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Classifying Madness

A Philosophical Examination of the Diagnostic and Statistical Manual of Mental Disorders

by Rachel Cooper

Springer

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A PHILOSOPHICAL EXAMINATION OF THE DIAGNOSTIC AND STATISTICAL MANUAL OF MENTAL DISORDERS

by

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Some of the material in this book has been published previously. Chapter One draws on "Disease" in *Studies in History and Philosophy of Biological and Biomedical Sciences* 33 (2002), 263-282. A section of Chapter Two has been published as "Why Hacking is wrong about human kinds" in the *British Journal for the Philosophy of Science* 55 (2004), 73-85. The whole book is based on my Ph.D. thesis of the same title, submitted to Cambridge University, January 2002. A short summary of this Ph.D. has appeared as "What is wrong with the D.S.M.?" in *History of Psychiatry* 15 (2004), 5-25.

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INTRODUCTION

This book is about the Diagnostic and Statistical Manual of Mental Disorders, more commonly known as the D.S.M. The D.S.M. is published by the American Psychiatric Association and aims to list and describe all mental disorders. Within its pages can be found diagnostic criteria for types of depression, types of schizophrenia, eating disorders, anxiety disorders, phobias, sleeping disorders, and so on. Also included are less familiar, and more controversial, conditions: Mathematics Disorder, Caffeine Intoxication, Nicotine Dependence, Nightmare Disorder.

It must be admitted that the D.S.M. is not an exciting read. Its pages follow a standard format: Each disorder has a numerical code. This is followed by a description of the disorder, which includes information regarding prevalence, course, and differential diagnosis. Finally explicit criteria that patients must meet to receive the diagnosis are listed. These generally include lists of the symptoms that must be present, restrictions as to the length of time that the symptoms must have been troublesome, and clauses that state that the symptoms must not be better accounted for by some other condition.

Until comparatively recently the D.S.M. was of interest to no one but the most bookish of research psychiatrists. The first edition, published in 1952, was designed specifically to enable the collection of statistics regarding hospital inpatient populations. Cheap, slim, and ring-bound, few read it. The second edition, published in 1968, was used more frequently, but it remained possible for psychiatrists to spend their working lives blissfully unaware of its existence. With the publication of the D.S.M.-III, in 1980, however, the D.S.M. took off. Within a short few years psychiatrists in the U.S. were using the D.S.M. on a daily basis. Nowadays the D.S.M. is embedded in mental health care at every turn. In the U.S., hospital records note a D.S.M. diagnosis and medical insurance companies demand D.S.M. codes before they will consider reimbursing for the cost of care. Worldwide, research papers are couched in D.S.M. terminology and pharmaceutical companies list the D.S.M. diagnoses that their drugs treat. Mental health professionals, and their patients, can no longer avoid being affected by the D.S.M.

Even those of us who are not directly influenced by the D.S.M are influenced by it indirectly. Plausibly, we conceptualise what is normal at least in part by contrasting it with what is not. Thus, those of us who are sane know we are sane because we don't behave in particular ways. It is because we don't hallucinate, and don't have panic attacks, and don't want to commit suicide, that we think of ourselves as being normal. As the links between academic and popular sciences of the mind are tight, classifications such as the D.S.M. play a key cultural role in defining what counts as "disturbed behaviour". Once included in the D.S.M, a disorder is sure to come to be recognised as a problem in popular culture, and, once the idea that there might be a condition such as “Pathological Gambling” or “Caffeine Addiction” becomes widespread, it affects how we think of ourselves. A few years ago, those who gambled away their rent money could think of themselves as being idiots, or desperadoes - or talented gamblers down on their luck - now they can’t long avoid wondering whether they need psychological help.

The D.S.M. is important, but it is also controversial. While its publishers, the American Psychiatric Association, claim that the D.S.M. is a scientific classification system based on sound data, many have doubts. Big business has interests in the D.S.M. Perhaps the D.S.M. has been distorted by pressures stemming from insurance companies, or from pharmaceutical companies? Others are concerned that whether a condition is classified as a mental disorder depends too greatly on social and political factors. Infamously, homosexuality was declared a non-disease following a referendum of the rank and file of the American Psychiatric Association. Since when, critics wonder, has scientific truth been discovered by a democratic vote?

More conceptual worries are also frequent. If classification requires a theory, and if mental disorders are poorly understood, then a sound classification system may be presently unobtainable. Possibly even attempting to construct a classification system that “cuts nature at the joints” is conceptually naïve. Maybe types of mental disorder are radically unlike, say, chemical elements, and simply fail to have a natural structure.

This book addresses these concerns. The first half of the book asks whether the project of constructing a classification of mental disorders that reflects natural distinctions makes sense. I conclude that it does. The second half of the book addresses epistemic worries. Even supposing a natural classification system to be possible in principle, there may be reasons to be suspicious of the categories included in the D.S.M. I examine the extent to which the D.S.M. depends on psychiatric theory, and look at how it has been shaped by social and financial factors. I aim to be critical of the D.S.M. without being antagonistic towards it. Ultimately, however, I am forced to conclude that although the D.S.M. is of immense practical importance, it is unlikely to come to reflect the natural structure of mental disorders in the foreseeable future.

As well as using philosophical theory and conceptual analysis to address these questions, I also use resources seldom employed by philosophers. In researching this book I travelled to the archives of the American Psychiatric Association and of the American Psychological Association, in Washington, and read all existing documents relating to the construction of the D.S.M.s I, II and III. The material in the archives mainly concerns the D.S.M.-III, published in 1980. Prior to the D.S.M.-III, the D.S.M. was not considered sufficiently important for extensive documentation to have been retained, and material relating to editions after the D.S.M.-III has yet to be deposited. I would have liked to read documents regarding the D.S.M.-IV as well, but for my project this would have been a luxury rather than a necessity. The committees responsible for the D.S.M.-IV adopted a policy of transparency, and sought to make the reasons for their decisions public. As such, much relevant material has been published in psychiatric journals and in a four-volume *Sourcebook* that accompanies the D.S.M.-IV. Moreover, present editions of the D.S.M. follow the same basic format as the D.S.M.-III and make the same kinds of assumption. As such, they continue to suffer from many of the same problems.

Classifying Madness will be of interest to both mental health professionals and to philosophers interested in classification in science. The D.S.M. has become extremely controversial, and the possibility that there may be philosophical difficulties with it has become a commonplace in the mental health literature. *Classifying Madness* offers mental health professionals an opportunity to explore suspicions that there might be conceptual problems with the D.S.M. For philosophers, this book aims to contribute to debates in the philosophy of science concerning natural kinds, the theory-ladenness of classification, and the effect of sociological factors in science. These issues are normally approached via a consideration of the natural sciences and, as will be seen, approaching them via a consideration of psychiatry helps shed new light on old problems.

This book can also be read as an exercise in applied philosophy. Somewhat unusually it is not primarily a work in applied ethics, but rather in applied philosophy of science. My intention has been to write philosophy that is directly of relevance to non-philosophers and that could contribute to solving practical problems. In keeping with this aim, the issues that I consider are all issues that have already been raised in the psychiatric literature, and I have paid far more attention to the scientific details and social context of the debates I consider than is usual in philosophical work. I suggest that works of applied philosophy such as this are of importance both because they can help solve practical problems, and because showing that this is the case provides one reason for thinking that philosophy is worth doing. While there are of course philosophical arguments that aim to prove that philosophy is worthwhile, those who doubt the worth of philosophy will almost certainly find these unconvincing. Showing that philosophy is of practical use thus provides a particularly effective justification for practising philosophy.

For readers who find it useful, an overview of the argument of the book follows. Those who prefer to take things as they come are invited to turn straight to Chapter One.

1.OVERVIEW OF THE ARGUMENT

The first half of this book asks whether it is possible to construct a classification of mental disorders that is "natural" in the sense that it "cuts nature at the joints". The paradigmatic example of a classification that does seem to be "natural" in this way is the Periodic Table. Whether something is an element, and (assuming it is) where it should be situated in the Table, are questions that can be determined purely by scientific research. Two chapters address the question of whether it would be possible to produce a classification system that reflects the structure of the domain of mental disorders in something like the way that the Periodic Table reflects the structure of the domain of chemical elements.

The first of these chapters considers the nature of mental disorder: Is it a purely scientific issue whether a condition is a disorder? Are there biological facts that determine whether something is pathological? Or, is whether a condition is a disorder at least in part a value judgement? In this chapter I show that the account of mental disorder implicit in the D.S.M. is unacceptable, and propose a new account. According to this account a condition is a disease if and only if it is a bad thing to have, those with the condition are unlucky, and the condition is at least potentially medically treatable. On such an account, whether a condition is a disorder is not merely a matter of biological fact, but also involves value-judgements. As a consequence, I claim, whether a condition is a disorder is not a question that can simply be decided by scientific research.

The second chapter asks whether conditions such as schizophrenia and depression are "natural kinds". "Natural kind" is a technical term used by philosophers to refer to those kinds that are objective and theoretically fundamental, like chemical elements and biological species. The differences between natural kinds, for example the differences between gold and copper, are ensured by the structure of the world. In this chapter I propose a new account of natural kinds, and argue that plausibly at least some types of mental disorder are natural kinds.

The upshot of these two metaphysical chapters is that mental disorders should be thought of in a way analogous to the way in which we think about weeds. Weeds are unwanted plants, thus whether a daisy is a weed is at least in part a value-judgement. Still, types of plant that are generally considered to be weeds - daisies, buttercups, stinging nettles - are natural kinds. Similarly, I argue that the claim that schizophrenia is a disorder is in part a value-judgement, but that it may well be the case that schizophrenia and depression are natural kinds.

The second half of this book concerns epistemic problems. I have argued that plausibly some types of mental disorder are natural kinds, but there may be reasons why a classification system that reflects their natural structure will be very hard to achieve. The third chapter considers the possibility that what psychiatrists observe is dependent on the theories that they hold. If this is the case, then it may well reduce the chance that a correct classification system will be developed. There is a danger that much present psychiatric theory is wrong. As such, if observation in psychiatry is theory-laden, false beliefs may well distort psychiatrists' observations of their patients and prevent then from seeing the true similarities and differences between types of mental disorder. I argue that theory-ladenness is indeed a worry, and that the D.S.M. can only be as good as current psychiatric theory.

The fourth chapter examines how the D.S.M. has been shaped by pressures that arise from the ways in which it is used. I show that the D.S.M. has been substantially affected by the needs of medical insurance and by the marketing practices of pharmaceutical companies. I produce an account of such "feedback effects" in applied science that makes it clear that these pressures on the D.S.M. mean that its categories are unlikely to reflect the true structure of the domain of mental disorders. The pressures on the D.S.M. that I discuss should not be thought of as occult forces. In principle it would be possible to introduce measures to prevent them affecting the categories included in the D.S.M. However, at present there appears little chance that the American Psychiatric Association will introduce any such protective measures. I conclude that although there may well be natural kinds of mental disorder the chances of the D.S.M.-V or VI describing them are remote.

CHAPTER 1

WHAT IS MENTAL DISORDER?

Since the publication of the D.S.M.-III in 1980, the D.S.M. has included a 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 behavioural, psychological, or biological dysfunction, and that the disturbance is not only in the relationship between the individual and society.[1]

With minor revisions this definition has also been included in later editions of the D.S.M. (for comparison, all these definitions are given at the end of the book in an Appendix). In this chapter I examine the notion of "disorder" used in constructing the D.S.M. One major issue to be addressed is whether there are objective, biological matters of fact that determine whether a condition is a disorder, or whether value judgements are necessarily involved.

At the outset, a note regarding terminology is necessary. In the philosophical literature on the pathological, as well as in much medical discourse, it has become usual to use "disease" or "disorder" interchangeably to refer to all pathological conditions - diseases in the narrow sense, injuries, wounds, and disabilities. This is the sense of disease on which it makes sense to say, for example, that "Health is the absence of disease". Here I shall follow this philosophical and medical usage - and will use "disorder" or "disease" interchangeably to refer to all pathological conditions.

As well as exploring conceptual issues, I will examine the political debates that have surrounded the development of the D.S.M. definition of "disorder". It may seem odd to consider conceptual and political problems together, but there are advantages. Many conceptual issues have come up in the political debates; thus considering the political debates can save the philosopher time. More importantly, many philosophers before me have written about disorder and have been almost totally ignored by physicians.[2] In part this is because medical debates over "disorder" often have political overtones to which philosophers have been inadequately sensitive. For example, within psychiatry, debates concerning accounts of mental disorder have been linked to the question of whether psychologists should

[1] A.P.A. 1980 p.6

[2] For example a recent edition of the *Journal of Abnormal Psychology* (1999 Vol.108 No.3) was devoted to discussion of accounts of disease. Bill Fulford was the only philosopher to contribute to the discussion or have his views discussed at any length, and he is also a practising psychiatrist

treat mental illness, and to debates concerning the status of homosexuality. By considering these debates alongside conceptual issues I hope that my discussion of accounts of disorder will be of relevance to the debates within medicine as well as to debates within philosophy.

1.WHY DEFINE "DISORDER"?

Whether a condition is considered a mental disorder often has social, economic, and ethical implications. As such, debates that hinge on whether some condition should be considered a disorder are commonplace in both medicine and popular culture. Are psychopaths evil or sick? Should health insurance pay for the treatment of nicotine addiction? Is it right for shy people to take character-altering drugs? All these debates may be seen to depend on whether the conditions are disorders, and developing an account of disorder may be hoped to help us in addressing such questions. For many individual philosophers and physicians this has been sufficient motivation to attempt to produce an account of disorder. Questions concerning the status of particular conditions have always been with us, however, and yet it is only in the 1970s that the American Psychiatric Association (from now on the A.P.A.) seriously began to try and define "mental disorder". Why did the A.P.A. become interested at this particular time?

One possible answer should be dismissed from the outset. Prior to the D.S.M.-III the A.P.A. produced classifications of mental disorders without ever producing a definition of "mental disorder". Retrospectively commentators have often seen the absence of a definition in the earlier classification systems as a strange deficiency.[3] How, they have asked, can one classify mental disorders without knowing what mental disorders are? The implication is that it is a logical requirement for a scientific classification system to contain a definition of its domain. These commentators suggest that the lack of a definition in the D.S.M.-I and D.S.M.-II is just one example of the ways in which these classification systems were deficient from a scientific point of view. As psychiatric classification became scientific they claim that it was inevitable that a definition of "mental disorder" would come to be included in the D.S.M.

That this response is mistaken, however, can be seen when it is remembered that most scientific classification systems do not explicitly define their domain. The *International Classification of Diseases*, for example, gives no definition of "disease". Similarly, taxonomies of the flora and fauna of particular areas do not normally start off by saying what they mean by living thing. Clearly it is quite possible to produce a classification without the domain being explicitly defined. It is the presence of a definition of "disorder" in the D.S.M.-III, rather than its absence in earlier editions, that is unusual.

I will argue that the A.P.A. became interested in producing a definition of "mental disorder" in the 1970s for political reasons. Understanding these reasons sheds light on much that otherwise seems odd in the debates over "disorder" – why

[3] For example, Klein 1978 p.41, Spitzer and Williams 1982 p.15

it is that interest in the question has ebbed and flowed, and why it is that so many discussants have been obsessed by the case of homosexuality, for example. It will also illustrate exactly why it is that defining "disorder" can be important.

During the 1970s the A.P.A. was under heavy attack from two quarters: On the one hand, "anti-psychiatrists" challenged the legitimacy of psychiatry as a branch of medicine. On the other, gay activists protested at homosexuality being classified as a mental disorder. Defining "mental disorder" was rhetorically useful for the A.P.A. in both these battles.[4] Armed with the "right" definition, it could show that mental disorders are medical disorders, and determine whether homosexuality is pathological.

First, the anti-psychiatrists: In the early 1970s the anti-psychiatry movement was in full swing. The anti-psychiatrists were a diverse group, united only in their distrust of psychiatry. They argued on various different grounds that mental illness is profoundly unlike physical illness, and that therefore psychiatry is only dubiously a branch of medicine. Thomas Szasz, in particular, was a serious irritation to American psychiatry at this time. Szasz was a trained psychiatrist, and published regularly in respectable and widely read outlets such as the *New York Times*. He claimed that psychiatric patients don't suffer from diseases but are rather malingerers, social misfits, and people with problems in living.[5] Correspondingly, psychiatrists should not be classed with medical doctors but are rather, at best, expensive agony aunts or, at worst, agents of social control.

Against the background of such attacks, in 1973, at the same time as setting up a Task Force to revise the D.S.M., the A.P.A. set up a Task Force to define the term "mental illness".[6] It was hoped that the definition produced could be used in the preamble to the D.S.M.-III. Considerable effort was put into defining "mental illness", as can be seen from the fact that a special session at the annual A.P.A. meeting was dedicated to the issue. Various contributors were invited to discuss the proposed definitions from the perspectives of psychoanalysis, law, medical insurance and sociology. Other discussants were asked to consider possible implications for the individual patient, and for the interface of psychiatry with other branches of medicine and psychology.[7] Unfortunately the A.P.A. archives do not hold a copy of the definition produced. Still, that such effort was expended in an attempt to defend psychiatry from the attacks of the anti-psychiatry movement is made clear by the organiser's expressed satisfaction that the Task Force had managed to "avoid an overly broad definition of mental disorders that would view all individual and social unrest or problems in living as psychiatric illness, and at the same time justify the designation of mental disorders as a subset of medical disorders."[8] The phrase "problems in living" had been popularised by Szasz, and I

[4] Kutchins and Kirk 1997 also consider the A.P.A.'s interest in defining "disorder" to be linked to these issues.
[5] Szasz 1960
[6] Barton 1973
[7] Spitzer 1975
[8] Spitzer et al. 1977 p.3

suggest that its use here indicates that the A.P.A. was seeking to refute his claims in particular.

At around the same time the A.P.A. was coming under attack from gay rights groups who wanted homosexuality removed from the D.S.M.[9] Gay rights protesters mobbed the 1970 A.P.A. annual meeting in San Francisco, shouting down speakers with whom they disagreed and disrupting much of the meeting. Throughout 1971 and 1972 the activists continued to protest against the A.P.A. position. Robert Spitzer, who would later become chairman of the D.S.M.-III committee, became involved in the debates and found defining "disorder" to be a useful way of defending his stance on homosexuality.[10] 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 claim, Spitzer formulated a definition of "mental disorder" that he claimed was satisfied by all the disorders in the D.S.M.-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 A.P.A. in 1973, when homosexuality was removed as a diagnosis from the D.S.M.-II and "Sexual Orientation Disorder", a diagnosis for homosexuals who are unhappy about being gay, was added. On becoming chairman of the D.S.M.-III Task Force, Spitzer returned to his definition repeatedly to defend decisions to include or omit conditions from the classification system.[11] In due course, a version of Spitzer's definition came to be included in the introduction of the D.S.M.-III.

2.SHOULD MENTAL AND BODILY DISORDERS BE CONSIDERED TOGETHER?

The D.S.M.-III definition speaks only of mental disorders, but the D.S.M.-IV includes a note distancing the A.P.A. from the idea that any meaningful distinction can be drawn between disorders that are mental and those that are physical. Thus, by implication, the current A.P.A. position suggests that mental and bodily disorders are fundamentally similar.

One of the main reasons for thinking that mental and bodily disorders should be considered together is that it is difficult to find any coherent criterion for deciding

[9] These debates over homosexuality are described in detail in Bayer 1981.

