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What is wrong with the *DSM*?

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The DSM is the main classification of mental disorders used by psychiatrists in the United States and, increasingly, around the world. Although widely used, the DSM has come in for fierce criticism, with many commentators believing it to be conceptually flawed in a variety of ways. This paper assesses some of these philosophical worries. The first half of the paper asks whether the project of constructing a classification of mental disorders that ‘cuts nature at the joints’ makes sense. What is mental disorder? Are types of mental disorder natural kinds (that is, are the distinctions between them objective and of fundamental theoretical importance, as are, say, the distinctions between the chemical elements)? The second half of the paper addresses epistemic worries. Even if types of mental disorder are natural kinds there may be reason to doubt that the DSM will come to reflect their natural structure. In particular, I examine the extent to which the DSM is theory-laden, and look at how it has been shaped by social and financial factors. Ultimately, I conclude that although the DSM is of immense practical importance it is not likely to become the best possible classification of mental disorders.

Keywords: *atheoretical; classification; Diagnostic and Statistical Manual of Mental Disorders; diagnosis; disease; DSM; insurance natural kinds*

The *Diagnostic and Statistical Manual of Mental Disorders* (known as the *DSM*) is published by the American Psychiatric Association (APA), and is the main classification of mental disorders used by psychiatrists in the United States and, increasingly, around the world. Although widely used, the *DSM* has come in for fierce criticism, with many commentators believing it to be conceptually flawed in a variety of ways. This paper assesses some of these

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philosophical worries. The first half of the paper asks whether the project of constructing a classification of mental disorders that 'cuts nature at the joints' makes sense. What is mental disorder? Are types of mental disorder natural kinds (that is are the distinctions between them objective and of fundamental theoretical importance, as are, say, the distinctions between the chemical elements)? The second half of the paper addresses epistemic worries. Even if types of mental disorder are natural kinds, there may be reason to doubt that the *DSM* will come to reflect their natural structure. In particular, I examine the extent to which the *DSM* is theory-laden, and look at how it has been shaped by social and financial factors. Ultimately, I conclude that although the *DSM* is of immense practical importance, it is not likely to become the best possible classification of mental disorders.

1 WHAT IS MENTAL DISORDER?

The introduction to the *DSM-III* (APA, 1980: 6) includes the following definition of mental disorder:

... each of the mental disorders is conceptualized as a clinically significant behavioural or psychological syndrome or pattern that occurs in an individual and that is typically associated with either a painful symptom (distress) or impairment in one or more areas of functioning (disability). In addition there is an inference that there is a behavioral, psychological, or biological dysfunction, and that the disturbance is not only in the relationship between the individual and society.

With minor revisions this claim is also made in the *DSM-III-R* (APA, 1987), the *DSM-IV* (APA, 1994) and the *DSM-IV-TR* (APA, 2000). Here, I shall examine whether this definition is conceptually adequate.

The *DSM* definition of mental disorder was born out of the 1970s debates over homosexuality. In the *DSM-II* (APA, 1968), until 1973 homosexuality was listed as a mental disorder, a fact that aroused great hostility from the gay community.¹ In 1970 gay rights protesters mobbed the APA annual meeting in San Francisco, shouting down speakers with whom they disagreed and disrupting much of the meeting. Protests continued throughout 1971 and 1972. Robert Spitzer, who would later become chairman of the *DSM-III* committee, became involved in the debates and found defining 'disorder' to be a useful way of defending his stance on homosexuality (Spitzer, 1973, 1981). Spitzer suggested that homosexuality per se is not a disorder but that a diagnosis should be included for homosexuals who experience distress concerning their sexual orientation. Such a proposal was politically useful because it found some middle ground between those who considered homosexuality to be a mental disorder and those who considered it a normal variant of human sexuality. To defend his stance, Spitzer formulated a definition of mental disorder that he claimed was satisfied by all the conditions in the

DSM-II with the exception of homosexuality. According to Spitzer's definition a condition can only be a mental disorder if it causes distress or disability. As many homosexuals experience no distress or disability, homosexuality in and of itself cannot be a disorder. However, those people who are distressed about their sexual orientation *can* be considered to suffer from a disorder and are appropriately treated by psychiatrists. Spitzer's position came to be adopted by the APA in 1973, when homosexuality was removed as a diagnosis from the *DSM-II* and 'Sexual Orientation Disorder', a diagnosis for homosexuals who are unhappy about being gay, was added.

In due course, a version of Spitzer's definition came to be included in the *DSM-III*. In a series of articles, Jerome Wakefield has convincingly argued that the core idea behind the *DSM* definition is that diseases are harmful dysfunctions, where 'dysfunction' is to be understood in an evolutionary sense (Wakefield, 1992*a*, 1992*b*, 1993). (In the literature concerning the definition of mental disorder, 'disease' and 'disorder' are used interchangeably.) According to this account, being an evolutionary dysfunction is necessary but insufficient for a condition to be a disease. For homosexuality to be a disorder it would have to be *both* an evolutionary dysfunction and also harmful.

