but no one was ever able to prove the parallel postulate. In the 19th century geometers discovered that if they chose to assume certain postulates were false, they can work out alternative geometries. In one alternative geometry based on rejecting the parallel postulate, the interior angles of a triangle add to less than 180 degrees. In another alternative based on rejecting the second postulate, the interior angles add to more than 180 degrees. These alternative geometries are as deductively rigorous as Euclidean geometry.

A leading advocate for scientific conventionalism, Henri Poincaré (1905/2001), argued that some geometric axioms are conventions, not absolutely true or false, but more or less convenient for certain purposes. Such conventions are established by decisions and agreement at choice points.

Debates about alternative geometries ceased to be armchair exercises when Einstein used one of the alternatives to formulate the general theory of relativity. For various reasons, however, conventionalism fell out of favor in the philosophy of science.³ Nevertheless, it lives on in other guises such as the partial nature of operational definitions, the underdetermination of theory by evidence, the theory-ladenness of observation, and the role of non-empirical factors in classification. Different versions of conventionalism straddle the scientific realist-anti-realist divide in various ways. I am interested in scientific conventionalism because it can highlight the importance of practical decisions without viewing the outcomes of those decisions as theoretical fictions or constructions we just make up.

Poincaré claimed that scientific conventions are freely chosen, but he did not believe that they are merely arbitrary or random. Consider the dimensions of blood pressure and mood. There

³ One reason scientific conventionalism fell out of favor is some people construed conventions to be like analytic statements – true by definition alone not true because of the facts. Quine's argument against the analytic versus synthetic distinction showed that analytic statements were not forever isolated from factual considerations. This argument also undermined scientific conventionalism, so construed.

are clear differences between low blood pressure and a hypertensive crisis or between a euthymic mood and a melancholic depression. Between these categorically labeled endpoints, there is typically a region in which things are vague.

The classic example of vagueness in philosophy is the paradox of the heap. Sand scattered on the floor and a heap of sand can be viewed as distinct categories. If you slowly add one grain of sand to that on the floor, eventually you will end up with a heap of sand but there is no fixed point or threshold at which adding one more grain of sand transforms scattered sand into a heap. Thus, between the scattered sand and a heap there is a region for which there is no fact of the matter as to whether the sand forms a heap. These quantities of sand are called borderline cases.

There are also borderline cases for high blood pressure and disordered mood. For both, blood pressure and mood disorder, risk for negative consequences raises with increasing values. It is thus reasonable to target the setting of thresholds near or in regions that are transitional between normal and abnormal.

Let us look more specifically at major depressive disorder. In the DSM, a person has to meet 5 of 9 criteria to be diagnosed with depression. Consider two different approaches to setting diagnostic thresholds – cautious and bold. Cautious classifiers argue that thresholds should be set high to avoid false positives (Horwitz & Wakefield, 2012; Wakefield & First, 2003). Bolder classifiers argue that thresholds should be lowered to encourage early interventions when problems are milder- an approach known as the staging model (McGorry et al., 2006).

There is no absolute fact of the matter as to where the diagnostic threshold should be for all purposes but setting it to either minimize false positives or track a greater range of risk is not arbitrary. Once the relevant conventions are in place, false positive and false negative rates are factual matters. A diagnostic threshold, however, remains a classificatory decision or a convention. It does not establish the true measure of depression any more than Euclidean geometry establishes the true measure of space or than a meter establishes the true measure of length.

Situations which lack decisive facts of the matter about how to classify raise the question of the relationship between conventions and facts. As noted above, there is no absolute fact of the matter whether the diagnostic threshold for depression should be 4, 5, or 6. In thinking about this feature of conventions, the philosopher Leszek Kolakowski (1969) described scientific conventionalism as the destruction of the concept of fact.

The notion that scientific conventions are definitions that are radically isolated from a world of facts, however, begets misunderstanding about how some scientific conventions work. With that in mind, let us examine the convention-fact relationship.