[10] Spitzer 1973, 1981

[11] See Spitzer and Endicott 1978 for a version of the definition used in the construction of the D.S.M.-III. In this paper the definition is used to defend the inclusion of diagnoses for people who lack sexual responsivity, tobacco use disorder, and anti-social personality disorder.

which disorders are bodily and which mental. One cannot simply divide diseases into those that have psychological and behavioural symptoms on the one hand and those that have bodily symptoms on the other, as many diseases have both psychological and physical effects. People with Down Syndrome, for example, suffer from mental retardation, but also have a distinctive appearance and often have heart problems. Epilepsy causes fitting but also mental confusion. Flu causes a temperature and makes our noses run, but it also makes us tired and irritable.

Nor can diseases be split on the basis of whether they have physical or psychological causes. Many, if not most, diseases will be affected by both psychological and physical causal factors. The risk that someone will develop schizophrenia, for example, is increased by social stressors, and also by drug abuse, birth complications, and genetic factors. Many diseases that are generally considered to be physically caused are made worse by stress, for example allergies and high blood pressure.

The D.S.M.-IV notes that there seems to be much that is "physical" in "mental" disorders, and much that is "mental" in "physical" disorders, but then it goes on to condemn any attempt to distinguish mental and physical disorders as a "reductionistic anachronism of mind/body dualism".[12] Here the D.S.M. errs. The physicalist is simply committed to the claim that minds are made from physical things (neurones, whatever). It is quite compatible for a physicalist to also hold that the mental can be distinguished from the non-mental, for example by features such as intentionality. If the mental and the non-mental are ultimately made from the same stuff this no more implies that they cannot be distinguished than the fact that chairs and tables are both physical implies that chairs and tables are indistinguishable. Physicalism itself does not imply that one account of disorder should encompass both mental and bodily disorders.

I suggest that the A.P.A. is right to think that mental and physical disorders should be considered together, but wrong to think that this conclusion follows from adopting physicalism about the mind. Rather the reason why it seems sensible to seek one unified account of disease is simply that attempts to find a clear-cut distinction between bodily and mental disorders have failed.

It is often thought that if mental and bodily disorders are considered together, political implications follow. Generally speaking, in the 1970s, psychiatrists were keen to consider mental and physical disorders as being similar, while psychologists preferred to consider them quite distinct.[13] The debate was seen as linked to the question of who should treat mental disorders. Tensions came to a head in a controversy regarding the wording of the introduction to the D.S.M.-III. Originally the introduction was going to contain the claim that mental disorders are a sub-set of medical disorders.[14] When they heard about this, the American Psychological

[12] A.P.A. 1994 p.xxi.

[13] This may well no longer be the case. A recent special edition of the *Journal of Abnormal Psychology* (1999, Vol. 108 No.3) was devoted to discussion of accounts of disease. In it many psychologists expressed the view that mental and physical disorders should be considered together.

[14] In a letter from Jack Weinberg, the President of the A.P.A., to Theodore Blau, President of the American Psychological Association (Weinberg 1977) it is claimed that the statement that mental disorders are a sub-set of medical disorders was never intended to be included in the D.S.M., but was

Association wrote and complained to the A.P.A., sought legal advice, and began lobbying for the claim to be removed.[15] The psychologists feared that any statement that mental disorders are medical disorders might be taken to imply that only psychiatrists should treat mental disorders and that potentially this could lead to insurance companies refusing to reimburse for treatment undertaken by psychologists. The Presidents of the A.P.A. and American Psychological Association exchanged a flurry of strongly worded letters.[16] In their defence the psychologists pointed out that the etiology of many mental disorders is unknown and claimed that "although there may be justification for considering mental disorders to be health disorders there is no justification for any attempt to equate mental disorders with medical disorders".[17] That the debate was motivated by concerns over professional control is made clear by the request of the Chairman of the D.S.M.-III committee, Robert Spitzer, that the exchange between the Presidents of the two associations be made public to the A.P.A. membership because this would "be another way of demonstrating our conviction that psychiatry is a specialty within medicine. It would also make clear to our profession that D.S.M-III helps psychiatry move closer to the rest of medicine."[18] Eventually, however, the psychiatrists were forced to back down and agreed not to include the offending sentence in the D.S.M. Unfortunately, the A.P.A. archives contain no documents that outline the reasons for this decision.

Psychologists have also mounted parallel attacks on psychiatrists. In the introduction to his 1960 *Handbook of Abnormal Psychology*, Hans Eysenck argues that psychologists, not psychiatrists, should treat the majority of mental disorders. Eysenck claims that psychiatry should be divided into two: a medical part "dealing with the effects of tumours, lesions, infections, and other physical conditions", and a behavioural part under which would fall most neurotic disorders as well as some or most of the functional psychoses. He accepts that physicians should be left to deal with the medical part, but when it comes to the treatment of the behavioural disorders he claims that "psychology is the fundamental science, and rational methods of treatment have to be based on a thorough knowledge of modern psychological theory".[19]

Whether mental and physical disorders are fundamentally similar or dissimilar is also often thought to have implications for patients. Being mentally ill is stigmatised in a way that being physically ill is not, and the mentally ill are often denied benefits that are granted to physically ill people. As a consequence, patient support groups for the mentally ill are often moved to argue that "mental illness is illness like any other", and that thus psychiatric patients should be treated like other patients. Claiming that mental disorders are biologically based and describing them as "brain

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rather merely a claim made in a paper by Robert Spitzer. Seeing as Spitzer's paper is included in the draft of the D.S.M.-III held in the A.P.A. archives (A.P.A. 1976) there are reasons for thinking that Weinberg's claim is untrue.

[15] Carter 1977

[16] Psychiatric News 1977

[17] Blau 1977

[18] Spitzer 1977

[19] Eysenck 1960 p.3

disorders" play an important role in the rhetoric used by such groups. For example, in 1999 Senators Domenici and Wellstone proposed a bill that would require U.S. medical insurance coverage for some mental illnesses to be equal to that granted for other medical disorders. The senators reasoned that "severe mental illnesses are biologically based illnesses and should be treated like any other medical illness".[20] Similarly, The National Alliance for the Mentally Ill, one of the best known U.S. mental health charities, states, "Just as diabetes is a disorder of the pancreas, mental illnesses are brain disorders...".[21]

Other patient groups have found their interests to be better served by arguing that mental and physical disorders are quite distinct. Often this strategy is employed by patients who suffer from disorders that are borderline between being considered as mental or as physical disorders and that can reasonably be claimed to have strong physical components. Such patients tend to argue that they are significantly unlike psychiatric patients and thus should not be treated like them. For example, The National Association of Councils of Stutterers appealed to Robert Spitzer when they found out that stuttering was to be included in D.S.M-III, and asked that stuttering be removed, because they wished to avoid the stigma attached to suffering from a mental illness.[22] They argued that stuttering probably has a neurological basis and is thus not a mental disorder. They lost the argument, and stuttering became disorder number 307.00. More recently, some patients with Chronic Fatigue Syndrome and some transsexuals have been campaigning for their conditions to be recognised as physical as opposed to mental disorders.[23]

These arguments put forward by professional groups and patient support groups are invalid. Even if someone doesn't suffer from a medical disorder it might be appropriate for them to see a psychiatrist. Healthy people visit doctors for immunisations. There is no reason why they shouldn't visit psychiatrists for help with problems in living. Equally, mental disorders might be a sub-set of medical disorders and it still be the case that psychologists are the best people to treat them. Psychologists already play a lead role in treating certain medical disorders, for example those incurable disorders where the only possible treatment is Cognitive Behavioural Therapy aimed at helping patients adapt to a new way of life.

The arguments put forward by patient support groups are also dubious. Patients with prototypical physical conditions are considered eligible for medical insurance payments and other benefits primarily because their conditions are thought to be involuntary and disabling. Thus, when considering whether other patients should be granted the same benefits, what is relevant is whether their conditions are also disabling and involuntary, not the general degree of similarity between their condition and prototypical physical disorders.

With these preliminary issues dealt with, I shall shortly move on to consider accounts of disease. First, however, it is worth briefly summarising the discussion so far. I have explained that I will be using the terms "disease" and "disorder"

[20] N.A.M.I. undated a.
[21] N.A.M.I. undated b.
[22] Psychiatric News 1980
[23] Chronic Fatigue Syndrome: Tucker 1996; Transsexuals: Gendertalk 1996, Minter no date.

throughout to refer to all injuries, disabilities, and diseases in the narrow sense. This usage is in line with that of much of the philosophical and medical literature on disease. I shall be looking for an account of disease that encompasses both mental and physical diseases. This seems the most reasonable path to take as it is plausible that mental and physical disorders cannot be cleanly distinguished. The claim that physical and mental disorders should be considered together has often been taken to imply that psychiatrists should treat mental disorders and that psychiatric patients should be granted the same benefits as patients with prototypically physical conditions. Neither of these conclusions necessarily follows.

3. ACCOUNTS OF DISORDER

This section examines existing accounts of disorder. Although my ultimate aim is to assess, and where necessary improve on, the D.S.M. account, I will not start with a consideration of that here. This is because the D.S.M. account can only be understood as a reaction to biological accounts, and so it is with these accounts that I shall begin.

3.1 Biological Approaches To Defining Disorder

Early biologically-based accounts of disease claimed that a condition is a disease if and only if it is statistically infrequent and reduces an organism's life-expectancy or fertility.[24] Some proponents of such an account have thought that it could work for mental disorders as well as physical disorders. In a 1975 paper Robert Kendell uses such a biologically-based account to defend psychiatry from claims that it only treats problems in living.[25] He argues that manic-depression and schizophrenia are genuine diseases because sufferers live less long, and have fewer children, than the rest of the population.

The claim that diseases are conditions that reduce life-expectancy or fertility must be rejected, however. Reduced life-expectancy is neither a necessary nor a sufficient condition for a person being diseased. People with minor diseases, for example warts and athletes foot, live as long as anybody else. On the other hand mercenaries and rock-climbers may be healthy but have short life-expectancies. Neither are health and fertility necessarily linked. Choosing to be celibate reduces someone's chance of having children but plausibly is not a disease.

A more sophisticated biological account of disease has been proposed by Christopher Boorse.[26] In line with the earlier biological accounts, Boorse seeks to construct an account whereby value judgements have no part to play in deciding whether a condition is a disease. Whether a condition is a disease is to be determined solely by biological facts.

[24] Scadding 1967
[25] Kendell 1975
[26] Boorse 1975, 1976a., 1977, 1997

Boorse urges us to think of the human body and mind being made up of numerous sub-systems. "Sub-system" is used in the broadest sense imaginable, referring to organs, systems in the body such as the nervous system, and sub-systems of the mind, for example those devoted to memory or language comprehension.[27] According to Boorse each sub-system has one or more functions that it performs in a healthy human.

How do we identify the function of a sub-system on Boorse's account? Boorse defines "function" thus:

> 'X is performing the function of Z in the G-ing of S at t' means 'At t, X is Z-ing and the Z-ing of X is making a causal contribution to the goal G of the goal-directed system S'.[28]

In other words, according to Boorse, the function of a sub-system is whatever it does that contributes towards achieving the goal of a goal-directed system. At first sight "goal" and "goal-directed" systems suggest that the sub-systems can only have functions if there is some conscious purpose behind them. Boorse, however, uses Ernst Nagel's notion of a "goal-directed system" as one that "tend[s] to persist in some integrated pattern of behaviour of activities in the face of environmental changes" and in which "the constituents of the system...undergo mutual adjustments so as to maintain this pattern in relative independence from the environment."[29] Homeostatic systems, such as the system that normally acts to maintain body temperature, are goal-directed systems in Nagel's sense. At a higher level, Boorse claims, the human being as a whole can be seen as a goal-directed system that tends to act to counteract threats to its continued survival and reproductive ability. Thus, on such an account the function of the heart is to pump blood, and this is because this is what the heart does that contributes towards the organism's goal of staying alive. When we are healthy each of our sub-systems performs its proper functions and all is well.

Sometimes, however, a sub-system dysfunctions. In such cases there is a disease (in the broad sense of "disease" in use here, i.e. a pathological condition). Thus, a heart attack is pathological because it prevents the heart pumping blood. As another example, a cut in the skin reduces the ability of the skin to perform its function of preventing pathogens entering the body.

In the remainder of this sub-section I shall argue that Boorse's account is unacceptable. Although I shall argue that it is wrong, Boorse's account is sophisticated and can be adapted in various ways. Thus in the course of arguing against Boorse it will be necessary for me to explore ways in which his account

[27] Boorse is not entirely consistent with respect to whether he thinks his account can be used for mental disorders. In Boorse 1975 and 1977 he limits his account to physical disorders. At other times he takes it to also apply to mental disorders (Boorse 1976a. and 1997). Most of those who have been influenced by Boorse take his account to apply to both mental and physical diseases, and even in those papers where he takes his account to apply only to physical diseases he gives no reason for this restriction. Thus it seems fair to here consider the adequacy of Boorse's account as an account of both physical and mental disease.

[28] Boorse 1976b.

[29] Nagel 1961 p. 408

might be improved, but it should be borne in mind that in all such cases I will eventually go on to show why these adaptations will not be enough to save him.

3.1.1 First Problem For Boorse – Finding An Account Of Normal Function

A fundamental problem with Boorse's account may lie in the account of function that he adopts. Much of the following discussion will revolve around the question of whether there is any account of function can be used to formulate a value-free account of disease, and so this point must be examined in some detail. As mentioned earlier, Boorse considers the function of a sub-system to be whatever it does that contributes towards achieving the system's goals. As Larry Wright has pointed out, this account of function cannot distinguish accidental from non-accidental contributions to the goal of the system. [30] According to Boorse's definition sweating has the function of cooling down the body, but this function would also be attributed to my accidentally knocking a bucket of water over myself when I happened to be hot.

There are two possible ways of dealing with this objection, and I shall consider the plausibility of each in turn. Boorse deals with the objection by claiming that the normal function of some system is whatever it *typically* does that promotes survival and reproduction in an appropriate reference class of organisms.[31] Boorse claims that the appropriate reference class for an organism is the group consisting of individuals of the same species, sex, and age. Thus accidentally knocking water over myself is not a normal function as it is not something that members of the reference class, that is organisms of the same species, sex, and age as myself, typically do. In contrast the normal function of my heart is to pump blood round my body because that's what hearts in members of the reference class usually do that contributes to the goals of the organisms. If my heart stops pumping blood then I am diseased, if I fail to knock water over myself I am not.

There are, however, reasons to doubt that Boorse's reference class trick will do the job required. Boorse claims that the reference class for an organism consists of individuals of the same species, sex, and age. However, reference classes are going to need to be far more fine-grained than this. What is normal depends on a host of additional factors. Masai are naturally sensitive to growth hormone, pygmies are not. Athletes normally have a lower heart rate than other people. People who live at high altitude, or in hot climates, adapt in various ways. Thus the organisms in a reference class must not only be of the same species, sex, and age, but must also be of the same race, and must have undergone similar training, and have lived in the same kind of environment. This means that some reference classes are going to turn out very small. Elderly female Masai mountain-bikers, Asian male teenagers who have been brought up in Wales, and half-Chinese half-Eskimo boy toddlers will all need their own reference classes. In those cases where a reference class consists of just one individual, accidental benefits and normal functions cannot be distinguished by appealing to what is normal for the reference class, as whatever occurs in the

[30] Wright 1973
[31] Boorse 1977 pp.556-7

individual will thereby occur in 100% of the reference class. Small, but non-singular, reference classes also present problems. In such classes the probability of the same accidental benefit occurring in the majority of the class is far higher than it is in a larger class. Thus, where the reference classes are small, Boorse's method of distinguishing accidental benefits from normal functions becomes unreliable. To sum up, Boorse's claim that accidental benefits will be statistically rare in the reference class and can thereby be distinguished from normal functions is only plausible when the reference classes are assumed to be large. Often, however, the reference classes will be small and in some cases they may consist of just one organism. For these reasons Boorse's suggestion for overcoming the problem of distinguishing normal functions from accidental benefits must be rejected.

The second way of dealing with the problem of distinguishing accidental benefits from normal functions is to reject accounts of functions that are based on contributions to goals altogether. We have only been led to the current problems through our acceptance of Boorse's account of function. As readers familiar with the literature on biological functions will be aware, Boorse's definition of function is not generally accepted (because of the problems it has with distinguishing functions from accidental benefits). Many theorists would instead adopt an evolutionary-based account of function. According to such theorists,

> The function of X is Z means:
> (a) X has been naturally selected because it does Z
> (b) Z is a consequence (or result) of X's being there.[32]

In other words, the function of a sub-system is whatever it does that it was naturally selected to do. Thus, the function of our eyes is to enable us to see. This is what eyes do, and is what they were naturally selected to do. This is the account of function that has been adopted by most theorists (apart from Boorse) who favour disease-as-dysfunction accounts.[33] However, I shall argue that it is also unacceptable.

There are difficulties with claiming that the function of a sub-system is whatever it evolved to do that while recognised by philosophers of biology have generally been ignored by disease-as-dysfunction theorists. The difficulties arise because selection pressures are seldom constant. As such, it is necessary to state the time period in which selective pressures must have promoted an ability for it to be

[32] This account of function has been proposed by a number of writers. It is most often attributed to Millikan 1984

[33] For example Papineau 1994, Wakefield 1993. Accounts of disease that employ an Aristotelian account of function have also been proposed (see, for example Megone 1998, 2000). On an Aristotelian account, functions are value-laden, and so such accounts do not seek to provide a value-free account of disease. Aristotelian accounts of disease are not discussed in detail here because they can only be understood within an Aristotelian framework, and setting out such a framework would simply take too long for a book of this kind. For those familiar with such accounts, however, I have two reasons for thinking them problematic. First, Aristotelian accounts of disease require one to adopt an Aristotelian metaphysics – and such a metaphysics is uncomfortably distant from that informing most contemporary philosophy. In addition, an Aristotelian account considers both diseases *and vices* to be states that diminish human flourishing, and I suggest that it will be problematic for the Aristotelian to adequately distinguish between the two kinds of state.

counted as a function. There are a multitude of options. For example, one could hold that the function of a sub-system depends on any of the following:

(a) Original selection pressures.
(b) Selection pressures in the recent past.
(c) Current selection pressures. [34]

It is not clear which of these options is to be preferred. All have unwelcome implications for a disease-as-dysfunction account. Appealing to original selection pressures leads to difficulties because in some cases a sub-system that originally evolved for one purpose later comes to be used for another. For example, it has been suggested that flies originally evolved wings to help cool them down. Only later were wings used for flying. If one claims that the function of a sub-system is whatever it originally evolved to do, then one must claim that flying is not a function of a fly's wings. As a consequence, and counterintuitively, on a disease-as-dysfunction account a healthy fly may be flightless.

Claiming that functions are determined by current selection pressures also leads to problems. In modern societies, humans are affected by selection pressures very differently from in previous times. Some risks have disappeared. While shortsighted people would once have been eaten by sabre-tooth tigers, today glasses correct their vision. Other risks are new. Those who are boring, have no sense of humour, or forget their partners' birthdays, would have been able to get away with it in the Pleistocene, but today risk reproductive failure. Coupling a disease-as-dysfunction account with the claim that the function of a sub-system is determined by current selection pressures results in the wrong conditions being classified as diseases. Being boring ends up as a disease, while shortsightedness doesn't.