Here, however, I shall argue that the claim that diseases are harmful dysfunctions cannot be accepted because evolutionary dysfunction is not even necessary for disease. The problem with the *DSM* definition is that the genetic bases of some conditions that we would normally class as diseases may confer an evolutionary advantage. Evolutionary psychologists have been struck by the fact that many mental diseases appear to have a genetic basis and yet occur at prevalence rates that are too high to be solely the result of mutations. Examples include manic-depression, sociopathy, obsessive-compulsivity, anxiety, drug abuse and some personality disorders (Wilson, 1993: 45 in reprint). This means that the genetic bases of these mental diseases must be promoted by natural selection, which implies that the genes are adaptive in some way or other. In such cases, from an evolutionary point of view, there may be no dysfunction when cases of the disease occur. The claim that a disease is a harmful dysfunction must be rejected, and a new account of disease is required.

I suggest that a tidy definition of 'disease' cannot be achieved. By 'disease' we aim to pick out a variety of conditions that through being painful, disfiguring or disabling are of interest to us as people. This class of conditions is by its nature anthropocentric and corresponds to no natural class of conditions in the world. I suggest that by disease we mean a condition that it is a bad thing to have, that is such that we consider the afflicted person to be unlucky, and that can potentially be medically treated. All three criteria must be fulfilled for a condition to be a disease. The criterion that for a condition to be a disease it must be a bad thing is required to distinguish the biologically different from the diseased. The claim that the

sufferer must be unlucky is needed to distinguish diseases from conditions that are unpleasant but normal, for example teething. Finally, the claim that for a condition to be a disease it must be potentially medically treatable is needed to distinguish diseases from other types of misfortune, for example economic problems and legal problems.²

If the definition of disease used by the *DSM* must be rejected, what implications does this have for the *DSM*? Does it imply that the *DSM* includes the wrong class of conditions? I suggest that the implications for the *DSM* are limited. There is reason to doubt the extent to which decisions to include particular conditions in the *DSM* were influenced by accounts of disease. Letters in the APA archives do not argue that conditions should be included because they are diseases or excluded because they are not diseases. Rather, correspondents argue that a diagnosis should be included because psychiatrists see patients with the condition, or that the condition is required for insurance purposes, or that research on the condition is being carried out. This suggests that accounts of disease may have been little used in deciding which conditions to include in the *DSM*. During the 1970s and 1980s, in public the APA found defining disease a useful rhetorical strategy. Defining disease was useful both in the debates over homosexuality and in attempts to defend psychiatry against anti-psychiatric claims that psychiatrists merely treat problems in living. However, this public interest is compatible with APA committees paying little attention to accounts of disease behind closed doors.

Recently it seems that the APA has begun to lose interest in defining disease even for rhetorical effect. Concerns regarding homosexuality and anti-psychiatry were peculiar to a specific time in American history, and have now largely vanished. Right on cue the APA has started to lose interest in defining disease. The *DSM-IV* and the *DSM-IV-TR* define disorder but comment that 'no definition adequately specifies precise boundaries for the concept "mental disorder"' and admit that 'the definition of *mental disorder* [italics in original] that was included in *DSM-III* and *DSM-III-R* is presented here because it is as useful as any other available definition' (APA, 1994: xxi). These comments scarcely give the impression that defining disorder was considered of much importance by the committees responsible for the most recent editions of the *DSM*.

If I am right and the APA is losing interest in defining disease, then I suggest that this is a pity, for two major reasons. First, a good account of disease can be helpful in determining which conditions should be considered to be diseases. Hypomania is an example of a condition that has plausibly been wrongly listed in the *DSM* as a disorder (according to both the *DSM* account of disorder and my own). Hypomanic episodes are characterized by a mood that is 'unusually good, cheerful, or high . . . The expansive quality of the mood disturbance is characterized by enthusiasm for social, interpersonal, or occupational interactions.' (APA, 1994: 336) The person may have a decreased need for sleep and be more talkative than normal.

Hypomanic episodes are distinguished from manic episodes in that there is no, or little, impairment in the person's social or occupational functioning, and there are no psychotic features. Quite simply a hypomanic episode is generally a great thing to experience. Many psychiatrists believe that it is important to record hypomanic episodes because if a depressed person has been hypomanic in the past then this can have implications for their treatment. I have no quarrel with such claims. However, I suggest that hypomania in and of itself should not be considered to be a *disease* because it is not a bad thing to have.

Second, developing an account of disease is important because it is relevant to the discussion of various social and political issues. Take, for example, the question of who should determine whether a condition is a disease. Depending on the account of disease adopted, different answers to this question will seem attractive. If whether a condition is a disease is thought to depend on biological facts, then it will seem appropriate for experts in biology to determine which conditions are diseases. In contrast, I have argued that whether a condition is a disease is in part a value-judgement. As physicians are not experts in making value-judgements, it follows from my account that it is not appropriate for them alone to decide which conditions are diseases. Similarly, an account of disease will be of use in determining whether, and why, diseased people should be eligible for various benefits, or excused from wrongdoing, although exploring such issues is beyond the scope of this paper.

2 ARE TYPES OF MENTAL DISORDER NATURAL KINDS?

Whether a condition is a disorder is partly a value-judgement, but the distinctions between types of mental disorder might still depend solely on psychological and biological facts. If this were the case, then the domain of mental disorders would be analogous to the domain of weeds. Weeds are unwanted plants, so whether a daisy is a weed is at least in part a value judgement. Still the distinctions between kinds of plants generally considered weeds, such as those between dandelions and thistles, are fixed by the nature of the world. A fundamental assumption of the *DSM* project is that empirical research can tell us how mental disorders ought to be classified. When the APA committees developed the *DSM-IV* they reviewed thousands of empirical studies.³ These studies examined matters such as the biochemical correlates of disorders, how people with different disorders respond to particular treatments, and whether a particular disorder disproportionately affects people of a certain age or sex. The assumption is that by examining all these data it will be possible to construct a classification system that at least approximately reflects the true natural similarities and differences between cases of mental illness.