Conventionalism and factual constraints

As noted, a commonly discussed feature of scientific conventions is that they are neither true nor false.⁴ The claim that conventions are neither true nor false does not require that classificatory choices be isolated from facts absolutely. Indeed, the empiricist philosophers such as Carnap and Reichenbach who advocated for some forms of scientific conventionalism argued that conventions (such as operational definitions) promote the discovery of facts by bridging theoretical constructs with empirical reality (van Loo & Romeijn, 2015). An easy-to-understand example of this is that if we want to discover the point prevalence of major depressive disorder in a population, we need to select a convention for distinguishing between depressed and not depressed (e.g., five of nine DSM criteria).

Particularly in domains such as psychopathology, when there is an abundance of facts, how the facts sort can be influenced by decisions made at choice points, but that does not mean the decisions make the facts. The resulting classifications are subject to constraints.

van Loo and Romeijn (2015) illustrate such factual constraints with respect to comorbidity. The DSM-III had diagnostic exclusions rules where a disorder lower in a diagnostic hierarchy (e.g., dysthymia) would not be diagnosed if it could be associated with a more pervasive disorder higher in the hierarchy (e.g., schizophrenia). In a sense what the DSM-III said was that non-specific symptoms of schizophrenia such as social withdrawal and concentration problems should not be used to make an additional diagnosis. The DSM-IV eliminated many exclusion rules leading to an explosion of comorbidity. Using the DSM-IV, a person who meets criteria for dysthymia and schizophrenia would be diagnosed with two disorders. Some thinkers claim this comorbidity is artificial/arbitrary, based primarily on classification choices.

Using a large epidemiological sample and brief operational definitions of depressed mood and anxiety, van Loo and Romeijn examined all possible combinations of symptoms, finding that in the largest combination, 33.8 % of the sample report no symptoms ever meeting clinically relevant duration criteria to be considered present. The most common combination of symptoms was 15.3% of the sample reporting all depressed mood and anxiety symptoms present. Furthermore, starting with two categories, depression and insomnia (D1) and anxiety and concentration problems (D2,) they were able to modify the comorbidity rates by altering symptom overlap. As shown in Table 1, by adding the overlapping symptom of insomnia to anxiety, they were able to increase depression-anxiety comorbidity from 15.5% to 20.8% of the cases.⁵ In this example comorbidity rates were partly dependent on classification choices.

 Insert Table 1: Depression-Anxiety Comorbidity Here

⁴ Some conventions are what Poincare called definitions disguised as descriptions, e. g, “depression occurs when someone has 5 or more DSM symptoms.” Other conventions e.g., “e.g., classifications should be etiologically based” are decisions contingent on normative judgements.

⁵ This measure of the rise in comorbidity was not reported in the article. Thank you to Jan-Willem Romeijn for calculating it for me.

In contrast, when looking at combinations of manic mood, compulsions, obsessions, and drug use, 83.2% of sample reported no symptoms ever meeting criteria to be considered present. The most common symptom profile was drug use alone reported by 9.7% of the sample. Many possible combinations were rare or non-existent. In this example D1 was mania and drug use, D2 was compulsions and obsessions. As seen in Table 2, unlike for the depression and anxiety case, adding the overlapping symptom of drug abuse to compulsions and obsessions did not raise comorbidity with mania because none of the new possible combinations actually occurred. In this example, comorbidity rates were dependent on empirical population characteristics.

 Insert Table 2: Mania and OCD Comorbidity Here

The point here is that if we sent out to explain comorbidity rates, those rates can vary across different classificatory decisions. van Loo and Romeijn claim that these rates, however, are not just artifacts of classificatory choices but neither are they independent of classification choices. We make classificatory decisions, but once they are in place, we do not then make further choices about what the facts are.