Claiming that the functions of sub-systems are whatever they did that caused them to be selected in the recent past is also unsatisfactory. The period that counts as the "recent past" must be carefully selected in order to avoid both the problems posed by relying on original selection pressures, and those caused by relying on current selection pressures. Maybe it will not be possible to find such a time period at all. Even if such a time period can be specified, an account of function that makes use of it will have a somewhat arbitrary appearance. The account will end up claiming that the function of a sub-system is whatever it did that caused it to be selected between, say, 2000B.C. and 1000A.D. The proponents of disease-as-dysfunction accounts were motivated by a desire to show that disease is a natural category. An account of disease that makes essential reference to a time period that has been carefully selected so that the "right" functions are obtained does not seem consistent with this original desire.

Nor will it do to hold that selection pressures at all times are important. If this option is taken we may well end up with too few functions – plausibly in evolutionary history many attributes have been selected at one time but not at another – and if there are too few functions our account will provide too few diseases.

[34] List of possibilities adapted from Kitcher 1993

At this point all suggestions for determining the "normal" function of the sub-systems have been explored and none is suitable for incorporation in a disease-as-dysfunction account. This is a major problem for Boorse. Boorse claimed that a disease occurs when a sub-system fails to fulfil its normal function, but no current account of normal function can do the work he requires of it. It is possible that some other, acceptable, value-free account of normal function will be forthcoming. The present absence of such an account, however, provides a reason for beginning to suspect that disease-as-dysfunction accounts such as Boorse's should be rejected.

3.1.2 Second Problem For Boorse: Biological Accounts Of Disorder And Homosexuality

Biological accounts of disease might be expected to be highly attractive to psychiatrists. If it could be argued that there are biological facts that make it the case that mental diseases are "real" diseases then this could be used to defend psychiatry from anti-psychiatric attacks. However, since the 1970s, American psychiatrists have tended to reject biological accounts. I suggest that this is because these accounts suggest that homosexuality is a disorder, a view that has become increasingly untenable in American psychiatry.

According to the early biological accounts a condition is a disease if it is unusual and reduces life-expectancy or fertility. As only a minority of people are homosexual, and homosexuals have fewer children than other people, on such accounts homosexuality is a disease. Indeed Kendell makes it clear that he accepts this as a consequence of the account in the same article that he argues that manic-depression and schizophrenia are diseases.[35]

It is less clear whether someone who accepts Boorse's account must consider homosexuality a disorder.[36] According to Boorse's account there is a disease whenever a sub-system of the body or mind fails to fulfil its biological function. Maybe some sub-system of the mind has evolved to make sure that individuals are attracted to members of the opposite sex, and this sub-system dysfunctions in cases of homosexuality. But, of course, it might not be the case that there is any such sub-system. It might even be the case that homosexuality can be an evolutionary advantage. Maybe homosexuals are good at helping their relatives to raise children, for example. In the present state of knowledge, however, no one can be sure whether or not homosexuality is a dysfunction in evolutionary terms. Thus someone who accepts Boorse's account is forced to admit that homosexuality might be a disease.

As discussed earlier, in the early 1970s, the A.P.A. came under attack from Gay Liberation groups who wanted homosexuality removed from the D.S.M. Following its removal it soon became unacceptable for American psychiatrists to publicly express the view that homosexuality is a disorder. I suggest that this is why American psychiatry has largely rejected biological accounts of disease. Instead, within American psychiatry a consensus emerged that for a condition to be a disorder there must not only be a dysfunction but the dysfunction must be *harmful*.

[35] Kendell 1975a. p.310
[36] For a more detailed discussion see Ruse 1981

In the case of homosexuality there may or may not be some biological dysfunction, but even if there is a dysfunction, this need not be harmful, and so homosexuality need not be a disorder.

Boorse himself pursues an alternative option.[37] He claims that homosexuality is a disease but adds that his value-neutral account of disease means that this does not imply that it is bad thing. However, if Boorse's account is to be an account of *disease*, as opposed to an account of some quite distinct concept, it cannot stray far from our normal concept. As such, Boorse's value-free account will only be an account of disease at all if it is the case that the normative implications of our current concept of disease are slight. The furore surrounding the debate over the disease-status of homosexuality reveals that this is not the case. Gay rights protesters wanted homosexuality removed from the D.S.M-II because it seemed clear to them that calling something a disease implies that it is a bad thing. Their anger implies that it is part of out concept of disease that diseases are bad. Thus, I suggest, Boorse's account must be rejected. Despite Boorse's claims, that a condition is an evolutionary dysfunction is not a sufficient condition for it to be a disease, as a dysfunction that did no harm would not be considered to be a disease. In the next section we must consider whether an evolutionary dysfunction is even necessary for a disease. In considering this we come at last to the D.S.M. definition of disease.

3.2 D.S.M.-III And Disorder As Harmful Dysfunction

The introduction to the D.S.M.-III defines disorder thus:

> In D.S.M-III each of the mental disorders is conceptualised 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 important areas of functioning (disability).* In addition there is an inference that *there is a behavioural, psychological, or biological dysfunction*, and that the disturbance is not only in the relationship between the individual and society. (Emphasis added).[38]

This definition is a descendent of the definitions produced by Robert Spitzer in the debates concerning the disease status of homosexuality. In slightly modified form it is also included in the introductions to the D.S.M.-III-R, the D.S.M.-IV and the D.S.M.-IV-T.R. In a series of articles, Jerome Wakefield has convincingly argued that the core idea behind the D.S.M. definition is that a condition is a disease if and only if it is a harmful dysfunction.[39]

As has already been argued, for a condition to be a disorder it is not sufficient for there to be an evolutionary dysfunction. An evolutionary dysfunction that did no harm would not be considered a disease. On Wakefield's interpretation the D.S.M. definition recognises this and takes disorders to be *harmful* dysfunctions. Now, however, I shall argue that evolutionary dysfunction is not necessary for disease either, and that thus the D.S.M. definition must also be rejected.

[37] Boorse 1975 p.63
[38] A.P.A. 1980 p.6
[39] Wakefield 1992a., 1992b., 1993

The problem for the D.S.M. definition is that in some cases the genetic bases of conditions that we would normally class as diseases may confer an evolutionary advantage and thus be selected. In such cases, from an evolutionary point of view, there may be no dysfunction when cases of the disease occur. 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.[40] 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.

The evolutionary hypotheses concerning particular diseases that I shall discuss here are controversial. Still, even if the hypotheses turn out to be false, that counterfactually they might have been true will be enough to show that it is not *necessary* that a condition be an evolutionary dysfunction for it to be a disease. Even if sociopathy, for example, is not selected in the way described, we can imagine a hypothetical disease very like it that is.

A condition might be an evolutionary advantage in all environments, or it might just confer some biological advantage to sufferers in some present environments, or it might just have conferred benefits in the past. As discussed earlier, an evolution-based account of function must specify the time period in which selection pressures are going to be taken to be important for determining the functions of sub-systems (that is it must specify whether the function of a sub-system is what it was selected for originally, or what it is selected for in the present, or in the recent past). That a condition has been evolutionarily advantageous at some time, *t*, will only show that the condition is no dysfunction if *t* falls within the time period within which selection pressures are taken to determine the functions of sub-systems. As such, not all the cases of selected-for diseases that I shall discuss will disprove all disease-as-dysfunction accounts. Still, I hope to discuss enough cases to make it plausible that whatever the time period that is taken to determine functions, within that period some disease will have been, or at least counterfactually could have been, evolutionarily advantageous.

A condition may be selected because it benefits sufferers in some present environment. Mealey suggests that the genes for sociopathy are selected for this reason.[41] It makes sense to suppose that in a tough environment males who are violent and promiscuous may live longer and have more children than their milder-mannered counterparts. If this is the case then sociopathy may increase the biological fitness of otherwise disadvantaged males. If Mealey is right, and if functions are taken to be determined by current selection pressures, then in sociopathy there is no evolutionary dysfunction.

Alternatively, a condition might be of no benefit currently but have been biologically beneficial in earlier times. It has been suggested that agoraphobia and other anxiety disorders were adaptive when humans lived in more dangerous

[40] Wilson 1993 p.45 in reprint.
[41] Mealey 1995

environments.[42] In dangerous environments anxious people have a better chance of avoiding danger and so live longer and have more children than others. Whether diseases that were beneficial in earlier times can be said to be dysfunctions depends on the account of function adopted. If the time period within which anxiety disorders were biologically beneficial falls within the time period within which selection pressures determine functions, then anxiety disorders cannot be said to be dysfunctions.

A condition might be selected through kin selection processes. Through kin selection a condition that is of no direct benefit to an individual may be selected because it benefits the individual's relatives. Such mechanisms can occur because individuals are genetically similar to their kin. As such, an individual can increase the number of copies of their genes through helping their relatives to breed successfully. It has been suggested that the genetic basis of Generalised Anxiety Disorder is promoted for this reason.[43] People with Generalised Anxiety Disorder spend a lot of time worrying, often about the welfare of their relatives. It is possible that although their anxiety does not benefit people with Generalised Anxiety Disorder directly, it does help their relatives to have someone looking out for them. Again, if a disorder were selected through kin selection mechanisms within the period of time considered important for determining functions there would be no dysfunction from an evolutionary point of view.

Finally, we should consider conditions that are caused by several genes acting together in which the detrimental consequences to an individual who possesses all of the genes are offset by the advantages to relatives who just possess a subset. Sickle-cell anaemia is the classic example of such a condition. Individuals with two copies of the sickle-cell gene suffer from the disorder, but those with only one copy are protected against malaria. Simon Baron-Cohen has hinted that the genes that cause autism may similarly be advantageous to those who just possess a sub-set. He found that the relatives of autistic children have an increased probability of being gifted in areas such as engineering.[44] Whether such conditions should be thought of as being selected depends on whether one thinks of selection as acting on phenotypes or on genes. If we consider selection to act on phenotypes, then autism itself is not selected, as autistic people themselves are at an evolutionary disadvantage (the case is different from kin-selection as in kin-selection the individual with the disease has a high *inclusive* fitness).[45] On the other hand, if we think of selection as acting on the genes, then autism is selected; the genetic basis of the disorder causes proficiency in engineering and autism, and the genes exist because they do this. Here I do not wish to argue that the situation should be considered in one way or in the other. The case is just mentioned here because it tends to be discussed by evolutionary psychologists. Readers who think that selection acts on genes can consider it alongside the other cases, those who do not can reject the case and just consider the others.

[42] Nesse 1987, Marks and Nesse 1994.
[43] Akiskal 1998
[44] Baron-Cohen 1997
[45] Wakefield 1999 p.389 takes this line.

These examples show that whatever evolutionary account of function is adopted, it is plausible that in at least some cases the mind of an individual who suffers from a condition generally considered to be a disease will be fulfilling its evolutionary function. As such, we should conclude that it is not *necessary* that there be an evolutionary dysfunction for a condition to be a disorder. Thus the claim that a disease is a harmful dysfunction must be rejected. The D.S.M. account of disease is inadequate.

I have now completed my argument against the idea that whether a condition is a disease depends on whether it is a biological dysfunction. First, I have pointed out that providing a satisfactory account of normal function is problematic. This should make us doubt whether a satisfactory disease-as-dysfunction account will be possible. Second, even if some account of normal function is forthcoming, being a biological dysfunction is neither sufficient nor necessary for something being a disease. That a condition is a biological dysfunction is not sufficient for it being a disease because we would not consider harmless dysfunctions (e.g. possibly homosexuality) to be disorders. This shows that Boorse's account, according to which for a condition to be a disease it is necessary and sufficient that it be a biological dysfunction, must be rejected. The D.S.M., with its disease as harmful dysfunction account, recognises that harmless conditions are not diseases, but holds that being a biological dysfunction is at least necessary for a condition to be a disease. However this is also false. There may well be conditions that are diseases, but that are not biological dysfunctions, because they confer some biological advantage.

Thus, Boorse's account of disease and the D.S.M. account must be rejected, but what are the prospects for finding a better account? In a much cited recent article, Scott Lilienfeld and Lori Marino suggest that the failure of the D.S.M. account is symptomatic of a deeper problem.[46] They suggest that all proposed accounts of mental disorder have been wrong-headed, because it is in principle impossible to give a set of necessary and sufficient conditions for something being a "mental disorder". Lilienfeld and Marino claim that "mental disorder" is what they call a "Roschian concept". In this context by "Roschian concept" is meant something very close to a Wittgensteinian family resemblance concept.[47] Famously, Wittgenstein argued that there are no necessary and sufficient conditions for something being a game. Many, but not all, games are fun; many, but not all, have rules; many, but not all, have a winner. Rather than it being possible to give necessary and sufficient conditions for something being a game, games are united by a network of similarities, in the same kind of way that the members of a family share family resemblances. The members of the family need have no one feature in common, but any two members will be similar in a variety of ways. Similarly, Lilienfeld and Marino claim, necessary and sufficient conditions for something being a mental disorder cannot be given. Rather whether a condition counts as a mental disorder depends on how similar it is to prototypical cases, such as psychotic depression and

[46] Lilienfeld and Marino 1995. Most of the articles in a recent special edition of the *Journal of Abnormal Psychology* (1999 Vol.108. No.3) devoted to discussion of accounts of disorder discuss this idea.

[47] Rosch 1978, Wittgenstein 1953 §66, 67.

schizophrenia. Conditions that seem like these central cases get counted as disorders, but there are no general rules that determine what it takes for something to be a disorder.

Although Lilienfeld and Marino's paper has been influential, their main argument for the claim that "mental disorder" is a Roschian concept is flawed. They claim that no account of disorder in terms of necessary and sufficient conditions can be given because whether a condition is a mental disorder may be vague - normal sadness shades into depression, normal drinking shades into alcoholism. Characteristically whether something falls under a Roschian concept may also be vague and so Lilienfeld and Marino conclude that mental disorder is a Roschian concept. As has been pointed out by Wakefield this argument is completely confused.[48] That a concept has vague boundaries does not show that necessary and sufficient conditions for something to fall under it cannot be given. All it shows is that at least one of any necessary and sufficient conditions must also be vague. Thus, to use Wakefield's example, it may be vague whether an unmarried, seventeen-year old male counts as a bachelor. Still, necessary and sufficient conditions for being a bachelor can be given. We can still say that someone is a bachelor if they are an unmarried adult male. Whether a particular individual is a bachelor may then be vague because it may be vague whether or not they are an adult.

The only other reason Lilienfeld and Marino give for thinking that mental disorder is a Roschian concept is that attempts to provide necessary and sufficient conditions for the concept have repeatedly failed. Arguing that a concept is a family resemblance concept because necessary and sufficient conditions cannot be found ties in with Wittgenstein's approach in *Philosophical Investigations*. Wittgenstein asks his reader to "look and see whether there is anything common to all [games]".[49] It is because games can be seen to have nothing in common that he concludes that "game" is a family resemblance term. I shall argue that this is not the case with "mental disorder". In the next section I will give an account of mental disorder in terms of necessary and sufficient conditions that works. As mental disorders do have something in common, I argue, the claim that mental disorder is a Roschian concept should be rejected.

3.3 A Better Account

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 shall argue that by "disease" we mean a condition that it is a bad thing to have, that is such that the afflicted person is 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

[48] Wakefield 1999 pp.377-378
[49] Wittgenstein 1953, §66

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.

All three of my criteria, or criteria close to them, have previously been employed by other writers to provide accounts of disease. These writers' accounts will be referred to as I develop my own. The novelty of my account lies not in the criteria themselves but in their combination, in the arguments for them, and in the development of their implications. Now the outlines of my account have been sketched, I shall discuss each of my three criteria in more detail.

3.3.1 Diseases Are Bad Things To Have

A condition can only be a disease if it is a bad thing for the potential patient. The fact that a person is biologically different from others can never be sufficient to establish that they are diseased. Ginger haired people are different from other people but having ginger hair is not a disease. Similarly geniuses might plausibly all have something similar about their brains, but they are perfectly healthy. For something to be a disease, sufferers must both be different from most people and worse off. Many writers agree with me that a condition can only be a disease if it is harmful.[50] However, the discussion given here of the implications of this claim is novel.

Sometimes it is suggested that something can be a disease if it is a bad thing for society even if it isn't necessarily a bad thing for the potential patient. Here proposed examples include personality disorders and pedophilia.[51] This is a mistake. Although some behaviours that are bad for society are symptomatic of diseases, others are not, but are rather behaviour that is criminal or otherwise anti-social. Whether behaviour is symptomatic of disease cannot be determined by the type of behaviour - someone might set fire to buildings because they suffer from pyromania, or they might do it as an act of terrorism. Behaviour that is symptomatic of a disease can only be distinguished from behaviour that is not by its failure to be under normal voluntary control. And, if someone does not have normal control over their behaviour then this is a bad thing not only for society but also for the individual. Thus, something cannot be a disease just because it is bad for society, it must also be bad for the individual potential patient.

Sometimes it has been thought that for a condition to be a disease it must be a bad thing for most, or typical, potential patients.[52] On this view someone might have a disease even though in their particular case this was not a bad thing, so long as the

[50] King 1954 p.109 in reprint, Flew 1973 p.437 in reprint, Sedgwick 1973 p.123ff in reprint, Veatch 1973, Engelhardt 1974, Reznek 1987 ch.9, Wakefield 1992a., 1992b.

[51] For example, Spitzer 1999 claims that pedophilia is a disorder because it seems reasonable to suppose that there is an evolutionary dysfunction and "Because pedophilic behaviour results in the victimization of children, the dysfunction also represents a harmful condition by social standards." p.431

[52] Spitzer and Williams 1982 p.20

majority of the people with the condition were harmed by it. This is a mistake, as can be seen by considering the case of sterility. Some people who are sterile are deeply unhappy about it, for others it is a good thing (indeed many people choose to be sterilised). Quite conceivably it might be the case, or come to be the case, that being sterile is a good thing for the majority of sterile people. Still, regardless of this, those for whom it is a bad thing to be sterile would still suffer from a disease. Thus, someone can have a disease even if their condition is a good thing for most people. For someone to have a disease it is only necessary that the condition be a bad thing in their particular case.

How should it be determined whether a condition is a bad thing for the individual potential patient? This is a very difficult question and one that I will not be able to answer fully here. It should be noted that the question of what is good for an individual is not only a problem for me, but is a problem that arises in many other areas of philosophy. The question has been much debated by moral philosophers, particularly by utilitarians who must determine the nature of happiness if they are to have much chance of maximising it.[53]

Various accounts of the good for an individual have been proposed. All of them are problematic. The nature of the difficulties can best be grasped by thinking of the possible ways of determining what is good for an individual as varying along a scale. At one end of the scale lie methods that rely on asking actual people what they want. At the other end of the scale lie methods that claim that something is good for an individual if it helps that individual to meet some ideal standard of human flourishing. In between these two extremes lie methods that claim that something is good for an individual if that individual would judge it to be good in ideal circumstances, for example if they had all the information, and were calmer and wiser than they probably are.

Methods that rely on asking actual people are unattractive because it is plausible that actual people often do not know what is in their own best interest. They may make mistakes because they lack essential information. Thus Rene Dubos reports on a South American tribe who valued dyschromic spirochaetosis for the pretty coloured spots it produced on their skin.[54] However, in this case it seems the tribe only valued their condition because they were ignorant of some of its consequences; if they had known that the spot-producing condition had a tendency to kill them they would probably have decided that it was not, after all, a good thing to have.

Actual people are also notoriously prone to self-deception. Psychologists have repeatedly found that the vast majority of people believe they are cleverer and better looking than average.[55] Self-deception is perhaps particularly likely to arise when people are faced with making judgements regarding their health as within our society whether someone views themselves as being healthy or not has profound consequences. Sick people may both be stigmatised and receive certain social

[53] See, for example, Griffin 1986 for a fuller discussion of these issues.