The similarities and differences between types of mental disease are

assumed to be not only objective but also of great significance to psychiatric theory. This is why psychiatric research generally examines groups of patients with the same diagnosis; these patients are assumed to be similar in some fundamental way. It is supposed that fundamentally different pathological processes underlie different disorders, and that different disorders can best be treated in different ways.

Thus the APA can be seen as aiming to produce a classification system very much like those found in biology or chemistry. Like the differences between the chemical elements and biological species, the differences between types of mental disorder are thought to be objective and theoretically important. In short, mental disorders are assumed to be 'natural kinds'. 'Natural kind' is a technical term used by philosophers to refer to the kinds of thing or stuff studied by the natural sciences. Sodium, fleas, dandelions and electrons are all examples of natural kinds. Members of a natural kind are thought to be naturally similar to each other because they are alike at a fundamental level. Fleas, for example, are all similar in that they jump, drink blood and are poisoned by flea-spray, and fleas are alike in these respects because they are similar in some more fundamental way: they are all genetically similar.

2.1 Arguments against mental disorders being natural kinds

In this section I examine whether types of mental disorder are plausibly natural kinds. First, I refute arguments claiming to show that types of mental disorder cannot be natural kinds. Second, I consider whether some kinds of mental disorder actually are natural kinds. Two arguments are commonly put forward for the claim that types of mental disorder cannot be natural kinds. Here I refute them both.

The historical argument

Some recent work in the history of medicine has aimed to show how disease entities have been constructed via the interaction of various technologies, institutions and social interests. To take an example, in *The Harmony of Illusions* Allan Young (1995) claims to show how Post-Traumatic Stress Disorder arose out of the interaction of lobbying by Vietnam veterans, and the various tests and treatment programmes which arose for diagnosing and treating the disorder. Young and the authors of other such case-studies argue that as a disease entity has been artificially manufactured it cannot be a natural kind.⁴

There are two possible ways of replying to such arguments and the appropriate response varies from case to case. First, one can agree that the disease has been created but argue that natural kinds can be artificial in this sense. The key to seeing that natural kinds can be artificially created is to remember that the 'natural' in 'natural kind' should be read as in 'natural law' rather than as in 'present in the Garden of Eden'. Plutonium is an

example of a manufactured natural kind, and doubtless some highly 'social' story could be told concerning its creation. Artificially produced diseases, for example, types of drug addiction and, arguably, Post-Traumatic Stress Disorder could similarly be both artificially manufactured and natural kinds.

Second, one can insist that the disease has always existed and that it is only the means of *recognizing* it that have been invented. This is a plausible response when retrospectively the disease can be seen to have afflicted people throughout history. For example, R. Daly (1983) has claimed that the mental symptoms recorded in Samuel Pepys' diary indicate that he suffered from Post Traumatic Stress Disorder after having witnessed the Great Fire of London. Given such evidence it can plausibly be claimed that people have always suffered from Post Traumatic Stress Disorder. If this is the case, Young's claim to have documented the social factors that led to the construction of Post Traumatic Stress Disorder is false. All Young's book actually shows is how it is that a pre-existing condition came to be recognized and diagnosed.⁵ As a consequence, historical arguments such as Young's fail to demonstrate that a disease is not a natural kind.

Hacking's argument

In a series of papers, Ian Hacking (1986, 1988, 1992, 1995a, 1995b) developed an argument purporting to show that types of mental disorder cannot be natural kinds. More recently Hacking seems to have changed his mind and, although he never gives reasons for rejecting his old argument, a chapter of his *The Social Construction of What?* (2000) discusses the possibility that at least some mental disorders might be natural kinds. Here I am concerned only with Hacking's earlier work in which he argues that classifying and describing kinds of people (he refers to these as 'human kinds') results in feedback that alters the very kinds under study. People care how they are classified. As a result they are motivated to attempt to alter the ways in which they are classified and, as their behaviour changes, so do the phenomena under investigation. This feedback results in human kinds having histories totally unlike the histories of natural kinds, leading Hacking to conclude that human kinds are not natural kinds. Here I will argue that Hacking's argument fails and that he has not shown that types of mental disorder cannot be natural kinds.

I accept that feedback occurs when human kinds are classified, as Hacking describes. However, our classificatory practices also result in feedback that alters some natural kinds.⁶ For example, the characteristics of domestic livestock change over time because particular animals are classified as being the 'Best in Show' and are used in selective breeding. The occurrence of feedback does not distinguish human kinds from natural kinds.

When faced with such examples, Hacking (1997: 15) claims that the feedback that affects human kinds is importantly different because it occurs as a result of subjects becoming *aware* of the ways in which they are being

described and judged. This idea needs working on before it can become an argument that human kinds cannot be natural kinds. Why does Hacking think that feedback that occurs as a result of subjects' ideas is metaphysically significant in a way that feedback caused by, say, selective breeding is not?