Conventions: Constituting what we measure but not making it up

Counting the number of people in a population who are depressed requires having a measure of depression. For instance, when epidemiologists agree to diagnose depression if a person meets at least five criteria on the *Patient Health Questionnaire-8* (PHQ-8) for two weeks or more, they are both establishing a way to measure depression and defining depression. van Loo and Romeijn (2015) refer to this as doing double duty. As a measurement device, the PHQ-8 *indexes* or detects the presence of depression. When the choices we make in defining disorders are written into how we measure them, the definitions also partly *constitute* what is measured. As stated by van Loo and Romeijn, what counts as a measure of depression and what depression is cannot be answered independently of each other.

What should we make of the claim that there is no absolute fact of the matter as to whether something like gastrointestinal distress should be included in our measure of depression, but when those choices are written into our measure, they partly constitute what the measure indexes?

Kenneth Kendler (2017) describes the constituting position as one that claims there is nothing to a disorder beyond the methods used to measure it. One problem with the constituting view, he notes, is that if the definitional criteria constitute the disorder, any change in the definition changes the nature of the disorder. In addition, if you meet the diagnostic criteria, you have the disorder and there is nothing more to it.

This view is reminiscent of Bridgman's (1927) early views on operational definitions. According to Bridgman, "the concept is synonymous with the corresponding set of operations" (p. 5) and "If we have more than one set of operations, we have more than once concept," and "strictly there should be a separate name to correspond to each different set of operations" (p. 10).

In contrast, if diagnostic criteria are fallible indicators of a disorder and only index its' likely presence as argued by Kendler, the criteria could be mistaken. Considered this way, false positive and false negative diagnoses make sense according to the indexical view but not to the constituting view.

I agree with Kendler's wariness of taking DSM definitions literally and treating them as fixed but suggest that in a scientific conventionalist framework, classificatory choices do not close off concepts. As Zachar, Schaffner, and Turkheimer (2020) claimed in their description of operational definitions as open concepts, such definitions are contingent upon classificatory choices and thus are revisable in the light of further factual considerations.

Considered as open concepts, the constituting function of scientific conventions is not limited to an initial anointing event that is then fixed. As Poincare held, conventions are not arbitrary but more or less convenient for certain purposes. Convenient means they have to work for those purposes. We can examine this further with some contrasts.

Thomas Edison invented the light bulb.

George Lucas made up light sabers, but he did not invent them.

To actually invent a working light saber is more involved than making one up. Making things up are not subject to the practical and empirical constraints of actual inventions. For example, a working light saber needs to visibly luminesce as red, green, or white. It needs to cut through objects and deflect laser beams. It needs to be balanced for use in battle. It needs a power source.

It is interesting to notice that Hasok Chang's (2004) book about the development of thermometers is not titled, *Inventing the Thermometer* but *Inventing Temperature*. In Chang's view, how we understand the nature of temperature is partly constituted by our thermometry. Chang states that one goal of thermometry was to devise a workable temperature scale that does not refer to the properties of any specific material substance. This was no small feat. As with light sabers, to say that such a measure of temperature was invented does not mean it was made up. Inventions have to work and are subject to empirical and practical constraints.

One can also use some engineering terminology and speak of measures being *designed*. To become a successful invention, designs have to be implemented, tested out, and iterated over time. During iteration, decisions made at choice points can make a difference in what is measured. Let me give two brief examples.

First, when the *Minnesota Multiphasic Personality Inventory* (MMPI) depression scale was originally designed, items were placed on the scale if they differentiated between a group of visitors to the University of Minnesota Hospitals who were not being treated for any illness and

people in the depressive phase of a manic-depressive illness (Hathaway & McKinley, 1940). As suggested by Tellegen et al (2003), one result of this approach was that the resulting depression scale included depression-specific items but was also boosted by non-specific markers of general psychiatric distress such as ‘sometimes feeling useless’ and ‘not being as content as others.’ Many clinicians value this scale for being a sensitive indicator of psychiatric distress that has a depressive dimension. MMPI depression was not made up but what the scale indexed and modeled was influenced by design choices.