[54] Dubos 1965 p.251

[55] For a study demonstrating such effects, that also reviews some of the literature in this area see Alicke, et al. 2001.

benefits. Thus people are often motivated to either consciously lie or to deceive themselves regarding whether or not they are sick.

Finally, it seems that some actual people are simply incompetent to judge the quality of their bodily and mental states. A lobotomised patient may sit around all day doing nothing and claim to be perfectly content, but here we feel that something has gone wrong with the individual's ability to evaluate their condition. Similar problems arise with all diseases that might themselves impair someone's ability to judge their condition.

Once the problems of relying on the judgements of actual people are realised, it becomes tempting to move to the opposite end of the scale and claim that something is good for someone if it helps that person meet some ideal standard of human flourishing. Here too, however, there are problems. Relying on the judgements of actual people to determine what is good is satisfyingly down to earth. On such a view if we want to find out whether a condition is good we have only to ask actual people in order to find out. In contrast appeals to "ideal standards of human flourishing" seem disturbingly anti-naturalistic. It is not at all clear how the ideal standards are fixed, nor is it clear how we can find out about them.

To a greater or lesser extent all other methods on the scale are beset by the problems of the extreme methods. When a method requires idealisation, epistemic problems arise. I know what I actually value, but how can I know what I'd value if I were more knowledgeable and wiser than I actually am? When a method relies on the judgements of actual people it risks giving the wrong answers; after all actual people make mistakes.

The problem of how to determine what is good for an individual will not be solved here. Rather I shall go on developing my account of disease and just make use of our everyday intuitions concerning the badness of various conditions. When, and if, some acceptable account of the good for an individual is developed this account can be plugged into my account of disease.

However the issue is eventually decided, plausibly it will be possible for one and the same condition to be a bad thing for one person but a good thing for another. Different people have different aims, different abilities, and different preferences. In addition, the same biological condition may produce varying experiences in different people - some schizophrenics see terrifying creatures, others see angels.

In *An Anthropologist on Mars* Oliver Sacks describes several cases of "patients" in whose cases it is plausible to think that a condition that would generally be considered a disease is a good thing. One chapter describes an artist who loses his colour vision following a head injury. After several years the artist adjusts to his new state and eventually he turns down a proposed new treatment. Sacks writes that "Mr I...has come to feel that his vision has become 'highly refined', 'privileged', that he sees a world of pure form, uncluttered by colour. Subtle textures and patterns, normally obscured for the rest of us because of their embedding in colour, now stand out for him."[56]

Similarly a few schizophrenics value their hallucinations to the extent that they would prefer to be schizophrenic than normal. One schizophrenic writes:

[56] Sacks 1995 p.35

> Hallucinations can be good or bad. The world can be transformed into heaven or hell at the drop of a hat...The plus side to them is certain moments of vividness that can turn a walk through a park, or whatever, into a walk through paradise…It's a type of drug, something that people would pay money for...I consider myself the luckiest of individuals, and am most pleased with this mind...My life is an adventure, not necessarily safe or comfortable, but at least an adventure. [57]

Often people with schizophrenia suffer cognitive deficits in addition to their positive symptoms.[58] Thus, it may well be that schizophrenics who value their hallucinations suffer other symptoms that must be weighed against any enjoyable aspects of their condition. Still, some people diagnosed as schizophrenic do not suffer from detectable cognitive deficits, and in others the deficits may be slight. Thus, in some cases, an individual who experiences enjoyable hallucinations might on balance benefit from having schizophrenia.

As discussed earlier, individuals may say that a condition is good for them when it is not. Here I am citing Mr I and the person with schizophrenia not simply because these people say that it is a good thing to be like them, but because they have given us good reasons for thinking that in their cases their condition may actually benefit them. Both Mr I and the person with schizophrenia have supplied us with a plausible explanation of why it might be a good thing for them to be as they are.

The best thing to say about cases where it seems that a condition is good for some people but not for others is that one and the same condition can be pathological for one person but not for another. The schizophrenic for whom it is a good thing to be schizophrenic is not diseased, while another for whom it is a bad thing is. Here I am suggesting that we should think about diseases in a way analogous to the way in which we think about weeds. A plant is only a weed if it is not wanted. Thus a daisy can be a weed in one garden but a flower in another, depending on whether or not it is a good thing in a particular garden.

This claim, that one and the same condition can be pathological for one person but not for another, may initially seem counterintuitive. I suggest that this implication of the concept of disorder has been easy to overlook because in the vast majority of cases there will be no disagreement between people as to whether or not a condition is a bad thing. So far as I know no one has ever claimed that cancer, or tuberculosis, or depression, or flu are good things to have. In addition, people who have a condition that is a good thing for them have largely been ignored by medicine because these people do not seek, nor need, help.

Still, that the same condition can be pathological for one person but not for another is recognised in some cases. Sterility is a disorder if it is not chosen, but not if it is the result of sterilisation. A scar may be a deformity if the person doesn't like it, but not if they do (perhaps, for example, it is a tribal marking). Occasionally people will be said to hear voices or to be a transvestite without there being any suggestion that they are sick. That a condition might be a disorder in some cases but not in others was recognised in the diagnosis of Ego-dystonic homosexuality, a diagnosis for homosexuals who didn't want to be gay, that was included in the

[57] Romme and Escher 1993 pp.130 and 133-4
[58] Lewis 2004

D.S.M. from 1980-1987. It is also recognised in demands made by Transgender pressure groups that only those who are distressed by their condition should be considered disordered.[59]

My suggestion that the same biological condition may be a disease for some individuals but not for others implies that we need to slightly rethink how we describe research on diseases. For example, epidemiologists are often said to study the incidence of disease. In measuring the incidence of a disease they count everyone who meets the appropriate diagnostic criteria. Asking whether the condition is a good thing in an individual case simply doesn't come into their work. I suggest that I can get around this potential problem in the following way: Instead of thinking of epidemiologists as studying the incidence of a disease, we should think of them as studying the incidence of conditions that are frequently diseases. To take a concrete case, suppose an epidemiologist is counting cases of schizophrenia. I accept that everyone who meets the diagnostic criteria for schizophrenia should be counted. All these people have schizophrenia (baring diagnostic error, of course). Still, it is consistent for me to hold that while these people all have schizophrenia it is possible that not all of them have a disease. For some individuals schizophrenia may be a good thing, and these individuals, while schizophrenic, are not diseased on my account. As a consequence, rather than saying that the epidemiologist is counting cases of a disease, I would say that they are counting cases of a condition that is of interest because it is normally a disease. This remains a useful activity on my account. Although I think that some cases of schizophrenia may not be pathological, I accept that the incidence of schizophrenia is of interest because incidence measures are of use for health planning, and for their value to those investigating the causes of schizophrenia, and so on.

I hold that for a condition to be a disease it must be a bad thing for the individual patient. Whether this criterion is met will not always be clear cut. In some cases some aspects of a condition may appear good but not others. The obvious example would be manic-depression. Many "sufferers" enjoy having manic episodes, but dislike the depressed periods that are normally part-and-parcel of their condition. Here whether or not their condition is a disease depends on whether they would be better off without it all things considered.

At this point one possible source of confusion should be cleared up. When I say that whether a condition is a disease depends on whether it is a bad thing for the "sufferer" I mean that disease-status depends on how the condition in and of itself is evaluated. Any secondary gains achieved via possession of the condition should be ignored in this evaluation. Thus, if someone has food poisoning they can consider this to be a bad thing in and of itself, even though they are glad to be poisoned because this gets them out of sitting a difficult exam. In such cases the food poisoning is a disease, because the condition is only valued because it just so happens to be linked to other benefits.

As mentioned earlier a disease must be a bad thing for the individual patient, and not just a bad thing for society. This might be thought to lead to difficulties with conditions such as pedophilia and personality disorders. If someone is a pedophile

[59] The House of Sissify, undated.

then this is bad for society, but it's not clear whether it need be bad for the pedophile who, after all, presumably acts in accordance with his desires. On some notions of the good for the individual this will not be a worry. An Aristotelian, for example, may claim that pedophilia is always bad for the pedophile because the condition reduces the degree to which the pedophile meets ideals of human flourishing. On other notions of the good for an individual, however, the worry remains. If, for example, it is thought that something is good for an individual if it fulfils their desires then it appears that having sex with small children need not be bad for the pedophile. I suggest that desire-fulfilment based accounts of the good can nevertheless adequately deal with conditions such as pedophilia so long as the disorder is thought of as being characterised primarily, not by a person's actions, but rather by their desires. Thus whether someone is a pedophile depends primarily on whether they want to have sex with small children, rather than on what they actually do. Whether pedophilia is a bad thing for the patient can then be taken to depend on their higher-order desires. A pedophile is diseased if they don't want to desire children as sexual objects but find that they can't help themselves, but not diseased if they are happy with their desires. All other conditions that are characterised by disordered desires (paraphilias, addictions, personality disorders) can be dealt with similarly. This stance comes very close to that adopted by the D.S.M.-IV. According to the D.S.M.-IV someone can only be diagnosed as having the disorder of pedophilia if "The fantasies, sexual urges, or behaviours cause clinically significant distress or impairment in social, occupational, or other important areas of functioning."[60]

Claiming that pedophilia need not be a disease is fully consistent with claiming that it is a bad thing for other reasons. This point is often missed by pressure groups who feel that it is necessary to claim that a condition is always a disease if they are to be able to voice disapproval of it. For example, in 1995 a Dallas-based Christian radio talk-show, *Point of View*, organised a petition campaigning against the D.S.M.-IV diagnostic criteria for pedophilia. The petitioners protested that the D.S.M.-IV "left an apparent loophole for certain child molesters who might escape being considered 'mentally disordered'".[61] More recently "Dr Laura", a radio talk-show host with an estimated audience of 18 million, has been campaigning against the same D.S.M.-IV criteria.[62] Quite rightly the American Psychiatric Association has responded to these allegations by claiming that it is perfectly consistent to hold that pedophilia need not always be a mental disorder while holding that "An adult who engages in sexual activity with a child is performing a criminal and immoral act".[63] All diseases are bad, but not all bad things are diseases.

[60] A.P.A. 1994 p.528
[61] Corbett 1996
[62] Saeman 1999
[63] A.P.A. 1999

3.3.2 The Afflicted Person Is Unlucky

My second criterion is that for someone to have a disease they must be unlucky. We only consider someone to be diseased if they could reasonably have hoped to have been otherwise. Thus ninety-year olds who can't walk as far as when they were younger are not diseased because we expect old people to become increasingly frail. Similarly baldness in men is not considered a disease, although it is in women.

Someone is unlucky if they could easily have been better off. In technical terms, their miserable state is not counterfactually robust. Talk of possible worlds is useful for making such claims precise. A possible world is a way in which things could have been different. One can imagine all the possible worlds as being arranged in a series of concentric spheres, with the actual world in the centre. In the actual world, things are as they are. The layer of possible worlds nearest to it differ in the myriad possible ways in which things could have been slightly different – in this one the cheese in my sandwich is a micron thicker, in that it is a micron thinner. A possible world must be fully consistent – in the world in which I have extra cheese in my sandwich, there will be less cheese left in the fridge, I will get a bit fatter, and so on. As one travels through the layers of possible worlds, and gets further from the actual world, the possible worlds differ more and more markedly from actuality. While the world in which I have extra cheese is quite close to the actual world, the world in which I joined the army rather than becoming a philosophy lecturer is further out, and the world in which I have wings and can fly yet more distant. As one moves away from the actual world one first passes through worlds that are physically possible (worlds in which the physical laws remain as they are), and then eventually through worlds that are physically impossible (worlds in which the physical laws differ).

When I say that for someone to suffer from a disease they must be unlucky I mean that there must be a good number of possible worlds at the right distance from the actual world in which they are better off. Which layers of possible worlds are the ones that we ought to be considering in such an evaluation? I suggest that the ones we need to consider are those that are consistent with the laws of human biology. We should ignore far distant worlds in which people live forever, or in which human anatomy has been re-jigged to make giving birth painless. Rather we should focus on worlds in which there are humans designed like us and ask whether we are badly off compared to them. Thus, an infant who is teething is not unlucky. In possible worlds consistent with the laws of human biology, young children go through teething. As such, while it can be unpleasant, teething is not a disease. In contrast, a child who has teeth growing from the roof of her mouth does suffer from a pathological condition. She is unlucky, because in possible worlds consistent with the laws of human biology most children have teeth that grow in at least roughly the right place.

Claiming that someone must be unlucky to suffer from a disease helps make sense of those disorders that afflict individuals who lie at the extreme ends of bell-curve distributions, of height, or I.Q., for example. Consider how we think of people who are severely mentally retarded because they are at the extreme end of the normal distribution of intelligence (as opposed to those who suffer from some distinct condition such as Down Syndrome). When someone has an I.Q. that is very low we think of them as being disordered, although we think of someone with a slightly lower than average I.Q. as being healthy. I suggest that this is because people who have a very low I.Q. are unluckier than those who have a slightly low I.Q. There are more possible worlds in which people who are very stupid are cleverer than there are possible worlds in which people who are slightly stupid are cleverer. At the other end of the scale, people who are unusually clever are of course considered healthy, because we think that intelligence is a good thing.

For some, talk of being unlucky has connotations that I must distance myself from here. In moral philosophy, discussion of luck has become linked to discussion of responsibility – thus someone is said to have suffered moral bad luck if their action turns out to have been bad for reasons beyond their control. When I talk of unluckiness here, however, I don't want to link it with questions of responsibility. Rather for me to say that someone is unlucky just means that their bad state is not counterfactually robust, and this implies nothing about the person's responsibility for their state. To take an example, if someone shoots himself in the foot he can be both responsible for his injury and unlucky in my sense. His injury is not counterfactually robust because there will be plenty of possible worlds in which his foot is intact (mainly worlds in which he didn't pull the trigger, but also worlds in which the bullet jams, and so on). Despite the potentially misleading connotations of talking about "luck", I think it is the best I can do here. Saying that someone has been "unlucky" is the closest one can get in everyday English to saying that there are possible worlds in which they are better off.

There are a variety of grounds we might have for thinking that our bad physical or mental state is not counterfactually robust, and that we are thus unlucky. The first, and probably most usual ground, is that we subjectively feel worse than we did yesterday or a week ago. When this happens we have reason to believe that we can, and indeed generally do, feel better. Second, we may consider ourselves to be unlucky because we have reason to believe that other people generally are in a better state than ourselves, for example someone born blind might consider themselves to be unlucky because other people generally can see. Third, we may have reasons for thinking that although many other people are in the same miserable condition as ourselves there is a good chance that everyone could have been better off. For example, we have theoretical reasons for thinking that although dental caries is an almost universal condition it is perfectly feasible for humans to be without it. Often, all three kinds of reasons will be available together; if I have flu, or suffer a panic attack, I will know that I myself am usually in a better state, that other people generally are in a better state, and that there are reasons for thinking that everybody could be in a better state.

For someone to be unlucky it needs to be the case that they could have been otherwise – there need to be a good number of possible worlds consistent with the laws of human biology where people like them are in a better state. Whether this is the case depends on objective facts, and is something that people can get wrong. On occasion, people have taken something to be a necessary part of human life when it is not, and so wrongly thought a condition to be normal. For example, as late as the 1960s, some branches of Chinese folk medical thought took measles to be a normal part of child development.[64] While it was recognised that measles was a bad thing, it was so common that it was taken to be something that humans just had to go through if they were to develop normally. Here, a mistake was made. The Chinese folk thought that children get measles in all possible worlds consistent with the laws of human biology, but actually this is not the case. As a result, they thought that measles is normal, when actually it is a disease. Equally, it is possible for people to think that something is abnormal, and a disease, when it is not. Thus some girls have taken themselves to be in an unusual and dire condition when they started menstruating, because they had not been warned that it is normal for women to menstruate.[65] These girls think they suffer from a disease when they do not, because they think they are unlucky, but they are not. For a condition to be a disease the individual with the condition actually needs to be unlucky – whether they think they are unlucky is a different issue.

At this point a possible objection needs to be dealt with. Some philosophers have claimed that there may be no possible worlds in which individuals who suffer from genetic diseases are better off.[66] This is because they think that the genetic make-up of a person is essential to them, so if someone had a different genotype they would be a different person. Thus, according to this account, if Fred has Huntington's chorea, he has Huntington's chorea in all possible worlds that are consistent with the laws of human biology – in worlds in which his genotype is different, Fred doesn't exist. If this is correct it looks like it causes trouble for me; Huntington's chorea is certainly a disease, but there are no possible worlds in which individuals with Huntington's are better off.

I'm not sure whether it is true that a person's genotype is essential to them, but here I will accept that this is the case for the sake of argument. Still, I think I can say that Fred is unlucky if he has Huntington's chorea. Even if there are no healthy Freds in any possible worlds consistent with the laws of human biology, there are still many people very like Fred in other possible worlds who are better off. These people are like Fred except that they lack the Huntington's gene – and these people generally have better lives. I suggest that this means that Fred is justified in thinking himself to be unlucky. There are many possible people like him who are better off.

I have now fleshed out what I mean when I say that for someone to suffer from a disease they must be unlucky. Somewhat similar criteria for a condition being a disease have been proposed by other authors, and I will now finish this section by examining how my own criterion differs from theirs.

First, claiming that a diseased person is unlucky is reminiscent of the idea that a condition must be statistically infrequent in order to be a disease.[67] However, although the two concepts overlap to a considerable extent, the notion of being unlucky is more flexible and for that reason preferable. Claiming that disease conditions must be statistically infrequent runs into well known difficulties. The requirement implies that if the only survivors of a nuclear holocaust were the

[64] Topley 1970
[65] While such cases are hopefully rare these days, March 1916 p.61 considered them common.
[66] Parfit 1984 ch.16
[67] Kendell 1975a holds this view, as does Taylor 1976 p.581 in 1981 reprint.

inhabitants of a remote leper colony the lepers would be cured by virtue of the new-found statistical normality of their condition. Employing the notion of being unlucky avoids this objection. Even though the lepers have survived the holocaust, they are still unlucky to have leprosy because there are many possible worlds consistent with the laws of human biology in which people like them are better off.

Second, in *The Nature of Disease* Lawrie Reznek suggests that something cannot be a disease if it is normal. Reznek claims that we choose what we will consider normal.

> We choose one norm rather than another because we wish to create certain priorities in dealing with all those conditions that we would be better off without...we would be better off without dental caries, but we regard it as an abnormal process because we choose to give its cure the same priority as we give to the cure of TB and multiple sclerosis. And so we regard it as a disease. We regard the process of ageing as normal, because we consider it more important first to rid ourselves of those processes we take to be abnormal. On the other hand, if it became as important to us to reverse the ageing process as it was to reverse cancer, we *would* come to think of ageing as an abnormal process, and classify it as a pathological condition.[68]

I suggest that Reznek's suggestion is unsatisfactory. Plausibly it is not true that the cure of dental caries is granted a higher priority than reversing ageing. In any case, and more importantly, here Reznek has got things back to front. Conditions are not abnormal because we aim to cure them, rather we can reasonably hope to cure some conditions because they are abnormal. Those with diseases are unlucky. This implies that they could have been healthy, and this suggests that it might be possible to find a cure.