Idea-dependence might be thought to matter because it betrays the subjective nature of a kind. The argument then would be that while natural kinds are objective, human kinds are affected by ideas and so subjective, and that thus human kinds cannot be natural kinds. Hacking gives no indication that this is a route he would wish to go down; however, it is the most obvious option for someone who wishes to claim that idea-dependence is metaphysically significant and so worth pursuing here.

Entities can be idea-dependent in fundamentally different senses, however, not all of which are indicative of subjectivity. Beauty, they say, is in the eye of the beholder. Indeed, it is easy to imagine that a woman wearing ornate false eyelashes may appear beautiful one year and absurd the next, depending on the whims of fashion. In this case the 'change', from beautiful to absurd, is a relational change only – it is brought about by a change in the viewer rather than by a change in the woman. Such idea-dependence indeed shows that a kind, such as beautiful women, is subjective and thus cannot be a natural kind.

However, there is also another sort of idea-dependence. Imagine that the woman starts wearing false eyelashes because she reads that they are this season's essential accessory. Here ideas regarding beauty have affected her. They are part of the causal story that culminates in her wearing false eyelashes. However, the resulting change is perfectly objective. She used not to wear false eyelashes, now she does. Idea-dependence of this causal type results in objective changes in entities and is compatible with a kind, such as women who wear false eyelashes, being objective. Indeed, there might be interesting laws concerning such kinds, for example, conceivably, 'Women who wear false eyelashes are more likely to get eye infections.'

Hacking has shown that human kinds are affected by ideas. In order to show that human kinds are subjective he also needs to show that human kinds are affected by ideas in a way that results in relational as opposed to genuine changes. Hacking's examples, however, appear to be of cases where ideas produce genuine changes in peoples' behaviour. His most detailed case study concerns Multiple Personality Disorder (Hacking, 1995*a*). The first patients diagnosed with Multiple Personality Disorder tended to have only two or three different personalities. Over time, however, more and more flamboyant cases started to appear on American chat shows; these patients had scores of different personalities, personalities of the opposite sex, and animal personalities. Hacking argues that as a result more and more patients started presenting with similar symptoms. Popular ideas about Multiple Personality Disorder affected the symptoms typical of patients. Nonetheless, it seems that the changes in patients' symptoms were genuine; patients really did start barking. In order to show that the changes in the symptoms of

Multiple Personality Disorder indicate that it is a subjective kind and so not a natural kind Hacking would need to show that the changes in symptoms were not genuine, and he makes no suggestion that this is the case. Thus, I conclude that Hacking has failed to show that types of mental disorder cannot be natural kinds. Having refuted common arguments that purport to show that mental diseases *cannot* be natural kinds it is now time to move on and consider whether it is likely that at least some mental diseases *actually are* natural kinds.

2.2 Are types of mental disorder natural kinds?

So far the examples of natural kinds considered – biological species, chemical elements, types of fundamental particle – have all been types of thing or stuff. Types of mental disorder should not be thought of in this way but should instead be thought of as types of process. Accounts of natural kinds can be readily adapted to deal with natural kinds of process. One can say that instances of a natural kind of process will all be fundamentally similar to each other *so long as they are at the same stage*. In addition to types of disease, natural kinds of process might include particular chemical reactions, for example rusting, and biological processes, for example the metamorphosis of some particular species of caterpillar into a butterfly.

In order for types of mental disease to be natural kinds it must be the case that instances of the disease are all similar to each other in some fundamental sense. Unfortunately many mental disorders are insufficiently well understood for it to be possible to know whether or not this criterion is met. Plausibly, however, there are at least some mental disorders that meet this condition. Take Huntington's Chorea, for example. Huntington's Chorea is caused by a single dominant gene on chromosome four. Symptoms generally appear in middle age and include jerky involuntary movements, behavioural changes and progressive dementia. Plausibly, Huntington's Chorea is a natural kind of mental disorder; in all cases an identical underlying property, the defective gene, produces characteristic symptoms.

In addition to some mental diseases being natural kinds, it is likely that some will be, what I shall call, 'partial kinds'. Diseases will be partial kinds when cases of the disease are similar to each other in some, but not all, fundamental respects. To take a fairly well-understood physical disease as an example, cases of meningitis caused by different viruses have different causes although the remainder of the disease process is very similar. When a disease forms a partial kind, cases of the disease will be similar to each other in many, but not all, respects.

I suggest that some types of mental disorder will turn out to be natural kinds, and that others will turn out to be partial kinds. In addition there will almost certainly be some categories of mental disorder that are neither, that is, mental disorders where cases do not possess any similar fundamental properties at all. Most obviously 'rag-bag' diagnoses included in the *DSM*,

such as 'Sexual Disorder Not Otherwise Specified' will fall into this category, as there is no reason to think that cases that receive such a diagnosis will be similar to each other in any interesting way. In addition, future research may well find that cases receiving other more 'respectable' diagnoses actually have nothing interesting in common. Some researchers hold that this is likely to be the case with schizophrenia, for example.⁷

To conclude this section, I have refuted arguments that purport to show that mental disorders cannot be natural kinds, and I have suggested that at least some types of mental disorder actually are natural kinds. Thus, it makes sense for psychiatrists to try to discover what these natural kinds are through empirical research. The next two sections consider whether it is likely that a future edition of the *DSM* will contain categories that map onto these natural kinds. Two potential sources of difficulty will be considered. These arise from the possibility that observation in psychiatry is theory-laden, and from the fact that the *DSM* is affected by pressures that emerge from the various ways in which it is used in practice.