My second example is highly speculative. In psychological testing, some test developers have decided that a symptom such crying nearly day is not an adequate depression scale item because it is related to indicators of depression for women but not men - and may bias the prevalence rates (Morey, 2003). When psychologists construct tests, they try to assure that the test has desirable psychometric properties such as high internal consistency and temporal stability. They do so with multiple iteration over which they select items that support these features and delete those that do not. In theory, test developers could set out to construct a depression scale that had the feature of being gender-neutral. Indeed, such a goal has some precedent in vocational assessment, work on the validity of ADHD, and the development a scale to measure prolonged grief disorder (Diamond, 1975; Morse et al., 2024; Prigerson et al., 2009). I am not saying this is a good idea or that it could developed, but let’s assume that after successive iterations the attempt is successful.

The gender-neutral standard is clearly a convention. All the same, if such a measure was developed, we might even say that gender-neutral depression was out there before we measured it. We did not make it up like someone made up conventions for ‘real bourbon,’ i.e., real bourbon must have at least 51% corn in the mash and be aged at least 2 years in new charred oak barrels. There was no ‘real bourbon’ in the world until this recipe was implemented. We probably wouldn’t say that no one manifested the gender-neutral depression pattern until the first person took the test, even though gender-neutral depression notion has strong conventional elements.

From a more essentialist perspective, ‘true depression’ is predefined by the world and all our measures should seek to represent that phenomenon. Nominalism allows for alternatives based on the idea that the world contains more than any measure or construct can represent. The differences between those who hold these perspectives can be seen in their likely reactions to Fried (2020) showing that different measures of depression (BDI, HAM-D etc.) are not simply interchangeable indices of the same thing. An essentialist might say one or more of these measures are flawed indicators. A nominalist would be more comfortable with variation across measures as long as it is recognized. Rather than seeking to bootstrap our way to the correct description and true measure of depression, nominalists would allow that some of the choices we make along the way become a part of what we measure and what is indexed.

Bringing it back to causal relationships: Explaining versions

One of most popular comic book characters ever created, The Batman, has been depicted with many different, yet overlapping versions over the years. There is the heroic Batman of the

1950s who has crime fighting adventures, the campy Batman of the 1960s television show, Batman the detective, Batman the dark knight, obsessed and angry, and so on.

Consider also the many possible versions of a song such as Amazing Grace, including standard gospel versions, jazz versions, country versions, alt-rock versions, hip-hop versions, and reggae versions. I can readily imagine hearing a new and creative version of Amazing Grace and thinking something like: “it is the same song but completely different!”

Complex psychological states are also version-rich. For instance, there are many versions of love and friendship. Queen Elizabeth and Prince Phillip or Atticus Finch and Scout are different versions of love. Matt Damon and Ben Affleck or Walter Sobchak and The Dude are different versions of friendship.

According to Nelson Goodman (1978), it makes no sense to talk about a world outside of any version - we only have access to versions. He goes so far as to say that versions make our worlds. Goodman does not use the term constituting, but he does talk about versions as designing worlds, i.e., differences between versions are partly dependent on choices. Goodman does not want to say that versions can be true by means of corresponding to the actual world because that claim would seem to posit a neutral something (undescribed, undepicted, unperceived) that a version is true of.⁶ This harkens back to the claim we saw above from van Loo and Romeijn that what counts as a measure of depression and what depression is cannot be answered independently of each other. Putnam (1996) also puts it nicely:

Our language cannot be divided up into two parts, a part that describes the world “as it is anyway” and a part that describes our conceptual contribution (p. 190).

Goodman does, however, say that versions can be right or wrong. Right versions are consistent with what else we know and they pay their way in William James’ sense – they are useful by helping us deepen knowledge, connect things up, see new things, increase what we can control, and so on. From this perspective, the neurodevelopmental disorder version of autism is a ‘right’ version, the version of autism as a reaction to cold parenting is not.

Goodman could be seen as taking it a too far by talking about *worldmaking*, particularly given recent developments in American culture. Social media has made it easier for people to be exposed primarily to claims about the way things are that correspond to how they want things to be. This has not revealed anything new about human psychology. Work in cognitive and social psychology on cognitive mechanisms such as the frequency bias and motivated reasoning makes this recent development unsurprising but disappointing, nevertheless.