3.3.3 The Condition Is Potentially Medically Treatable

For a condition to be a disease it must be such that it could potentially be treated by medical science.[69] A cure need not be presently available, but the condition must be such that there is reasonable hope that a medical treatment might become available in the future. This condition is required to distinguish diseases from other types of misfortune - economic problems, social problems, and so on. This criterion implies that conditions can sometimes come to be thought of as diseases as a result of treatments for them being discovered. Following the discovery that Paroxetine can be used to treat shyness, social anxiety disorder is a condition that is coming to be thought of as a disease for this reason.[70] Prior to the discovery no one thought of shyness as being something that might be treated by physicians, but the discovery of the drug-action proved them wrong.

I had been tempted to think that diseases must be presumed to have a biological basis, but such a claim is in fact both too strong and too weak. Claiming that

[68] Reznek 1987 p.94

[69] This view is also held by Reznek 1987 p.163. Veatch 1973 p.101 in 1982 reprint, notes that for a condition to be encompassed by the medical model physicians must be the technically competent experts, Taylor 1976 p.581 in 1981 reprint holds that diseases are the object of "therapeutic concern", Spitzer and Endicott 1978 p.18 note that when someone has a disorder there is a "call to action on the part of the medical profession".

[70] Irwin 1998

diseases must have a biological basis would be too strong because there might be some mental diseases where there is nothing wrong with the patient's brain. For example, it might well turn out that irrational phobias are completely indistinguishable from reasonable fears by the neurosciences.

Claiming that diseases must have a biological basis would also be too weak a requirement. Having a bad haircut and being unable to fit into last year's clothes are bad things and the sufferer may be unlucky. They have a biological basis, but they are not diseases. They are not diseases because we do not rely on medical help to fix these problems. The claim that diseases must be potentially treatable by medical science is made still more plausible when we consider how we think of conditions like acne, dandruff, and being over-weight. Acne, dandruff, and being over-weight are not thought of as diseases until non-medical means of dealing with them fail. It's only when we think the sufferer should resort to visiting the doctor that we think of them as suffering from a disease. The class of conditions that is "potentially medically treatable" is, of course, vague and messy. Is speech therapy for stuttering a medical treatment? Is corn-removal by a chiropodist? This indeterminacy as to what constitutes medical treatment may make it indeterminate whether or not some condition is a disease.

I claim that for a condition to be a disease it must be potentially medically treatable. This, of course, should not be taken to imply that someone must seek medical help in order to count as suffering from a disease. Some people who have a disease avoid treatment altogether, or treat themselves, for example by buying drugs over the internet. Still, on my account, someone with untreated syphilis, say, can count as having a disease. Their condition may be medically untreated, but it is still a condition that is potentially medical treatable.

If having said that diseases must be potentially medically treatable I went on to define "medicine" as the art of treating diseases, my account would be circular. However, there are other ways of giving content to "medicine". One possibility would be to take medicine to be the discipline practised by doctors and other medical personnel, and to adopt a sociological approach to deciding who counts as "doctors and other medical personnel". Very roughly, we would end up saying that doctors are those people who trained at medical school and are experts in human physiology and biology and other sciences.

Reznek, who also holds that for a condition to be a disease it must be potentially medically treatable, suggests that medical intervention can be defined "purely enumeratively without reference to the notion of disease - in terms of pharmacological and surgical interventions".[71] This suggestion must be rejected. Some medical interventions cannot be distinguished from some non-medical interventions in terms of what is actually done. If someone is given amphetamines by their doctor this is a medical intervention, if they are given them by their drug-dealer it is not. These interventions can only be distinguished sociologically.

Treating some conditions is technically feasible but socially unacceptable.[72] Thus technically it is possible to treat violent people with brain surgery. Someone who is

[71] Reznek 1987 p.163
[72] This point is also noted by Reznek 1987 p.167

violent may wish that they weren't, may be unlucky and may have a condition that is technically treatable, but still we don't think of them as having a disease. For a condition to be a disease it must be not only technically potentially treatable but also socially potentially treatable. Here homosexuality is an interesting case. As discussed earlier, in the 1970s, it was usual to consider homosexuals who were unhappy about being gay to suffer from a disease, and this disease could be treated by psychiatrists either through counselling to help the person accept their sexual orientation, or through therapy aimed at changing it. More recently it has become socially unacceptable for therapists to aim to change their clients' sexual orientation even at their clients' request.[73] At the same time homosexuality has ceased to be considered a condition that can be a disease. Here we have an example of a condition that ceased to be considered a disease as it became socially unacceptable to treat it.

My basic account of disorder has now been developed, but some further work is needed to make it plausible. To this end I will first re-examine how the account proposed here differs from the accounts of disease that I rejected earlier in this chapter. Then I will go onto show how various potential counterexamples and problems can be overcome.

3.3.4 How The Account Differs From Others

I have claimed that for a condition to be a disease three criteria must be met. First, the condition must be bad for the individuals with the condition. Second, the individuals with the condition must be unlucky. Third, the condition must be at least potentially medically treatable. These criteria, I claim, are jointly necessary and sufficient for a condition to be a disease.

This account differs most obviously from biologically-based accounts of disease. I have suggested that there are problems with the whole notion of "biological dysfunction". In any case, in so far as any sense can be made of dysfunction-talk, on my account this is not directly relevant to determining whether a condition is a disease. I have argued that it is neither necessary nor sufficient for a condition to be a disease that it be a biological dysfunction.

This being said, I do not want to imply that whether a condition is a disease has nothing to do with biology. On my account, facts concerning human biology can come into determining whether a condition is a disease at several points: Facts concerning human biology can be relevant to determining whether a condition is a bad thing for an individual. Thus, harbouring a few cancer cells is a bad thing because facts about human biology mean that such cells may well multiply and eventually cause death. Whether we can be considered unlucky also depends on biological facts – we are only unlucky if there are a good number of possible worlds consistent with the laws that govern human biology in which people like us are better off. Finally, biological facts have a role to play in determining whether a condition is potentially medically treatable. Medical treatments are prototypically

[73] For statements by professional organisations condemning "reparative therapy", that is therapy that attempts to change the sexual orientation of homosexuals, see Robinson 2000.

those that involve bringing about changes in our biological natures. On my account, biological facts thus remain relevant to determining whether a condition is a disease. Still, my account is not a biologically-based account, because on my account the question of whether a condition is a disease is divorced from the question of whether the condition is a biological dysfunction.

The account I have proposed also differs from the family resemblance account proposed by Lilienfeld and Marino. A family resemblance term is one for which a definition in terms of necessary and sufficient conditions cannot be given. Here I think I have provided a set of conditions that are jointly necessary and sufficient for something to be a disease, and thus I do not think that "disease" is a family resemblance term.

Having said this, I accept that my account of disease is not a "tidy" account. On my account, in many cases it will be indeterminate whether a condition is a disease or not – because it may be indeterminate whether the condition is bad, or whether sufferers are unlucky, or whether the condition can be appropriately medically treated. Some of this indeterminacy arises because my criteria are vague. Whether something is bad, whether there are a good number of possible worlds in which people like me are better off, and whether a condition is potentially medically treatable, may all be a matter of degree. Another possible source of indeterminacy is that my account of disease makes use of concepts for which a family-resemblance based account is plausibly appropriate. Most obviously, "medical treatment" looks like a family resemblance term. Thus, while my account is not a straightforward family resemblance account – because I have provided necessary and sufficient conditions for something being a disease – I accept that the messiness of some of my criteria means that my account has some similarities with family resemblance accounts.

3.3.5 Potential Counter-Examples And Problems

Unwanted Pregnancy Unwanted pregnancy may appear to cause problems for my account. A pregnant woman may wish she wasn't pregnant, if she used contraceptives she may well be unlucky to be pregnant, and her condition is medically treatable. Still, we don't normally think of unwanted pregnancy as a disorder.[74] Does this mean that my account has gone wrong?

I suggest that it does not. The pregnancy-objection can be rebuffed in at least two ways, with the appropriate reply depending on the account of the good that is adopted. On some accounts of the good for an individual it is possible to claim that an unwanted pregnancy need not be a bad thing for a woman. For the Aristotelian whether something is good for a person doesn't depend on whether it satisfies her desires, but on whether it helps her to approach an ideal standard of human flourishing. Some Aristotelians claim that having children is an intrinsic human

[74] Veatch 1973 p.89 in 1982 reprint cites unwanted pregnancy as a dubious candidate for the category of illness.

good, along with things like health, knowledge, pleasure and virtue.[75] From such a position it becomes plausible to claim that in most cases a pregnant woman who does not want to be pregnant has made a mistake in the same kind of way that a school-child who wants to be severely ill so he doesn't have to go to school has made a mistake. Both are mistaken about what is in their own best interest. They think they are badly off in being pregnant, or in being healthy, but they are wrong.

It should be noted that the Aristotelian does not need to take this line. An Aristotelian need not think that having children is a good thing, or, more plausibly, they might think that it is only a good if it is wanted, and then the above reasoning will not be attractive to them. Still, the position outlined above illustrates that it is possible to accept my account of disease and to also deny that unwanted pregnancy is a disease.

What, however, if one is an Aristotelian who denies unwanted pregnancy is a good, or if one adopts a desire-satisfaction account of the good? Then the second way of rebuffing the pregnancy-objection must be employed. From such stances an unwanted pregnancy is a bad thing, the pregnant woman may also be unlucky, and the condition can be medically treated. Thus unwanted pregnancy, at least in cases of contraceptive failure, counts as a disease. Admittedly our intuitions do not cohere with this result. However, I suggest that this is only to be expected because even if unwanted pregnancy is a disease now it will only have become a disease comparatively recently, and there can be expected to be a time-lag between changes in the disease-status of a condition and changes in our intuitions. Prior to the invention of effective contraceptives those who had unwanted pregnancies were not diseased because they were not unlucky. In addition, until comparatively recently it has been socially unacceptable to treat unwanted pregnancy (and to a certain extent this is still the case).

Animal And Plant Diseases According to my account a disease is a bad thing, the sufferer is unlucky, and the condition is such that it could potentially be medically treated. All these criteria can be met by animal diseases. If a dog has a bone stuck in its throat this is a bad thing, the dog is unlucky, and a vet can probably get the bone out.

It is harder to see how my account can work for plant diseases. Plants don't have a point of view and so it is hard to see how a condition could be a bad thing for a plant. Boorse takes this point to show that only a biologically-based account of disease can work for plant diseases.[76] He claims that plants, like humans, can be said to have sub-systems that have evolved to fulfil particular functions. According to Boorse when these sub-systems fail to fulfil their functions the plant suffers from a disease.

Boorse's account of plant diseases must be rejected, however. There are many conditions that render plants less able to fulfil their evolutionary function but that are not considered pathological. Many varieties of fruit and vegetables have been

[75] For example Hursthouse 1987 p.309
[76] Boorse 1975 p.53

developed that are good to eat but that are not very good at reproducing, for example seedless grapes and varieties of vegetable that are slow to bolt. Although these plants often fail to fulfil their evolutionary function they are not considered to suffer from some genetic disease. This shows that a biological account of plant diseases is inadequate.

My account of disorder can work for plant diseases so long as the criterion that a disorder be a bad thing is understood rather differently in the case of humans and of plants. For a condition to be a bad thing for a human means that they would have been better off being otherwise. For a condition to be a bad thing for a plant means that the condition causes the plant to deviate from an ideal standard. I suggest that ideal standards for domestic plants are determined by plant breeders; roughly the ideal standard for a plant corresponds to the picture on the seed packet. Even though seedless grapes cannot reproduce, they are not diseased because they are as plant breeders want them to be.

In some cases a similar notion of a condition being a bad thing can be used for animal diseases. Some domestic animals are bred to meet standards that put them at a biological disadvantage and may plausibly be supposed to cause them pain. For example, the British Rabbit Council standards for Netherland Dwarf rabbits dictate that the ideal weight for a Netherland Dwarf is 2lbs.[77] As they are so tiny, Netherland Dwarf does have smaller litters than larger rabbits and have more problems giving birth. Still, the small Netherland Dwarf rabbit is not considered to suffer from a genetic disorder, as she is as the rabbit breeder wants her to be.

Are Mental Diseases Particularly Problematic? Often it has been thought that mental disease is more problematic than bodily disease. As my account treats mental and bodily disease together I am under some pressure to provide reasons why deciding whether someone suffers from a mental disease might appear particularly difficult.

I suggest that questions concerning mental disease are especially frequent for rather mundane practical reasons. We debate whether someone suffers from a mental disease more often than whether someone suffers from a bodily disease because suffering from a mental disease carries heavier social and legal consequences within our society. The existence of the insanity defence, and of compulsory treatment orders, and the stigma attached to mental disease, all make it more important to decide whether or not someone suffers from a mental disease. In addition, problems linked to deciding whether or not someone suffers from a mental disease have received far more publicity than those linked to deciding whether or not someone suffers from a bodily disease. R.D.Laing, Thomas Szasz, Michel Foucault, and other influential authors chose to write about mental and not bodily disease. The emphasis of public debate is now perhaps beginning to shift; debates as to whether deaf children should be given cochlear implants, which are often in effect debates concerning the disease-status of deafness, have recently received widespread media

[77] British Rabbit Council undated

attention.[78] I suspect that deciding whether someone suffers from a bodily disease can be just as problematic as deciding whether they suffer from a mental disease.

Having said this, I should point out that it is not an integral part of my account of disease and I shall now outline how my account of disease is compatible with Foucault's and Laing's accounts of mental disease. I do not wish to commit myself to accepting these accounts, but they have been influential and so it is worth pointing out that they are compatible with my own. If acceptable, any of these accounts would explain why mental disease is more problematic than physical disease.

In *Madness and Civilisation* (1961 as *Histoire de la Folie*, 1967 in English) Foucault argued that contemporary notions of mental illness are rooted in contingent, historical developments. According to Foucault, prior to the Enlightenment the mad were tolerated and seen primarily as different, and possibly gifted, rather than as ill. The Enlightenment idolisation of reason then rendered society newly incapable of coping with the "unreasonable" in its midst, and so vagabonds, delinquents, and the mad came to be shut away in huge institutions. Of this mixed group, the mad alone were unable to fit into institutional life and so, through forming a residual problem population, became visible as a group for the first time. Following various inter-professional power struggles, the medical profession eventually gained authority over this group, who came to form "the mentally ill" as we know them today. If Foucault is right, then the mad have not always been seen as suffering from mental diseases. The reasons Foucault cites - that madness was not always seen as a bad thing, and that madness was not thought of as being a medical problem - are precisely the kinds of reasons that my account suggests should lead us to think of a condition as a non-disease. Thus his account is compatible with my own.

My account is also compatible with Laing's accounts of schizophrenia. Laing developed two completely different and influential accounts of schizophrenia during his career. First, with A.Esterson in *Sanity, Madness and the Family* (1964) he developed an account according to which, rather than there being something wrong with schizophrenics as individuals, there is something wrong with their families. According to Laing and Esterson the families of schizophrenics present them with confused and impossible demands. The schizophrenic in the family tries to make the best sense possible of an insane situation. Still, since you can't make a silk purse out of a pig's ear, the best sense possible isn't very good and so the schizophrenic ends up appearing to be insane. This account can be glossed as claiming that schizophrenics are not suffering from a disease because they do not require medical treatment - there isn't actually anything wrong with them as individuals. Again, this is the kind of reason that my account suggests should lead us to think of a condition as a non-disease.

Later, in *The Politics of Experience* (1967) Laing developed an account according to which schizophrenia is a mystical journey to a higher form of sanity. According to this account it is us "normals" who are truly alienated from ourselves. From childhood on we have been conditioned, first by our family, then at school, then at work, to act in ways that do not conform with our experiences, for example

[78] For example Weale 1999

we are trained to be polite to people who offend us. Under such pressures we create a false-self to present to the world. Schizophrenics are people who have refused to construct a false-self and as such are better off than the rest of us. Their experiences are part of a healing spiritual journey that can potentially lead them away from normality and into a higher form of sanity. This account is also compatible with my own. Laing can be understood as claiming that schizophrenia is not a disease because it is not a bad thing and, if this were so, I would be forced to agree with him.

My account is not compatible with Thomas Szasz's account of mental illness. In a series of influential publications, spanning from the 1960s to the present day, Szsaz has argued that mental diseases do not exist.[79] According to Szasz, talk of sick minds is merely metaphorical, in the same way as is talk of sick economies. Szasz claims that someone can only be said to have a disease, in the literal sense, if this disease is caused by some physical abnormality. Claiming that someone has a mental disease is taken to imply that they literally have a disease but that this disease has no physical basis. Thus Szasz concludes, while there may be brain diseases, there can be no mental diseases.

Szasz promotes his ideas as if they are extremely radical. His slogan that mental illness is a "myth" implies that psychiatrists are charlatans and/or agents of social control and, not surprisingly, psychiatrists have often been insulted by this. Still, I think that on the most plausible reading Szasz's claims turn out to be fairly moderate, and the disagreement between his account and my own will be slighter than might have been expected. Szasz accepts that if schizophrenia, depression, autism, and so on turn out to have some physical basis then they are real diseases. He just chooses to refer to any real diseases that have psychological symptoms as brain diseases rather than as mental diseases. As there is increasing evidence that a great many of the conditions that psychiatrists treat have some kind of physical basis, this means that Szasz will have to accept that much of the time psychiatrists treat real diseases.

Much of Szasz's anger has been directed at those who have claimed that what he thinks of as being symptoms of social discord, such as war, crime, and relationship problems, are in fact symptoms of mental illness. Such attempts to put medicine in the place of politics and ethics are dangerous, Szasz thinks, because their implicit denial of the importance of individual responsibility and of free-will is dehumanising. Here I can agree with Szasz. On my account social problems and problems in living are not diseases either, because they are not appropriately medically treated.

The only remaining disagreement between myself and Szasz concerns the possibility that some genuine diseases might have no biological basis. Szasz claims that all real diseases have a physical basis; I claim that it is conceivable that some diseases do not. For example, I think that quite possibly we will never be able to distinguish phobias from rational fears by looking at someone's brain, but that phobias can still be diseases.

[79] Szasz 1960, Szasz 2000

I shall try to make it plausible that Szsaz makes a mistake in claiming that all genuine diseases have a physical basis by showing that this claim does not actually satisfy the motive that lies behind it. Szasz's ultimate aim is to distinguish behaviours for which individuals should be held responsible from those for which they should not be held responsible. He thinks that the class of behaviours for which individuals should not be held responsible can be equated with the class of behaviours that are caused by physical abnormalities, and that diseases are conditions for which individuals cannot be held responsible. This leads Szasz to claim that all diseases have a physical basis. I suggest that Szasz has made a mistake and that it is in fact highly implausible to think that an individual is not responsible for some behaviour if and only if it is caused by a physical abnormality. On the one hand it seems that there are physical abnormalities for which I might be to blame - if I don't take my tablets, or if I shoot myself in the foot, then it can be my own fault if I am physically abnormal. Conversely, it is plausible that there might be involuntary behaviours that are not produced by physical abnormalities. Plausibly there are mental mechanisms that are not under our conscious control, for example at least part of our memory system. When something goes wrong with such mechanisms the behaviours produced might well be involuntary and yet mentally caused. As such, I suggest that the class of behaviours produced by physical abnormalities need not be the same as the class of involuntary behaviours. The motivation for Szasz's claim is thus lost, and there is no reason to think that diseases must have a physical basis.

4.IMPLICATIONS OF ACCOUNTS OF DISEASE FOR THE D.S.M.

Before examining the implications of accounts of disease for the D.S.M. it will be useful to review the argument of this chapter so far. In this chapter I have argued that it is neither a sufficient nor a necessary condition for something to be a disease that there be an evolutionary dysfunction. That something is an evolutionary dysfunction is not a sufficient condition for it to be a disease because some evolutionary dysfunctions, for example plausibly homosexuality, do not harm the dysfunctioning individual and these dysfunctions are not diseases. As such, biologically-based accounts of disease must be rejected.