3 THE PROBLEM OF THEORY-LADENNESS

If observation is theory-laden, then this might give us reason to doubt that the categories included in the *DSM* will correspond to natural kinds of disorder. There is a danger that much current psychiatric theory is wrong. If so, and if observation in psychiatry is theory-laden, then psychiatrists' false beliefs could be expected to distort their observations of their patients and prevent them from seeing the true similarities and differences between types of mental disorder.

Three distinct claims can be teased out from the general thought that observation is in some sense theory-laden. First, perception itself might be theory-laden, where 'perception' here refers to the immediate awareness an organism has of the world. Second, the language in which observation reports are couched might be theory-laden. As a result, scientists holding different theories, although perceiving the same thing, would produce observation statements that differ in meaning. Third, where scientists choose to direct their attention might be determined by their theoretical beliefs about what is important. Scientists with different theories would seek out different stimuli and so make different observations. I will examine each of these three claims in turn.

3.1 Is perception theory-laden?

There are experiments that indicate that what a subject perceives is affected by their expectations under certain conditions. Bruner and Postman's (1949) anomalous playing card experiment and Stratton's (1897) inverting lenses experiment are the classic examples. However, there are problems with extrapolating from these experiments to claim that perception in psychiatry is theory-laden.

Clinical case studies of people who have suffered brain lesions suggest that different types of perception are dependent on different areas of the brain. One of the most discussed cases concerns a patient, D.F., who suffered brain damage as a result of carbon monoxide poisoning (Milner, 1997). D.F. is unable to recognize objects, places or people, as she is unable to discriminate size, shape or orientation. However, D.F. can still perform actions that require perceptual information. For example, although incapable of stating the orientation of a letter-box type slot, D.F. can reach to insert her hand through the slot with her hand correctly positioned. She can make use of perceptual information to guide her actions although being unable to recognize objects. Conversely, other brain-damaged patients have problems grasping objects but are able to recognize them (Milner and Goodale, 1995: ch. 4). This suggests that perception-for-recognition and perception-for-action are processed in different parts of the brain.

Some experiments suggest that perception-for-action does not employ top-down processing. When subjects reach for the central line of a Müller-Lyer figure their grasp aperture corresponds to the actual rather than to the perceived size of the line (Otto-de Haart, Carey and Milne, 1999; Post and Welch, 1996). Similar results have been gained with Titchener circles – the illusion in which the apparent size of a central circle depends on the size of the circles surrounding it (Aglioti, de Souza and Goodale, 1995; Haffenden and Goodale, 1998). These illusions are generally thought to occur as a side effect of top-down processing. As perception-for-action is not vulnerable to the illusions, this suggests that perception-for-action does not employ top-down processing and thus will not be theory-laden.

It is also likely that face recognition (e.g., recognizing Tony Blair) depends on a distinct perception-processing system. Evidence for this hypothesis comes mainly from the clinical condition of prosopagnosia, in which patients cannot recognize faces but can recognize other stimuli. There are also cases of brain-damaged people who can recognize faces but are unable to recognize facial expressions (Young, Newcombe, de Haan *et al.*, 1993). These people can recognize a photo as being of Tony Blair, but they are unable to tell whether he is happy, angry or bored. This suggests that the perceptual-system that recognizes facial expressions is different from that which recognizes faces.

That different types of perception are processed differently in different areas of the brain means that it is possible that some types of perception are theory-laden while other types are not theory-laden. For the most part, the descriptions of conditions included in the *DSM* are based on psychiatrists' observations of psychiatric patients. Thus we must ask whether perceptions of people are affected by theoretical beliefs.

Psychological studies examining this question are scarce. However, some studies have examined the effects of contextual information on subjects' perceptions of emotions. Psychiatrists have various theoretical beliefs

concerning the emotions that are likely to be manifested by different types of psychiatric patients, and so these studies are of interest here. In the studies, subjects are shown a face, for example a woman crying, and are given information regarding the context, for example they might be told that the woman has been given a present, and then they are asked to judge the emotion that the person is probably experiencing. Unfortunately the results of experiments have been inconsistent. Fernández-Dols and Carroll (1997) reviewed eighteen studies. Seven of these found that subjects' perceptions were unaffected by the contextual information, while the others found that the contextual information had some effect. No overall conclusion can be drawn from the series. The final conclusion of this subsection is that whether our perceptions are theory-laden is not, as is often assumed, a closed question, but on the contrary should continue to be a live issue.

3.2 Are observation reports necessarily theory-laden?

Those philosophers who claim that language is necessarily theory-laden claim that the meanings of the terms used in an observation statement are at least partially dependent on theory. To use Popper's example (1959: ch.V, §25), suppose someone reports 'Here is a glass of water'. This might seem like a straightforward observation-statement. However, Popper claims that it is part of the meaning of 'glass' and of 'water' that these are kinds of stuff that show law-like behaviour. If it turned out, for example, that the stuff in the glass was actually inflammable, then that would show that it was not, after all, water. Thus 'Here is a glass of water' is not merely a report of what is seen, but assumes much theoretical knowledge.