This unfortunate state of affairs has focused philosophical attention on the notion that, often, the world is not the way we want it to be or think it should be. We cannot just make it up – but notions such as *worldmaking* and *right versions making worlds* seem to go beyond the deflationary claim that there is no master version of the world to which all accurate versions can be reduced. Because Goodman believes that true versus false and actual versus fictional are

⁶ Goodman accepts that statements such as ‘Montgomery is the capital city of Alabama’ can be true or false but in his view van Gogh’s starry night painting is neither true nor false.

important distinctions, he likely does not take it too far, but emphasizing worldmaking makes that hard to see.

Batman and *Amazing Grace* are made up – you can generate as many versions of them as you can imagine. Movie versions of depression such as Conrad in *Ordinary People*, musical versions of depression such as *Like the Weather* by 10,000 Maniacs, and pictorial versions of depression such as *Melancholy* by Edvard Munch are made up, but they are also made up out of other versions that are not fictional.

Because the DSM and ICD operational definitions are partial representations of depression, they are also versions. As noted above, one can generate different versions of depression depending on what features one chooses to foreground and background by convention. For example, a more melancholic version of depression would foreground an incapacity to experience pleasure and a despondency that is qualitatively distinct from depressed mood. A version of depression that is akin to general psychiatric distress would foreground non-specific depression symptoms. To be an actual version of depression, however, a pattern has to occur and cannot be made up. To be useful, a pattern should also occur with high enough frequencies to support generalizations.

With respect to classification, the view that classifications should be etiologically based is a convention. Up until this point in the chapter, I have portrayed versions as descriptively distinct – focusing on the descriptive or definitional features of measurement. Lurking in the background was the possibility that two descriptively distinct versions such as MMPI depression and gender-neutral depression are produced by different causal scenarios. Indeed, another possible way to demarcate versions would be to say that two versions of psychopathology are different if they require somewhat different causal explanations (even if they are descriptively similar). That is also assuming that different causes would require different interventions.

The causal explanation of psychopathology is complicated by how many different versions of what we attempt to study can be articulated. There are many reasons for there being a plurality of versions of psychopathology. The reason emphasized in this chapter is that in developing ways to measure psychopathology, conventions introduced at diagnostic choice points can generate different versions. This is consistent with the nominalist idea that the object eludes the concept, thus concepts cannot be more than versions of objects. The multiplicity of versions is another reason why diagnostic literalism, the reification of concepts and seeking privileged classification are problematic. Being contingent on a convention, however, does not mean what is discovered is made up.

Conclusions

Human behavior is causally complex, involving events at multiple levels of analysis and that span across different spatial and temporal scales in a way that is hard to integrate. Unlike remote galaxies and hard to imagine subatomic particles, human behavior is familiar and accessible to us and it seems it should be easier to get a hold on, but it has not been. Rather than conceptualizing the study of human behavior as an immature science in comparison to physics

and chemistry, however, it may be more accurate to see the study of human behavior as the more difficult science.

In the past I have argued against essentialist approaches to psychopathology, largely emphasizing the significant variation that is there (Aftab, 2020; Zachar, 2014). An equally strong non-essentialism about diagnostic categories can be gleaned in Hyman's (2021) intimation that some categories could be considered chance conjunctions of symptoms that can dissipate and reassort over a lifetime (i.e., consistent with the wax metaphor mentioned above).

I do not know if there is a continuum of psychopathology with more random events on one end and determined events on the other, but if so, complex psychopathology (and psychology) would probably fall somewhere in between. Some versions might even be borderline cases where it is hard to parse out chance and causation, but others will be marginally closer to the determined end and others marginally closer to the random end. If understanding causality is a priority, we should focus on defining versions that are closer to the determined end. But if causality is the only priority, then the versions we include in our etiologic-based classifications may also under-represent the psychopathology that is there.

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