The claim that diseases are harmful dysfunctions, a claim that is implicit in the definition of disease included in the D.S.M., must also be rejected. This is because it is not even necessary that there be an evolutionary dysfunction for a condition to be a disease. Some diseases may increase the inclusive fitness of an organism and in such cases there may be a disease but no evolutionary dysfunction. Having rejected these accounts, I have argued for a new account of disease according to which for a condition to be a disease it is necessary and sufficient that it be a bad thing, that the sufferer be unlucky, and that it be potentially medically treatable.

If the definition of disease used by the D.S.M. must be rejected, what implications does this have for the D.S.M? Does it imply that the D.S.M. includes the wrong class of conditions? I suggest that the implications for the D.S.M. are limited. The D.S.M. committee employed an account of disease according to which a disease is a harmful dysfunction. I have argued that this is the wrong account of disease and that instead a condition is a disease if and only if it is a bad thing to have, sufferers are unlucky, and it is potentially medically treatable. As they stand these two accounts of disease are quite different. However, it turns out that in order to be practically useful the D.S.M. account has to be revised, and the revised version is fairly close to the account I have been promoting. An account that claims that diseases are harmful dysfunctions is of little practical help in deciding whether particular conditions are diseases because in most cases we lack sufficient knowledge to know whether or not a condition is a dysfunction. As was seen in the earlier discussion of homosexuality, in many cases we just don't know whether or not a condition is biologically advantageous. As a result, the dysfunction part of the D.S.M. account can do little work. I suggest that in practice a condition is assumed to be a dysfunction if it is unusual and if it appears to be a biological or psychological problem. These proxy criteria would have seemed attractive to the D.S.M. committee because it is often assumed that the majority will function normally and that an evolutionary dysfunction will manifest itself at the biological or psychological level. These criteria, it turns out, are very close to my criteria that those who suffer from a disease should be unlucky and that diseases should be potentially medically treatable. Thus in practice it is unlikely that the dysfunction-criterion would have led the D.S.M. committee far astray.

The D.S.M. account and my own account both claim that diseases are bad things to have. I take it to be a consequence of this claim that one and the same biological condition can be a disease for some individuals and not for others (depending on whether it is a bad thing for the individual). At many points the D.S.M. takes the same line. Ego-dystonic homosexuality is a classic example of a condition that was only taken to be a disease so long as it was bad for the individual, although as discussed this diagnosis was dropped in 1987. Similarly, as we saw, the D.S.M. considers pedophilia to only be a disease when it is bad for the pedophile.

The account of disease used by the D.S.M. committee in practice, I suggest, was not far wrong. This being said, there may be reason to doubt the extent to which decisions to include particular conditions in the D.S.M. were influenced by accounts of disease. The A.P.A. archives contain files full of letters to and from the D.S.M-III committee. Many of these letters argue for the inclusion or exclusion of particular disorders. The archives contain letters arguing that disorders 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. However, there are no letters, either to or from the D.S.M. committee, that argue that conditions should be included because they are diseases or excluded because they are not diseases. This suggests that accounts of disease may have been little used in deciding the conditions to be included in the D.S.M. As I have argued, during the 1970s and 1980s, in public, the A.P.A. found defining "disease" a useful rhetorical strategy, but this is compatible with A.P.A. committees paying little attention to accounts of disease behind closed doors.

During the 1990s the A.P.A. begun to lose interest in defining "disorder" even for rhetorical effect. I have suggested that psychiatrists became interested in defining "disorder" during the 1970s and 80s because they needed to defend themselves from the claims of the anti-psychiatry movement and because they wanted to determine whether homosexuality is pathological. These concerns were peculiar to a specific time in American history and by the late 80s had largely disappeared. Right on cue the A.P.A. started to loose interest in defining "disorder". The introductions to the D.S.M-IV and the D.S.M.-IV-TR include a definition of "disorder" but add "no definition adequately specifies precise boundaries for the concept 'mental disorder'" and admit that "the definition of *mental disorder* that was included in D.S.M.-III and D.S.M.-III-R. is presented here because it is as useful as any other available definition".[80] These comments scarcely give the impression that the definition of "disorder" was considered of much importance by the committees responsible for these editions of the D.S.M.

There are some signs that interest in defining "disorder" is once again increasing. In 2002 the A.P.A. published *A Research Agenda for D.S.M.-V*. This brings together a series of "white papers", produced by committees of experts, that lay out some of the most pressing research problems for psychiatric classification. The first of these "white papers" is concerned with issues of basic nomenclature, and argues that a revised definition of "mental disorder" should be developed for inclusion in the D.S.M.-V. The committee think that such a definition is needed to justify why some conditions but not others are included in the D.S.M. in the face of "rising public concern about what is sometimes seen as the progressive medicalization of all problem behaviours and relationships".[81] Once again defining "disorder" has become a matter of political importance.

The A.P.A.'s interest in defining "disorder" varies with the political climate. However, I suggest that providing an account of disorder is always a matter of importance, whether this is recognised in particular periods or not. First, an account of disease can be helpful in determining which conditions should be considered to be diseases. As an example of a condition which has plausibly been wrongly included in the D.S.M. take hypomania. Hypomanic episodes are characterised 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."[82] 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. Such conclusions are of practical importance because many benefits and costs accrue to those who are considered to suffer from a disease.

Second, it is important to develop an account of disease because this is relevant to the discussion of various ongoing social and political problems. Take, for

[80] A.P.A. 1994 p.xxi
[81] Rounsaville 2002, p.3.
[82] A.P.A. 1994 p.336

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. Boorse, for example, argued that whether a condition is a disease is a matter of biological fact. On such an account of disease it will seem appropriate for experts in biology to tell us which conditions are diseases. In contrast, I have argued that whether a condition is a disease is in part a value-judgement. As doctors are not experts in making value-judgements, it follows from my account that it not appropriate for them alone to have a say in deciding 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 book.

CHAPTER 2

ARE MENTAL DISORDERS NATURAL KINDS?

Whether a condition is a disorder is partly a value judgement, but the distinctions between types of 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, thus whether a daisy is a weed is at least in part a value judgement. Still, the distinctions between kinds of plants generally considered weeds are fixed by the nature of the world. Botanical facts make it the case that daisies and thistles are genuinely distinct types of plant.

A fundamental assumption of the D.S.M. project is that empirical research can tell us how mental disorders ought to be classified. When the A.P.A. committees developed the D.S.M.-IV they reviewed thousands of empirical studies.[1] 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 this 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. Whether the correct classification of mental disorders should be dimensional or categorical, whether there are three or five different types of schizophrenia, and whether there is such a thing as caffeine withdrawal syndrome are all problems that it is thought could potentially be solved by empirical research.

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 A.P.A. 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 naturally similar to each other, and there is some explanation for this. Fleas, for example, are all similar in that they jump, drink blood, and are

[1] These studies are summarised in the D.S.M.-IV *Sourcebook*. Widiger et al. 1994, 1996, 1997.

poisoned by flea-spray, and fleas are alike in these respects because they are similar in some more fundamental way, plausibly because they are all genetically similar.

In this chapter I ask whether the D.S.M. committees are right to assume that types of mental disorder are natural kinds. This is an important question for several reasons. First, and as already mentioned, if mental disorders are natural kinds then this implies that the domain of mental disorders has a natural structure that it should be possible to discover via empirical research. In addition, whether mental disorders are natural kinds is important because natural kinds and natural laws are linked.[2] The behaviour of members of a natural kind is governed by natural laws. For example, it is a law that copper melts at 1083°C, and a law that Syrian hamsters have a sixteen day gestation period. As a consequence of being governed by laws, natural kinds can function in explanations (for example, "Miffy is afraid of dogs because she is a rabbit"), and support inductive inferences (for example, we can conclude that cow_n will eat grass like all other cows). As such, whether mental disorders are natural kinds matters. If mental disorders are natural kinds then there will be laws, explanations, and sound inductive inferences in psychiatry – in short psychiatry will be a genuine science. If on the other hand mental disorders are not natural kinds, whether psychiatry is a science must be questioned.

Throughout this discussion it should be borne in mind that the question of whether types of mental disease are natural kinds is completely distinct from the question of whether the super-category of mental disease forms a natural kind. The category "weed" is not a natural kind, but types of plant that are commonly considered weeds are natural kinds. I will argue that the situation with diseases is similar. "Disease" is not a natural kind – because whether a condition is a disease depends on whether it is bad thing. Nevertheless I will argue that many of the conditions that are generally considered diseases – tuberculosis, Huntington's chorea, and so on - are natural kinds.

The remainder of this chapter falls into three sections. The first assesses accounts of natural kinds. Various different accounts of natural kinds have been proposed, and whether mental disorders are natural kinds will depend on the account of natural kinds adopted. As such, before I can argue that some types of mental disorder are natural kinds, it is necessary to establish which account of natural kinds is correct. Once a satisfactory account of natural kinds has been outlined, in the second section I go on to refute arguments that purport to prove that types of mental disorder cannot be natural kinds. The arguments that I attack are philosophically more technical than the other material dealt with in this book. To readers who find this section difficult, I can only say that all three of the arguments I refute need refuting. All have been influential and have convinced some people that mental disorders cannot be natural kinds. As such, when asking whether mental disorders are natural kinds, there is nothing for it but to tackle these arguments head on. Finally, with an account of

[2] Classic formulations of the links between kinds, laws, explanations, and inductive inferences can be found in Quine 1969 and Nagel 1979, pp.30-1, footnote 2. For more recent discussion of these links see Bird 1998. It should be noted, however, that there is also a distinct Aristotelian tradition that thinks of talk about natural kinds as being important chiefly for debates about identity and change. See, for example, Lowe 2002.

natural kinds in place, and arguments that mental disorders are not natural kinds refuted, in the last section of the chapter I argue that some types of mental disorder actually are natural kinds.

1. ACCOUNTS OF NATURAL KINDS

Whether mental diseases can be considered natural kinds may well depend on the account of natural kinds adopted. As such, before going further it is necessary to ask which account of natural kinds is most plausible. Traditional accounts of natural kinds centre around ideas of "essences" or "essential properties".[3] Popular candidates for such essential properties are the atomic numbers of chemical elements and some kind of genetic property in the case of biological species. The essentialist claims that all members of a natural kind possess the same essential property. Thus, all samples of gold have an atomic number of 79, and all water is H_2O. The essential property fulfils two roles. First, possession of the essential property determines membership of the kind - to belong to the natural kind "gold" it is necessary and sufficient to have an atomic number of 79. Second, the essential property largely determines the other properties possessed by members of the kind - it is a lawful consequence of having an atomic number of 79 that a piece of material will be metal, will conduct electricity, will be solid at 20°C, will be largely inert, and so on.

Importantly for the essentialist, natural kinds provide a basis for our inductive inferences. So long as the background conditions are kept constant, this sample of gold will melt at the same temperature as other pieces of gold. This is because all members of a kind possess the same essential property, and the essential property lawfully determines the behaviour of the entity. It is because all members of a natural kind behave similarly that natural kinds are of interest to science. Measuring the melting point of *this* sample of gold is worthwhile because it provides one with information about all pieces of gold. Similarly, when a biologist dissects an organism they learn about the physiology of all organisms of that kind, not just about the individual.

In recent years traditional essentialist accounts of natural kinds have come in for fierce criticism. A major difficulty is that for biological species, which are traditionally considered amongst the best examples of natural kinds, no plausible candidates for the essences can be found. Several different criteria may be employed by biologists seeking to delineate species: morphological features, evolutionary lineage, the criteria of reproductive isolation, or genetic features. On examination none of these appear suitable candidates for being the essential properties of biological species.

In practice, most organisms are sorted into kinds on the basis of their morphological characteristics. Species X is known to have such and such wing markings, species Y has tail feathers shaped just so, and so the species to which individual organisms belong can be identified. Could morphological features, such

[3] For a recent defence of traditional essentialism see Wilkerson 1995. Other prominent essentialists include Kripke 1980 and Putnam 1970.

as possessing particular tail markings, serve as the essential properties of biological species? Saul Kripke presents the classic reasons for thinking not in *Naming and Necessity*.[4] For the sake of argument, suppose that tigers essentially are four-legged, large animals with yellow and black stripes. Then, argues Kripke, tigers would necessarily possess these features. However, in actual fact there are three-legged tigers, and albino tigers, and so we can see that our initial supposition was wrong and that tigers do not necessarily have to look like tigers. Nor is it sufficient for something to be a tiger that it looks like a tiger. Suppose, argues Kripke, that we discovered that some of the creatures we'd considered to be tigers were actually reptiles. Admittedly, this would be a highly surprising discovery, but then people were surprised to find out that whales are not fish. In this situation, Kripke says, we'd be forced to conclude that we'd made a mistake and that these reptilian "tigers" weren't tigers at all, rather they belonged to some other species, Fools' Tiger say. As looking like a tiger is neither necessary nor sufficient for being a tiger, morphological features cannot be the essential properties of biological species.

Nor can the essential property of a species be its evolutionary lineage. John Dupré shows this in his paper "Natural Kinds and Biological Taxa". Relationships of ancestry cannot be the essential properties of species because "Any sorting procedure that is based on ancestry presupposes that at some time in the past the ancestral organisms could have been subjected to some kind of sorting".[5] The point is that in order to make sense of claims such as "Cats are the offspring of cats, while dogs are the offspring of dogs" one must have some way of distinguishing the ancestor cats from the ancestor dogs. Relations of ancestry are only of any use once the parent organisms have been sorted into kinds. As such, sorting on the basis of ancestry must always be a secondary, parasitic method of sorting. When we are seeking the essential properties of species it is thus more appropriate to look to the basis of the primary method of sorting, whatever it might be, rather than to relations of ancestry.

For well-known reasons the criterion that members of a species be able to successfully interbreed will not do either. Some members of any species will be infertile and so unable to successfully breed with any other organisms. As such the criterion that members of a species be able to interbreed is too strong. It is possible to weaken the criterion so that infertile organisms can be accommodated. A revised criterion might require only that reproductive links exist between all members of the species. Infertile organisms satisfy this condition, as some member of the species must have given birth to them. Now, however, the revised criterion is too weak. There may well exist hybrid organisms that have been produced by matings between members of different species, and these individuals will now also count as members of the species. No criterion concerning interbreeding can be formulated that will both count infertile organisms as members of the species and also exclude hybrid organisms. Moreover, criteria concerning patterns of breeding are of no use when considering species that reproduce asexually.

[4] Kripke 1980 pp.119-121
[5] Dupré 1981 p.88

Given the current state of biology, genetic properties are left as the best candidates for being the essential properties of biological species. These properties seem appropriately theoretically important. However, as Dupré points out,[6] there are reasons for thinking that often there will be no one genetic property or set of properties shared by all members of a species. Most importantly, evolutionary theory suggests that it will be beneficial for there to be variation in the genes possessed by members of a species as this will facilitate quick adaptation if the environment changes. The existence of genetic diseases gives another reason for thinking that the genetic properties of members of a species will vary.

As no plausible candidates for the essential properties of biological species can be found, the claim that biological species have essential properties is thrown into doubt. Dupré concludes that as biological species are paradigmatic examples of natural kinds, and yet it is plausible that the members of a species need share no essential property, essentialist accounts of natural kinds must be rejected. [7] Instead he advances a view that he terms "promiscuous realism". Dupré has outlined promiscuous realism in a number of different works. The versions differ slightly, and it seems that over the years Dupré has become less promiscuous. Here I shall first outline Dupré's original account, as presented in his 1981 paper and 1993 book. Then I shall show why this original account is unacceptable, and go on to consider Dupré's more recent, revised account.

In his original account, Dupré asks us to imagine the individual entities of some domain (he considers biological organisms but his ideas can be generalised) mapped out on a multidimensional quality space. He claims that in such a map we would find numerous clusters corresponding to groups of similar entities. In many cases the clusters will not be discrete, but will be messy and hard to make out. Some clusters will correspond to traditional natural kinds – plausibly dogs will all cluster together, for example. At different levels of resolution different clusters will be discerned – as well as a cluster that corresponds to dogs, there will be finer clusters corresponding to dog-breeds, and, at a finer level still, to particular strains of pedigree dogs. Different clusters can also be generated by restricting our attention to particular dimensions of the map. If we restrict our attention to the dimensions that code for nutritional value, for example, we will find a cluster of things that are poisonous to humans. The task of the taxonomist is to pick out areas of relatively high density in the quality space. Dupré's account is realist because the clusters in the quality space reflect the real structure of nature. His account is promiscuous because many different classification systems can be extracted from the pattern of clusters in the space and none is privileged over the others.

Promiscuous realism salvages the idea that the divisions between classes of entities exist in an external world and can be discovered. Crucially, however, the distinction between natural kinds and groups of accidentally similar entities is lost. On Dupré's account any class of similar entities counts as a natural kind. But this is problematic. Consider the tins of tomato soup in Mr Smith's shop. These tins are all about three months out of date, all slightly dented, and all priced at 59p. The tins of

[6] Dupré 1981 pp.84-85
[7] Dupré 1981, 1993

tomato soup are similar to each other and so will form a cluster in the quality space. However, this cluster arises accidentally. There are no laws linking the fact that Mr Smith forgot to check the date labels, that the cleaner is heavy handed and knocked the tins over, and that the Saturday boy priced them all at 59p. In contrast the properties possessed by members of a natural kind, rabbits for example, tend to be lawfully linked. The properties of having long ears, being born blind, and being susceptible to myxomatosis are all found together for a reason.

The distinction between natural kinds and classes of entities that just so happen to be similar to each other must be maintained. It is only because the properties of members of a natural kind are lawfully linked that we can make inductive inferences about the members of natural kinds. It is because the properties of rabbits are lawfully connected that we can infer that anything that looks like a rabbit will also possess other rabbity features. Similarly, only natural kinds can support counterfactuals and function in explanations. As Dupré's clusters of similar entities merely exist as brute facts and need not be supported by natural laws, his account leaves our inductive and explanatory practices in limbo. For this reason, I suggest that Dupré's original account is an unsatisfactory alternative to traditional natural kind accounts and should be rejected.

Dupré thinks my tins of tomato soup example is unfair to him. He says that it is also part of his account that natural kinds should serve some kind of investigative or explanatory goal.[8] I don't think this comes over in the early versions of his account. In his 1981 paper Dupré says, "there are many sameness relations that serve to distinguish classes of organisms in ways that are relevant to various concerns… promiscuity derives from the fact that none of these relations is privileged". As an illustration he cites the texture of frogs' legs as being a quality that might interest gourmets.[9] In his 1993 book Dupré still holds that kinds might be distinguished on the basis of properties including those that are "economically useful or strikingly noticeable… [or]… of interest for further theoretical reasons",[10] which again seems to me too liberal. Kinds distinguished on the basis of properties that are merely economically useful or strikingly noticeable are little better than accidental kinds and as such will also often be incapable of doing the work required of natural kinds. Contrast a kind picked out on the basis of properties that are of economic significance, "clothes once worn by Princess Diana", with a kind that I would consider natural, "made of 100% cotton". Knowing that a piece of clothing was once worn by Diana tells one little about it – apart from giving an indication that it's likely to be worth money. In contrast, knowing that something is made of 100% cotton provides a host of information – it tells one what kinds of chemical and physical properties the cloth will have.