However, in his 1971 paper 'Theory and observation' Ernst Nagel argues that the theory-ladenness of observation-statements need cause no problems in practice. Nagel accepts that observation-statements presuppose various theories and background information. Nevertheless, he holds that theories can be tested by observations. This is because the theories assumed by the observation-statements that report the results of some experiment will generally be different from the theory that the experiment is testing. For example, Newton conducted various experiments with a glass prism to test his theory that white light is made up of coloured light. The result of the experiment can be reported by an observation-statement: 'When light is shone into one side of a glass prism rays of red, green and purple light can be seen on the other side.' This description is theory-laden. Calling something a 'glass prism', for example, assumes that the prism is actually made from a particular substance. However, the theories assumed by the description do not include Newton's theory of light. The observation-statements are independent of this particular theory and so can test it.

Nagel's suggestion implies that scientists with different theoretical orientations can often mean the same thing by their observation statements. So long as the theories assumed by a description do not include those

theories about which the scientists disagree, their differing theoretical beliefs will not lead to any difference in meaning. Communication does not need to be theory free but just neutral between the theories under debate. At this point it can be seen that the *DSM* committee were wrong to describe their classification system as 'atheoretical'.⁸ Still, it may yet be possible for it to be theory-neutral.

3.3 Problems with deciding where to look

Commonly, it is claimed that phenomena are too numerous and too rich for a scientist to be able to set about observing everything. Someone who is attracted to this line of argument will claim that classification systems must draw on some theory or another, as a theory must be used to decide which features of the entities under study are of scientific interest.

Although philosophers generally accept that scientists require a theory to help them to decide what to observe, there is a tradition in taxonomy that denies that this is the case. Proponents of numerical taxonomy sometimes deny that scientists must be selective with regard to the features of entities they consider in constructing classification systems (for example, Sneath and Sokal, 1973: 11). Here I will assess this claim.

The numerical methods most often used in the creation of categorical classification systems (such as the *DSM*) are cluster analytic techniques. Data are collected on many variables of the entities being analysed. These data are then plotted in multi-dimensional space. Similar entities end up being close together on the plot, and the distance between any two entities is a measure of their average similarity. A classification can then be extracted by searching for 'clusters', groups of highly similar entities.

Cluster analysts have sometimes held that any and all variables can be included in the analysis, and that thus an analyst who uses many variables can succeed in obtaining an atheoretical classification system (Sokal and Sneath, 1963: 114).⁹ Is this claim true? I suggest not. The problem is that the clusters found in the analysis depend on the *variables* analysed, but knowledge of the natural structure of the domain would require knowledge of the true similarities and differences between the various *properties* of the entities in the domain. Unfortunately, it is quite possible for us to miss the mark and choose variables that do not correspond to true properties. Thus, almost certainly 'distance from my desk' or, more seriously, 'being a schizophrenogenic mother' are variables that fail to measure genuine properties.¹⁰ Only if the cluster analyst's variables measure genuine properties will the clusters obtained reflect the natural structure of the domain. An analyst who wants to pick variables that measure genuine properties must rely on the current best scientific theories. As such, a theory is needed to guide the choice of variables, and as a consequence cluster analysis cannot be used to produce atheoretical classification systems.

Might it be possible to use Nagel's suggestion again at this point?

Although the selection of variables that measure genuine properties requires some theory, might it be possible to use a theory that is not among those about which different mental health professionals disagree? Unfortunately not. In a cluster analysis of psychopathology, biologically orientated psychiatrists will want to include biological variables but may well consider variables linked to 'defence styles' to be suspicious. Psychiatrists adhering to different theoretical frameworks will disagree. We can conclude that, contrary to the claims of some of its proponents, cluster analysis is not a technique that can be used to construct atheoretical classification systems.

If the *DSM* cannot be theory-free, what theory does it use? I suggest, as have many writers before me, that the *DSM* tacitly assumes that some biological account of mental illness will prove to be correct. The *Sourcebook* (Widiger, Frances, Pincus *et al.*, 1994, 1996, 1997) published alongside the *DSM-IV* reveals that the studies appealed to by the *DSM* committees are mainly biological in orientation. These studies examine, for example, the biological correlates of disorder, they assess whether drug treatments differentially affect different groups of patients, they look at whether disorders run in families, and at whether particular disorders tend to affect people of a particular age and sex. Unfortunately, biological accounts of mental illness are by no mean uncontroversial. As the *DSM* tacitly assumes some biological explanation for mental disorder, the *DSM* categories stand, or quite possibly fall, with such an account.

4 THE *DSM* AND FEEDBACK IN APPLIED SCIENCE

If the *DSM* is affected by pressures that arise from the ways in which it is used, this may be another reason for being suspicious of its categories. Here, I examine the ways in which the *DSM* has been affected by its use as a nomenclature for completing medical insurance forms. I ask whether such feedback makes it more or less likely that *DSM* categories will correspond to natural kinds of disorder.

4.1 Insurance and the *DSM*

Since the late 1960s it has been standard practice for medical insurance companies to request a *DSM* diagnosis before they will consider reimbursing for psychiatric treatment. Those responsible for the most recent editions of the *DSM* claim to have been entirely uninfluenced by insurance considerations.¹¹ I will argue that this is not true and that the *DSM* has been and continues to be affected by pressures stemming from medical insurance.

Pressures to include new diagnoses in the DSM

The fact that third-party payers only reimburse treatment for patients with a *DSM* diagnosis provides an incentive for psychiatrists and patients to lobby for new disorders to be included in the *DSM*. When such lobbying is

successful, new diagnoses come to be included in the *DSM* as a direct result of insurance pressures. In *The Harmony of Illusions* (1995), Alan Young argues that Post Traumatic Stress Disorder was included in *DSM-III* partly as a result of such lobbying. As the Veterans' Administration pays for the treatment of all combat-related disorders, the inclusion of PTSD in the *DSM* enabled thousands of mentally disturbed Vietnam veterans to obtain treatment in VA centres.

Less successfully, family therapists have been lobbying since the 1970s to have diagnoses suitable for describing the family problems they treat included in the *DSM*. This lobbying is explicitly motivated by a desire to obtain insurance coverage for their patients (Sletten, Spitzer and Hedlund, 1976: 20). To date, family diagnoses are not included in the *DSM*. The interactional model of mental illness adopted by family therapists is radically different from the medical/biological approach adopted by the *DSM*, making the development of *DSM*-style family diagnoses extremely difficult. However, attempts to get such diagnoses included continue.

Once a diagnosis is included in the *DSM*, the diagnostic criteria may be altered between editions so that more or fewer patients fall into the category. The *Sourcebook* that accompanies the *DSM-IV* documents many of the decisions made by the various Working Groups. Occasionally the *Sourcebook* records cases where the Working Groups considered altering diagnostic criteria in order to make it easier for patients to obtain reimbursement. For example, the Working Group examining Major Depression with a Seasonal Pattern state that 'In favour of broadening criteria are the following considerations . . . there is a need for reimbursement availability for phototherapy without waiting for a third episode.' (Rush, 1996: 15) Similarly, the Working Group on Post Traumatic Stress Disorder note that 'requiring a minimum duration before a diagnosis of PTSD could be made might reduce help-seeking behavior as well as reimbursement for treatment' (Davidson, Foa, Blank *et al.*, 1996: 596). The mechanisms of insurance coverage create pressures for the *DSM* to find diagnoses for more and more patients.

Pressures to massage diagnoses

Insurance coverage also creates incentives for psychiatrists to massage the diagnoses of individual patients. Psychiatrists may wish to protect patients from stigma, and thus record less severe diagnoses than would be justified. Alternatively, they may over-emphasize how sick their patients are in order to justify treatment.

In the 1960s and 1970s insurance forms were generally returned via the patient's employer or by the patient, so psychiatrists avoided recording severe or socially unacceptable diagnoses (Chodoff, 1972; Grossman, 1971). A 1977 study comparing insurance claims and confidential diagnoses found that 5.4% of the patients were schizophrenic according to the insurance forms compared with 10.4% according to the confidential diagnoses. Neuroses made

up 70.6% of the insurance claim diagnoses, but only 28.4% of the confidential diagnoses (Sharfstein, Towery and Milowe, 1980).

More recently, insurance companies have introduced a range of measures designed to reduce their costs, and it has become increasingly difficult to obtain reimbursement for mental health care. Many of the cost-cutting measures introduced by insurance companies make the diagnosis a patient receives of crucial importance in determining the care a patient receives. For example, peer-review physicians employed by insurance companies will consider whether the proposed treatment is appropriate given the diagnosis recorded, and Health Maintenance Organizations tend to make the number of sessions of care provided depend on the diagnosis a patient receives.

Such practices encourage psychiatrists to record more severe diagnoses. There is some evidence that mental health professionals have started to respond to these financial incentives. A 1988 survey of social workers found that 59% admitted using mental disorder diagnoses for insurance purposes even when they were unwarranted clinically, and 86% diagnosed individual family members when the primary concerns were family or marital issues (Kirk and Kutchins, 1988).

In addition, I hypothesize that economic and social considerations can influence the diagnoses psychiatrists make more subtly than by encouraging them to make fraudulent diagnoses. Many patients are borderline between several diagnoses. Financial and social incentives will have an effect when a psychiatrist who could equally well make one of several diagnoses is motivated to favour one possible diagnosis over the others. When finding cases of a particular type is rewarded, cases of that kind will be found. Over time, diagnostic boundaries will change, because once one case is perceived as a case of, say, major depression, similar cases encountered later will come to be grouped with it. Sometimes official diagnostic criteria will be altered as a result, and even when no changing in the wording of criteria occurs, the effective boundaries of the category may still expand or shrink, as criteria can come to be interpreted more or less strictly. In this way the use of *DSM* diagnoses in completing insurance forms leads to new pressures on diagnosis that in time can feed back and have an effect on accepted diagnostic criteria.

4.2 The epistemic significance of feedback

I have shown that pressures arising from medical insurance can affect the *DSM*. How might such feedback affect the chances that *DSM* categories will correspond to natural kinds of disorder?

Unfortunately, the feedback that arises from the use of the *DSM* in completing insurance forms is almost certainly a bad thing epistemically speaking. The basic problem is that the success of the practice of using 'schizophrenia', 'major depression' and so on to complete insurance forms does not require the diagnoses given to be valid. From the point of view of the patient and psychiatrist, all that matters is that the insurance company

pays for the treatment. Whether they do this does not require that patients actually have the diagnosed disorder, but only that the insurance company believes that they do.