This being said, in some of his later work, Dupré is less promiscuous and suggests that kinds must provide a basis for scientific theorising.[11] So, we should turn to consider the question of what would happen if, to the account described here,

[8] Dupré personal correspondence 1999
[9] Dupré 1981 pp82-83
[10] Dupré 1993 p.113
[11] Dupré 2002

Dupré adds a condition stating that genuine natural kinds can be distinguished from accidental kinds on the basis that they serve an investigative or explanatory function. Then, I suggest, his account would become something like the account that I think is the correct account of natural kinds, to which we can now turn. The account of natural kinds that I shall outline and endorse isn't particularly novel – it really is just Dupré's account tweaked. My aim here isn't to provide an account that is excitingly new, but merely to provide an account that is right.

1.1 The Right Account

I suggest that the right account of natural kinds claims that members of a natural kind possess similar important properties. These important properties are important because they determine many of the other properties possessed by members of the kind. For this reason I will call them "determining properties". The determining properties of members of a natural kind must be similar, but not necessarily identical; thus this is not an essentialist account. The determining properties lawfully determine many other properties of the members of the natural kind. Of course, many of these laws will be ceteris paribus laws, that is they apply all other things being equal, and so background conditions will also be important. Still, as the determining properties of members of a natural kind are similar, so long as environmental factors are kept constant, members of a natural kind end up being similar in many respects.

To present the account somewhat differently, we can imagine all the entities in some domain plotted on a Dupré-style multidimensional map in which Dupré's quality dimensions are replaced by determining-property dimensions. For different domains different numbers of determining-property dimensions will be required. For chemical isotopes, for example, the necessary dimensions would plausibly be atomic number and neutron number. For biological species it is plausible that dimensions corresponding to various genetic properties would be required (or even more probably dimensions corresponding to particular genetic properties plus whatever environmental factors are important in determining how they are expressed). Members of a natural kind will cluster together in such a space.

I begun by thinking that the determining properties would always be microstructural, "underlying" properties, such as having an atomic number of 79. I am grateful to Dupré for pointing out to me that this might be needlessly restrictive. It is possible that in some cases determining properties might shape members of a kind "from above" rather than "from below". For example, prey animals might all be significantly similar because they have all evolved under similar pressures. In response to being hunted, they may all have evolved to become timid, have large litters, and blend in to their environment. In such a case they would have been shaped by a determining property that acts "from above". In his 1995 paper, "A different kind of natural kind", Crawford Elder develops in much greater detail the idea that the "members of some natural kinds may reliably present the same distinctive packet of observable properties, not because of anything that is distinctive about their insides, but because of the moulding of a common environment".

I claim that members of a natural kind all possess similar determining properties. Obviously similarity is a matter of degree. In saying that members of a natural kind all possess similar determining properties I accept that this implies that there will be borderline natural kinds whose members possess determining properties that are quite, but not very, similar. Plausible examples are higher level biological kinds, such as "plant" or "vertebrate". Whether or not a property is important enough to count as a determining property may also be indeterminate, and so there will also be borderline natural kinds whose members are similar with regard to some moderately important property. The kind "red things" might be an example. Red things have something in common, namely being red, and there are some laws that concern red things, for example "In standard conditions red things appear red to normal observers", and maybe "Red berries are normally poisonous". However, being red isn't a property that is lawfully linked to much else. It doesn't seem important enough to count "red thing" as a proper natural kind. Members of borderline natural kinds will be similar to each other in fewer respects than members of more typical natural kinds.

At this point my account may be clarified by briefly discussing those classes of entities that will not qualify as natural kinds. Classes of entities completely fail to be natural kinds when they are not actually similar to each other in any way. Thus the class of things on my desk almost certainly fails to be a natural kind because "being on my desk" is highly unlikely to be a genuine property. Genuine properties, such as possessing negative charge, endow entities with particular causal powers, and ground objective similarities. The pseudo-property of "being on my desk" does neither of these. It is worth noting that when entities are artificially produced this does not necessarily preclude them forming a natural kind. The "natural" in "natural kind" should be read as in "natural law" rather than as in "present in the Garden of Eden". Plausible candidates for artificially produced natural kinds include plutonium and nylon.

So far I have only considered cases in which the determining properties of some class of entities cluster in all dimensions of the determining-property space. However, there may also be more complicated shapes produced. Some classes of entities might possess sets of determining properties only a sub-set of which cluster. Consider, for example, a set of entities that are similar with respect to all but one determining property. In such a case a fuzzy line lying in the direction of the dimension representing the varying property would be found. If a set of entities are similar with respect to all but two determining properties then a fuzzy plane would be produced. Such sets of entities are like natural kinds up to a point; the entities will all be similar with regard to whatever properties are determined by their similar determining properties, and the kinds will support inductive inferences concerning these properties. As these kinds can do part of the work of natural kinds I propose to call them "partial kinds". I suggest that partial kinds are very common. Those chemical elements that form isotopes will all be partial kinds, as samples of the elements possess the same atomic number but different neutron numbers.

Sometimes it is assumed that natural kinds must be discrete, that is that any two kinds have natural boundaries between them and that intermediate forms do not occur.[12] I do not make this assumption. On my account, the determining properties do all the work when it comes to making inductive inferences and grounding explanations. It is because members of a natural kind all have similar determining properties, and the determining properties determine the other properties of the entities, that we can predict that all members of a natural kind will behave similarly. The "gaps" between natural kinds, where there are any, do no work. Thus there seems to be no reason to claim that natural kinds must be discrete, and abandoning this claim has the advantage that kinds that are not discrete, such as alloys, can be accommodated within a natural kind account. Alloys can plausibly be considered natural kinds, I suggest, because knowing that that a sample is a particular alloy is as useful, and useful in the same kinds of ways, as knowing that it is a 100% pure metal. Once one knows that a sample is a 40% zinc and 60% copper alloy one can predict how the sample will behave just as well as if one knew it to be pure copper.

I claim that natural kinds do not need to be discrete. In order to count as co-members of a natural kind, entities just need similar determining properties, and whether or not kinds are discrete makes no difference to their ability to fulfil this criterion. As a consequence, I think that debates as to whether mental disorders are separated by "zones of rarity" (that is, whether there are gaps between them in quality space) are not relevant to the question of whether they are natural kinds.[13] It might well be the case that types of depression and of anxiety disorder merge into each other, for example. Conceivably this might occur because the genetic bases of both depression and anxiety disorders are similar, if not identical, and similar environmental stressors are risk factors in both cases.[14] In such a situation, when plotted in a multi-dimensional determining-property space, cases of the disorders would not form distinct clusters. Still, cases of depression could form a natural kind in my sense – all cases might be fundamentally similar, and this might also be the case for anxiety disorders.

Robert Kendell and Assen Jablensky (2003) have recently claimed that psychiatric diagnoses can only be considered valid if a zone of rarity separates each disorder from others. They take "validity" to mean "well-founded…sound…against which no objection can fairly be brought",[15] which clearly implies that it would be a bad thing if psychiatric diagnoses fail to be valid. In contrast, I hold that the absence of zones of rarity would not be that important. Indeed, Kendell and Jablensky seem to admit as much when they say that even if diagnoses fail to be valid they may yet have "utility" and, furthermore,

> …provide invaluable information about the likelihood of future recovery, relapse, deterioration, and social handicap … guide decisions about treatment; and …provide a wealth of information about similar patients encountered in clinical populations or community surveys throughout the world – their frequency and demographic

[12] For example, Mill 1973 p.123, De Sousa 1984 p.565, Haslam 2002

[13] Haslam 2002 provides an example of someone who runs these two questions together.

[14] This example is taken from Kendell and Jablensky, 2003, pp.9-10.

[15] Kendell and Jablensky 2003 p.8

> characteristics, their family backgrounds and premorbid personalities, their symptom profiles and their evolution over time; the results of clinical trials of several alternative therapies; and research on the etiology of the symptom.[16]

Faced with this list, I suggest that an "invalid" diagnosis that has "utility" can give us all we might want. Disorders that are "invalid" in Kendell and Jablensky's sense, can still count as natural kinds on my account.

Where there are gaps between natural kinds, I claim that these gaps are of no metaphysical significance. However, I accept that when a domain is "gappy" this can be epistemically advantageous. In order to predict how an entity will behave we need to know at least roughly what sort of an entity it is. In gappy domains, the problem of identification becomes a multiple-choice problem (is this an X, or a Y, or a Z) rather than one with a potentially limitless number of possible answers. This can make correct identification easier.

Whether a domain is continuous may also make a difference to whether a dimensional or categorical classification system should be preferred. *If* maximal information retrieval is the sole aim of a classification system, then a dimensional classification system is best for a continuous domain, and a categorical classification system is best for a discontinuous domain. Following this reasoning, Kendell and Jablensky assume that if there fail to be zones of rarity between disorders then psychiatric diagnosis should employ a dimensional system.[17] This conclusion need not follow, however. In practice, classification systems do not only need to supply information about the classified entities, they must also possess other virtues, such as being easy to use. For this reason a categorical classification system may be used when classifying a continuous domain – as is the case with British degrees that are classified as being third, second, or first-class. Thus, even if there are no zones of rarity between mental disorders, a categorical classification system, such as the current D.S.M., may still be the best option. Whether a classification should be categorical or dimensional, and whether a domain is discrete or continuous, are distinct questions, and I claim that both issues are separate from the question of whether a domain consists of natural kinds.

The outlines of my account of natural kinds are now in place. The central claim of my account is that members of a natural kind all possess similar determining properties, where the determining properties of an entity are those properties that determine its other properties. On this account the links between natural kinds and natural laws can be easily understood. A determining property is a property that determines many other properties. As such, determining properties appear in many natural laws and so are the kinds of properties in which scientists are likely to be interested.

Although determining properties are like essential properties in some respects they are also importantly different. Essential properties and determining properties are similar in that both are said to determine many of the other properties possessed by an entity. Essential properties and determining properties differ in that the essentialist claims that all members of a natural kind must share some identical

[16] Kendell and Jablensky 2003, pp.9-10.
[17] Kendell and Jablensky 2003 p.8.

essential property, while I only claim that members of natural kind must possess *similar* determining properties.

It is worth noting that the account proposed here has some similarities to that proposed by Richard Boyd. Boyd argues that members of a kind possess a cluster of regularly co-occurring properties and that this is for a reason, there is some "homeostatic" mechanism that makes it the case that these properties re-occur. [18] Boyd's account works well for biological species. Members of a species possess clusters of co-occurring properties and this is as a result of homeostatic mechanisms, such as gene flow between the organisms and pressures that arise from the fact that all members of the species must survive in similar environments.

I think that Boyd's account is adequate for some natural kinds, and indeed his account can be accommodated by my own. I can say that it is because homeostatic processes act on them that members of a species possess similar determining properties. Still, I do not regard Boyd's account as suitable as a complete account of natural kinds. His account may work well enough for biological species, but it does not lend itself to dealing with other types of natural kind such as types of fundamental particle. There are no homeostatic mechanisms that make it the case that the properties of fundamental particles hang together. A particle with the mass of an electron might be negatively charged (and so be an electron), or it might be positively charged (and so be a positron). Fundamental particles are paradigmatic examples of natural kinds and any satisfactory account of natural kinds should accommodate them. For this reason I reject Boyd's suggestion that there must be a homeostatic mechanism that makes it the case that members of a kind possess a particular cluster of properties. On my account all that is needed is that members of a kind share a cluster of determining properties – whether there is a homeostatic mechanism behind the co-occurrence of the properties is unimportant.

To summarise this section: I have proposed an account of natural kinds according to which members of a natural kind possess similar determining properties. The determining properties of a member of a kind determine many of its other properties. As a result, members of a kind can be expected to behave similarly in similar circumstances.

2.ARGUMENTS AGAINST MENTAL DISORDERS BEING NATURAL KINDS

Various arguments have been put forward that purport to show that mental disorders cannot be natural kinds. These arguments all aim at showing that mental diseases cannot be natural kinds on the traditional, essentialist understanding of natural kind. Still the arguments, if sound, would show that mental diseases could not be natural kinds on my account either. When describing the arguments I have altered the terminology to make it consistent with that used in formulating my account (i.e. changed "essential property" to "determining property" where arguments against the existence of one will also be arguments against the existence of the other). This

[18] Boyd 1988, 1991

section is devoted to refuting these arguments that mental diseases cannot be natural kinds.

2.1 The Historical Argument

Some recent work in the history of medicine has aimed at showing 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 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.[19]

There are two possible ways of replying to such arguments and the appropriate response varies from case to case. In some cases, 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. Diseases that are artificially produced – in the sense of being produced by modern ways of living – such as types of drug addiction and, arguably, Post-Traumatic Stress Disorder could similarly be both artificially manufactured and natural kinds.

In other cases, one can argue that the disorder itself has not been artificially produced. Rather, the disease has always existed and it is only the means of *recognising* it that is recent. This is a plausible response when retrospectively the disease can be seen to have afflicted people throughout history. Some authors consider this to be the case with Post Traumatic Stress Disorder. For example, R. Daly 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.[20] Turning to more recent times, a 1956 textbook describes a condition highly reminiscent of Post Traumatic Stress Disorder called "traumatic psychoneurosis".[21] Given such evidence it can plausibly be claimed that people have

[19] Young 1995 p.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.

[20] Daly 1983

[21] Henderson and Gillespie 1956 p.207 The disorder is described as one in which "The [triggering] experience is nearly [always], if not always overwhelming... The symptoms that follow the fright are usually insomnia with terrifying dreams in which the patient wakes again and again; these dreams representing the accident in more or less distorted form. Anxiety symptoms occur during the day, especially lack of concentration or uneasiness of mind, and the bodily discomforts associated with anxiety, such as tremor or palpitation. Such symptoms may appear even in the most stable individual if the experience is severe enough..."

suffered from Post Traumatic Stress Disorder throughout history. On such a view, rather than Young having documented the social factors that led to the construction of Post Traumatic Stress Disorder he has just documented the social factors that led to the condition being diagnosed.[22]

Amongst some historians of medicine, however, it has become fashionable to argue that studies that seek to show that people in times past suffered from the same disorders that afflict people today are anachronistic. Both historians of medicine and transcultural psychiatrists often claim that people from other cultures or times cannot be said to suffer from the same diseases as twentieth century Westerners.[23] Andrew Cunningham presents the clearest argument for this position in his paper "Transforming plague: the laboratory and the identity of infectious diseases". Cunningham claims that the meaning of disease terms, such as "plague", is partly fixed by the ways in which the disease is identified. On such an account, as we identify plague with laboratory tests while people in earlier times identified plague by its symptoms, "plague" as it was used by historical figures and "plague" as used by us have quite different meanings. To avoid confusion we could write "plague$_m$" or "plague$_h$" to indicate whether the modern or historical meaning is being signified. If we accept Cunningham's claims, then as "plague$_m$" and "plague$_h$" have quite different meanings we should not talk of historical figures as having suffered from the same disease that afflicts people today; they suffered from plague$_h$ while we can only suffer from plague$_m$. If Cunningham is right, people from other cultures and times cannot have suffered from the same diseases that afflict people currently.

I am happy to accept Cunningham's claim that we should be sensitive to the meanings of disease terms as they were used by historical figures, but I will argue that he goes too far in claiming that we should not talk of historical figures as having suffered from contemporary diseases. My argument depends heavily on the distinction between terms appearing in "referentially opaque" and in "referentially transparent" contexts. In statements such as "Mary believes that x", "Mary hopes that x", and "Mary is afraid that x", the phrase x appears in a referentially opaque context. Characteristically, in such cases the truth value of a statement may alter when a term is swapped for another that refers to the same entity. For example, suppose Fred Bloggs is the masked man, but Mary does not know this. In such a case, "Mary is afraid of the masked man" may be true, while "Mary is afraid of Fred Bloggs" is false. In contrast, when phrases appear in referentially transparent contexts, they may be swapped for other terms with the same reference, and the truth value of the statement is always preserved. For example, if it is true that Bloggs has a broken arm and acne it will also be true that the masked man has a broken arm and acne.

[22] Young is, of course, aware of cases such as that described in the 1956 textbook, but he claims that they are not cases of Post Traumatic Stress Disorder. On this, however, reasonable people can disagree.

[23] An example of a transcultural psychiatrist holding these views can be found in Fernando 1991 pp. 131-132. Fernando complains that "The basic assumption underlying the IPSS [International Pilot Study of Schizophrenia] is concerned with the meaning of "schizophrenia": it is assumed to be an objective entity...and, moreover, an entity that is "present" in objective form all over the world with a universally similar, if not identical, meaning irrespective of culture."

Returning to Cunningham's argument, I agree with Cunningham that a consideration of actors' beliefs is important when dealing with terms as they occur in opaque contexts. Thus if we are trying to decide whether the statement "I believe I've got the Black Death" can be adequately translated by the statement "I believe I've got plague" the beliefs of the actor whose mental states we are trying to describe must be considered. Given Cunningham's evidence that "Black Death" was a disease identified by its symptoms while contemporary "plague" is a disease identified by its cause, I accept that the suggested description of the patient's belief should be rejected. Even if the historical patient uses the word "plague" rather than "Black Death", it would be misleading to describe his belief as being that he had plague. "Plague" as used by the patient is "plague_h" which, Cunningham has shown, has radically different implications from "plague" as we use the word.

I break company with Cunningham when it comes to talking of plague in contexts that are referentially transparent. I claim that statements such as "There was a lot of plague around in the 18th century", "Maybe the plague bacillus was more virulent in the past", and "Mr Smith died of plague in 1756" would, if true, be perfectly respectable statements. This is because the "plague" in such statements has our modern meaning, and as such refers to all cases of disease that are caused by the plague bacillus. The beliefs of the various sufferers are neither here nor there; all that matters is whether or not the bacillus was present in their bodies. Similarly, while a statement like "Priestley believed he breathed oxygen" is anachronistic, a statement like "Priestley breathed oxygen" is admissible. The difference is that in the first case we are trying to describe Priestley's mental states and so must use concepts with which he would have been familiar; whereas in the second case we are just making statements about the actual gas that he breathed. Everyone breathes or breathed oxygen regardless of whether they have or had any beliefs about it.

As such, Cunningham's claim that historical figures cannot be said to suffer from contemporary diseases should be rejected. Thus there is no reason why historical and transcultural studies that make this assumption should be considered illegitimate. In some cases such studies can give us reason to believe that a particular disease has occurred throughout history and that only the means for recognising it are of recent origin. For this reason, and because natural kinds can in any case be artificially created, I conclude that historical studies, such as Young's, do not show that a disease is not a natural kind.

2.2 Hacking's Argument

In a series of papers written between 1986 and 1995 Ian Hacking developed an argument that purports to show that types of mental disorder cannot be natural kinds.[24] Since then, Hacking seems to have changed tack and, although he does not give reasons for rejecting his earlier work, Chapter Four of his *The Social Construction of What?* (1999) raises the possibility that at least some types of mental disorder are natural kinds. Hacking discusses autism as a possible example. Here I

[24] Hacking 1986, 1988, 1992, 1995a., 1995b.

will be concerned mainly with refuting Hacking's earlier argument that mental disorders cannot be natural kinds. Although Hacking may have moved on, his argument has been influential and so is still worth considering.

In his earlier work Hacking introduces the term "human kind" to refer to the kinds of people - child abusers, pregnant teenagers, the unemployed - dealt with by the human sciences. "Human kind" is a term chosen by Hacking to contrast with "natural kind". Hacking argues that classifying and describing human kinds results in feedback that alters the very kinds under study. The resulting feedback means that human kinds have histories totally unlike the histories of natural kinds, leading Hacking to conclude that human kinds are not natural kinds. Hacking's case studies include Multiple Personality Disorder and autism, thus it is clear that he considers kinds of psychiatric patient to be human kinds.