Even from the point of view of the insurance company there is no direct link between insurance being a success (that is, the insurance company making profits) and patients receiving valid diagnoses. In order to make a profit the insurance company needs to be able to predict the average cost of patient care. However, the cost of patient care is only weakly correlated with diagnosis. McCrone and Phelan (1994: 1025) found that diagnosis predicted only 3% in the variation in length of hospital stay, and conclude 'Diagnosis, even when clearly defined, is a poor indication of resource utilisation'. As diagnosis is only weakly correlated with the cost of care, whether insurance companies make a profit will not depend on patients receiving accurate diagnoses. Rather it appears that the requirement that patients receive a particular diagnosis to qualify for treatment is just used as a means of restricting the number of patients who qualify. So long as only a few people are diagnosed as suffering from schizophrenia, say, it does not matter whether or not those who are so diagnosed are actually schizophrenic. As the success of using the *DSM* to complete insurance forms is largely independent of the validity of the diagnoses made, there is no reason to expect that pressures that arise from the desire to make the process of filling in insurance forms work better (for the patient, psychiatrist or insurance company) will lead to diagnoses being more valid.

Once it has been decided that feedback arising from the use of the *DSM* for completing insurance forms is epistemically undesirable, the question arises as to whether, and if so how, such feedback can be controlled. I suggest that the mechanisms via which feedback operates should not be thought of as being occult, and that there is no reason why they cannot be discovered and measures be introduced to counteract them.

Feedback that arises from the use of *DSM* categories for completing insurance forms could be limited in various ways. Much diagnostic creep arises because a psychiatrist is faced with a patient who is borderline between several equally appropriate diagnoses, one of which carries more practical benefits than the others. Thus diagnostic creep can be controlled by minimizing the proportion of cases in which psychiatrists will be torn between alternative categories. This route is already being adopted by the creators of Health Resource Groups (HRGs), the British version of Diagnostic Related Groups, which are designed to facilitate financial planning within the NHS. Early versions of HRGs distinguished between psychotic and neurotic depression, a distinction where there were many borderline cases. On finding that the use of codes for these diagnoses was 'particularly idiosyncratic' (NHS Executive, 1997: 264), it was decided to abandon this distinction. Later versions of HRGs distinguish between depressed patients who are 'sectioned' (that is, legally detained because they are believed to pose a threat

to themselves or others) and those who are voluntary patients. Splitting categories on the grounds of such 'hard' criteria will reduce diagnostic creep.

The pressures on psychiatric classification that arise from its use by insurance companies can also be reduced by limiting the practical consequences of diagnosis. For example, as we have seen, in the 1960s and 1970s many psychiatrists recorded less severe diagnoses on insurance forms for fear that the forms would be seen by patients or their employers. Feedback that results from such practices could have been stopped by changing the system by which insurance was paid so that the patient's employer did not return the forms to the insurance company.

There is nothing occult about the feedback mechanisms that I have described. Their mechanisms can be discovered and it will often be possible to introduce measures to limit their effect. In the case of the *DSM*, however, such measures are not being taken. As we have seen, far from trying to prevent the *DSM* being affected by insurance considerations the *DSM* committee knowingly alters the classification scheme for insurance purposes. This makes it less likely that *DSM* categories will describe natural kinds of disorder.

CONCLUSION

I have argued that whether a condition is a disorder is partly a value-judgement. Nevertheless, at least some of those conditions that are generally considered to be mental disorders are plausibly natural kinds. However, while there may well be natural kinds of mental disorder, we should not expect the *DSM* to tell us what they are. Classification is theory-laden and thus the *DSM* can only be as good as current psychiatric theory. In addition, I have shown that pressures stemming from medical insurance can affect the *DSM*. Plausibly, the *DSM* will also be affected by pressures that emerge from the ways in which it is used in other contexts. I conclude that the categories of the *DSM* are unlikely to come to reflect the true structure of the domain of mental disorders in the foreseeable future.

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Notes

1. These debates over homosexuality are described in detail in Bayer (1981).
2. This account of disease is developed in greater detail in Cooper (2002a).
3. These studies are summarized in the *DSM-IV Sourcebook* (Widiger, Frances, Pincus *et al.*, 1994, 1996, 1997).
4. Young (1995: 5): 'The disorder is not timeless, nor does it possess an intrinsic unity. Rather, it is glued together by the practices, technologies, and narratives with which it is diagnosed, studied, treated, and represented and by the various interests, institutions, and moral arguments that mobilised these effects and resources.' As another example, Aronowitz (1998: ch. 3) argues that Lyme disease has been socially constructed and is thus not a natural kind.
5. Young (1995) is, of course, aware of cases such as Pepys' but he claims they are not cases of Post Traumatic Stress Disorder. On this, however, reasonable people can disagree.
6. This point is made by J. Bogen in comments following Hacking (1988).
7. A series of papers in *Schizophrenia Research* (1995) is devoted to the question of whether schizophrenia is a heterogeneous disorder.
8. As claimed in the introduction to the *DSM-III* (APA, 1980: 6–8).
9. Lorr (1982: 461) takes a similar view and states that cluster analytic techniques 'make possible the discovery of natural groupings'.
10. Schizophrenogenic mothers were thought to induce schizophrenia in their children.
11. For example, in an interview with *Psychiatric News* Spitzer claimed '... none of the changes [to the *DSM*], then or now, is political in nature. We have strongly and successfully resisted any changes in the draft D.S.M.-III not based on good, sound knowledge.' (HMG, 1977)

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