Here I argue that Hacking's argument is flawed, and that he has failed to show that types of mental disorder cannot be natural kinds. The feedback that Hacking claims makes human kinds so very different from natural kinds is supposed to operate at two levels, a cultural level and a conceptual level. I will examine each type of feedback in turn, and show that in so far as feedback occurs it is compatible with human kinds being natural kinds.

2.2.1 Cultural Feedback

Feedback at the cultural level is dependent on the description of a kind of person entering popular culture. Often human kind terms carry heavy moral overtones; consider for example, "sexual pervert", "alcoholic", and "normal". Being classified in a certain way may also carry institutionalised benefits or costs. For example, students classified as being dyslexic may receive extra time in exams, and one may have to be certified "psychologically fit" before being employed in certain roles. As a consequence, people are motivated to attempt to alter the ways in which they are classified and, as their behaviour changes, so do the kinds under study. Consider, for example, the kind "obese person": The characteristics of both obese and non-obese people are affected by attitudes towards obesity. When obesity becomes stigmatised obese people will tend to become socially isolated and unhappy, and go on diets, while non-obese people will start making jokes about obesity and worry about becoming obese themselves. Attitudes towards obesity also result in new human kinds, such as "people with stapled stomachs", coming into existence. Hacking claims that the existence of *such* feedback shows that human kinds cannot be natural kinds.

J. Bogen has interpreted Hacking as claiming that human kinds are not natural kinds because the classification of human kinds results in feedback.[25] Hacking rejects such an interpretation. In any case, as Bogen points out, such an argument would fail because our classificatory practices also result in feedback that alters some natural kinds. For example, because marijuana is classified as illegal the plants are grown in attics and wardrobes altering their physical appearance. As another example, the characteristics of domestic livestock change over time because

[25] Bogen 1988

particular animals are classified as being the "Best in Show" and are used in selective breeding – sheep and pigs would now look very different if it weren't for our classificatory practices.

Hacking's argument that human kinds are not natural kinds must rest not merely on the fact that feedback occurs, but rather on the fact that it occurs in a particular way. The difference, Hacking claims, is that feedback in human kinds occurs because subjects become aware of the ways in which they are being described and judged.[26]

This idea needs working on before it can become an argument that human kinds cannot be natural kinds. As it stands Hacking has merely claimed that human kinds can be affected by a mechanism to which other kinds of entity are immune. Although this shows that there is some difference between human kinds and other kinds, it is not sufficient to show that this difference is of any fundamental significance. After all many other types of entity can be affected by mechanisms to which only entities of that type are vulnerable. While it is true that only human kinds are affected by the subjects' ideas, it is also true that only bacteria are affected by antibiotics, and that only domestic animals can be selectively bred. But no one would cite this as evidence that "bacterial kinds" or "domestic animal kinds" are not natural kinds.

The fact that only human kinds are affected by the subjects' ideas will only be a reason for thinking that human kinds are distinct from natural kinds if an extra premise is added to the effect that being affected by ideas is of greater metaphysical significance than being affected by, say, antibiotics. In places Hacking suggests that feedback caused by the subject's awareness of being classified is important because it results in feedback occurring at a faster rate than that which affects natural kinds.[27] The thought seems to be that the speed with which change occurs confounds our attempts to use human kinds in inductive inferences. Such a claim is questionable. Do human kinds really change more quickly than bacteria and viruses mutate? In any case, a difference in the rate of feedback is inadequate to mark a fundamental metaphysical distinction between human kinds and natural kinds. If it were true that the characteristics of human kinds shifted more rapidly this would imply that human kinds are not particularly useful natural kinds, not that human kinds cannot be natural kinds at all.

Alternatively, 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.

However, entities can be idea-dependent in two fundamentally different senses. And, as I will argue, idea-dependence in only one of these senses is indicative of

[26] Hacking 1997 p.15

[27] Hacking 1992 p.190 suggests feedback in natural kinds is different because "it works not at the level of individuals but through a great many generations, be it for microbes or mammals."

subjectivity. Compare two senses in which ideas of female beauty "affect" entities: In one case a woman, influenced by images of the "ideal female form", decides she is too fat and so slims. Her altered shape is idea-dependent in the sense that her ideas concerning her weight caused her to slim. The development of Concorde was dependent on the ideas of its developers in much the same kind of way; the developers had ideas about aeroplane designs, and these ideas feature in the causal history that culminated in the building of Concorde. Nevertheless, despite being in a sense "idea-dependent" the reduction in the woman's weight, and the building of Concorde, are both perfectly objective. Idea dependence of this type results in objective changes in entities and is compatible with a kind being objective.

On the other hand consider the case where we look at old photos of the first Miss World. Miss World looks rather plump and short by today's standards, nevertheless presumably at the time she looked fine. Miss World's looks are also idea-dependent, but this time nothing about the photo of Miss World has actually changed. Rather the ideas prevalent in popular culture have made the properties of the photo appear different solely by acting on the viewers. The change is a relational change only. Such relational changes indicate that a kind, such as "attractive women", is merely a subjective kind and so not a natural kind.

Hacking has shown that human kinds are idea-dependent. In order to show that this means that human kinds are subjective and so cannot be natural kinds, it would need to be shown that human kinds are idea-dependent in the way that produces relational as opposed to genuine changes. All Hacking's examples, however, seem to be of cases where ideas produce genuine changes in people's behaviour. Take, for example, the case of Multiple Personality Disorder.[28] When patients with personalities of the opposite sex and animal personalities started to appear on American chat shows, more and more patients started presenting with similar symptoms. The ideas prevalent in popular culture affected the symptoms typical of Multiple Personality Disorder. Still, here it seems that ideas about Multiple Personality Disorder caused a genuine change in patients' symptoms. Patients really did start barking. Such a claim need no more incriminate the kind "Multiple Personality Disorder" than the claim that changing views on animal welfare have resulted in fewer dogs having their tails docked incriminates the kind "dog". In order to show that the changes in the symptoms of Multiple Personality Disorder indicate that it is not a natural kind Hacking would need to show that barking, like beauty, is in the eye of the beholder, and he makes no suggestion that this is the case. Hacking's examples of cases in which human kinds are affected by ideas are all cases in which the ideas cause genuine changes. Such feedback is compatible with the kinds being natural kinds.

2.2.2 Conceptual Feedback

Hacking's argument for feedback at a conceptual level is dependent on Elizabeth Anscombe's account of intentional action. In her 1957 monograph, *Intention,* Anscombe considers the circumstances under which an action can be said to be

[28] Hacking 1995a.

intentional. Her solution is that an action X can be said to be intentional when the actor could respond to the question “Why are you doing X?” by giving a reason for acting. Suppose I lock my office as I leave, for example. If you ask me why I do this, I will tell you that I believe there are thieves about and I don’t want my things stolen. I give a reason for my action, which thus passes The Why Test and counts as an intentional action. In other cases I will not be able to answer the “Why?” question. Maybe I am not aware that I am doing X, for example I have accidentally stood on someone’s toe. Maybe I know I am X-ing but only because I have observed it, for example I am blushing. Maybe I know I am X-ing but the cause is presumed to be purely non-mental, for example I suffer from a muscle spasm. In these cases the behaviour does not pass The Why Test and is not an intentional action.

On Anscombe’s account an action is only intentional under a description because occasionally when we ask an agent “Why are you X-ing?” he may fail to recognise his action under certain descriptions. For example, I am in the kitchen X-ing, where X may be either “cooking” or “getting in the way of my flat-mates”. I recognise my action only under the description of “cooking”, as I have not noticed that I am getting in the way. The action passes The Why Test, and thus is an intentional action, only under the description of “cooking”.

Following Anscombe, Hacking uses the slogan “Intentional actions are actions under a description” in his argument that feedback occurs in human kinds:

1. Intentional actions are actions under a description.
2. Intentional actions make us the kind of person we are.

New descriptions allow new intentional actions which allow new kinds of person.

If Hacking is correct then the creation of new descriptions makes logically possible the creation of new kinds of person.[29] In creating new terminology the human sciences would make it possible for people to act in new ways. Here, however, I shall argue that Hacking’s argument fails because he has misinterpreted Anscombe’s phrase “under a description”.

The phrase “under a description” occurs throughout Anscombe’s monograph. However its use is idiosyncratic and in her 1971 paper called “Under a Description” Anscombe explains how she intended it to be understood. She writes, “under a description is ‘qua’...in modern dress”. Anscombe gives an example indicating that she uses “qua” in the usual manner, she writes “A may, qua B, receive such-and-such a salary and, qua C, such-and-such a salary.”[30]

If Anscombe in fact meant “Intentional action is only intentional qua some aspect” why did she use the misleading phrase “under a description”? Anscombe worked in the ordinary language tradition. She sought to gain philosophical insight from considering the ways in which we use everyday language. As such, her monograph aims to give an account of what we say about commonplace actions.

[29] That Hacking’s claim concerns logical possibility comes out most clearly in Hacking 1986.
[30] Anscombe 1971 p.208 in 1981 reprint

Within Anscombe's domain of the everyday the possibility of something being intended qua X where there is no description that refers to X does not have to be considered, as it is fair to assume that all commonplace intentions have already been described. Thus Anscombe can treat "under a description" and "qua" as equivalent.

In contrast, Hacking is interested precisely in the situations that Anscombe can ignore. Hacking wants to consider the new possibilities for action created by a new description; he needs to compare what was possible before the description was invented with what is possible after. In such cases the interpretation of "under a description" becomes key. Consider Ug the caveman, sitting in his cave at the dawn of time before language developed. According to Hacking, Ug cannot intentionally light a fire, go outside, or hum himself a tune - as there are no descriptions, Ug must wait for them to develop before he can intentionally do anything.

If, on the other hand, we take "under a description" to merely mean "qua", Ug is free to intentionally act in many ways. Ug can intend his banging flints qua a way to make a fire, rather than qua a way to make a noise. Although we cannot use The Why Test to find out what Ug intends to do, there are other ways in which we can decide what it is that he intends. We can consider Ug's probable motives: if it's cold he'd have reason to make a fire, if other people are banging drums he probably wants to make a noise. We can watch Ug's response when we intervene in his action - if he intends to light a fire bringing in wood will tend to make him smile, if he's starting a music session singing would probably be more welcome.

Such an approach fits in well with Anscombe's discussion of the intentional actions of non-verbal agents. In "Under a Description" she discusses a bird who lands on a twig that happens to be both covered in bird lime and near some seeds.[31] The bird, she says, lands on the twig with the intention of reaching a seed but not with the intention of landing in the bird lime. We infer the bird's intention by attributing intentions that are appropriate for the bird given its perceptual apparatus, its intelligence, and typical bird behaviour. We think that birds can identify seeds, that they get hungry, and that typically birds try to get seeds, and so we attribute the intention of getting the seed to the bird.

The problem of deciding how an action is intended arises because one bodily behaviour can help fulfil several different possible goals. Thus we cannot decide what someone intends merely by looking at their movements. Hacking presumes that the conditions under which an intentional action can be performed are identical to the conditions under which an observer can infer the actor's intentions. He sets about asking when intentional actions are possible via asking how an observer can determine what it is that an agent intends, and assumes that if one cannot tell what an agent intends then no intentional action is possible. This is only permissible if some verificationist principle is adopted. Even if such a principle is considered acceptable, however, if "under a description" is interpreted as "qua" there is no reason to think that intentional actions are logically dependent on the existence of descriptions. Asking an actor to explain his actions is one way, but not the only way, to discover what an agent intended. Using the method of asking the actor requires

[31] Bird lime was a sticky substance put on twigs. Birds that landed on it became stuck, and so could be caught.

descriptions, but as there are other means of inferring an actor's intentions which do not depend on descriptions, it cannot be concluded that descriptions are essential for intentional actions. Ug can intend to make a fire, and the bird can intend to land on the twig, without any descriptions being required. In such cases Hacking is simply wrong to claim that descriptions are required for intentional action.

Of course not all actions are so contingently linked to language. Consider the act of marrying someone, or the act of promising.[32] In order to marry a man one actually has to say, "I hereby take this man to be my lawfully wedded husband". Similarly, someone can only promise to do something if they say, "I promise to do X". Without the descriptions relating to marriage and promising, there can be no such actions. I suggest, however, that such actions form an unusual class. Such actions are peculiar in that they are defined in pseudo-legal ways, and the law, of course, unlike everyday thinking, dislikes ambiguity. It is extremely important to people that they have a way of being sure whether or not they are married, and of being sure when they have been promised something. That these actions are defined as being tied to the utterance of descriptions acts to reduce possible sources of doubt as to whether an intentional action has occurred or not. That one actually has to say particular sentences in order to get married makes it extremely unlikely that one could find oneself considered married by accident. In contrast, in everyday life we are able, and forced, to tolerate uncertainty, and accept conventions whereby we can say that this or that person intended to do X or Y even though there is a chance that we are wrong. In short while there is a class of pseudo-legal actions that are logically tied to their descriptions, such actions are only a sub-set of all actions. I can accept that the logical link between such actions and descriptions means that kinds such as "promisee" and "husband" will not form natural kinds, but still argue that no such logical link between actions and descriptions affects kinds such as "autistic person", "obese person" and "homosexual".

I accept, in addition, that there might be contingent links between descriptions and the ability to perform certain types of intentional actions. Some actions might be too complicated to perform without the aid of a description, for example, cooking certain complicated dishes might require a recipe describing what is to be done at each stage. It might also be true that actors only act in certain ways because certain descriptions exist in a culture, for example, it might be true that the existence of a tradition of limerick writing in a sense makes it possible for us to intend to write a limerick, as without the tradition no individual would ever think of doing such a strange thing. In such cases, however, our ability to perform certain intentional actions is only contingently dependent on the existence of certain descriptions. The descriptions in the culture may feature in the causal histories that culminate in our acting in certain ways, but they are not needed for it to be logically possible for us to act in these ways. Here Hacking's conceptual feedback collapses back into his cultural feedback and, as I have already argued, the existence of such feedback does not show that human kinds are not natural kinds. I conclude that Hacking's argument fails and he has not shown that human kinds are not natural kinds. Thus, despite Hacking's argument, types of mental disorder might be natural kinds.

[32] I am grateful to Martin Kusch for these examples.

As mentioned previously, Hacking himself has moved on since his work arguing that human kinds cannot be natural kinds. His earlier work still needed to be considered here, however, because once proposed arguments can take on a life of their own. While Hacking now accepts that types of mental disorder can be natural kinds, other people (convinced by Hacking's earlier argument) think they cannot. Still, now his earlier argument that human kinds cannot be natural kinds has been refuted, for completeness, I will briefly discuss Hacking's current views.

In *The Social Construction of What?* (1999) Hacking replaces talk of "human" and "natural" kinds with talk of "interactive" and "indifferent" kinds. Like the old human kinds, interactive kinds are affected by feedback that stems from the fact that the subjects classified are aware of how they are classified. In contrast, "indifferent kinds" are those kinds that are not aware of how they are classified. Hacking gives "quark" as an example, "the classification 'quark' is indifferent in the sense that calling a quark a quark makes no difference to the quark."[33]

The key difference between the claims made in Hacking's earlier work and in *The Social Construction of What?* is that Hacking now thinks that a kind can be both interactive and indifferent, or, using the old terminology, both human and natural. In Chapter Four, "Madness: Biological or constructed?", Hacking discusses childhood autism as a kind that is plausibly both interactive and indifferent. In an earlier essay, Hacking had shown that the symptoms typical of autism have plausibly shifted over time as a consequence of autistic people responding to the ways in which they are classified – autism can thus be considered an interactive kind.[34] At the same time, childhood autism is a disorder that will plausibly turn out to have some underlying biological cause. We can imagine that one day scientists will announce that they have discovered the abnormality that causes autism (whether it be genetic, neurological, or whatever), let us call it P. In such a scenario, Hacking suggests, the newspapers can fairly report, "Autism is P". P will be an indifferent kind, "the neuro-geno-biochemical state P is not aware of what we find out".[35] Thus autism might well turn out to be both an interactive kind and an indifferent kind. The challenge that Hacking sets himself is to show how this might be possible; how might a kind be both interactive and indifferent?

Hacking proposes a "semantic resolution" to his problem. This resolution makes use of the theory of meaning developed by Hilary Putnam.[36] Putnam suggests that we think of the meaning of a term as being a vector made up of syntactic markers, semantic markers, a stereotype, and the extension. The syntactic and semantic markers say what kind of a word the word is. For example, "water" is a mass noun and natural kind term. The stereotype is that which any competent speaker must know if they are to be said to understand the term. In the case of "water" one must know that it is thirst-quenching, colourless, in rivers, and so on. The extension is the class of things to which the term applies. In the case of "water" the extension is all samples of H_20.

[33] Hacking 1999 p.105
[34] Hacking 1995b.
[35] Hacking 1999 p.117
[36] Putnam 1975

Hacking suggests that we think of the meaning of "autism" as being a Putnam-style vector, but that in to this vector we put an enriched stereotype. The stereotype should include "the current idea of autism – prototypes, theories, hypotheses, therapies, attitudes, the lot".[37] The extension of "autism" will be the class of people with P. With this semantic apparatus in place, Hacking claims that we can understand how someone might write a paper titled "The Social Construction of Childhood Autism":

> The author could perfectly well maintain (a) there is probably a definite unknown neuropathology P that is the cause of prototypical and most other examples of what we now call childhood autism; (b) the idea of childhood autism is a social construct that interacts not only with therapists and psychiatrists in their treatments, but also interacts with autistic children themselves, who find the current mode of being autistic a way for themselves to be.[38]

According to Hacking, "autism" refers to P, which is an indifferent kind. At the same time the autistic children form an interactive kind – their behaviour is affected by the stereotype of autism, and, given time, the stereotype will have to be revised if it is to continue to describe them.

I agree with Hacking that a kind can be both interactive and natural. It is worth pointing out, however, that his account of how this can be the case differs from my own. Hacking thinks that the underlying pathology, P, is a natural kind that is unaffected by feedback. Whatever feedback occurs is thus limited. For Hacking, feedback can affect ideas about the kind. It can also affect the behaviour manifested by members of the kind (as the manifestation of the underlying disorder will be shaped by the social environment). But, feedback cannot affect the underlying pathology itself. Hacking thus makes room for interactive kinds to also be natural kinds via limiting the power of feedback. It is because Hacking thinks that feedback doesn't go "all the way down" that he can claim that there is an underlying, unchanging, biological, natural kind beneath the surface complexity.

I am happy to grant that some natural kinds of mental disorder may be as Hacking describes. To take an example, the content of the delusional beliefs of psychotic people is clearly socially influenced. Once people thought themselves possessed by demons, now they think they are controlled by C.I.A. agents. Still, plausibly, the same basic pathology underlies the delusions in both cases, and I am happy to say, along with Hacking, that this may form a natural kind.

Still, the difference between Hacking and myself is that I want to go further, and argue that even if the underlying pathological cause of a disorder is affected by feedback that disorder may still be a natural kind. I have claimed that natural kinds can be affected by our classificatory practices (for example, domestic animals are shaped by selective breeding). I have further argued that there is no reason to think that feedback that stems from the members of a kind being aware of how they are classified is of any greater metaphysical significance than any other kind of feedback. Thus, I think that even if people's ideas about how they are classified affect the basic pathology that underlies disorders (for example, because people start

[37] Hacking 1999 p.121
[38] Hacking 1999, p.121

using genetic engineering to alter the genetic bases of conditions) that these disorders can still be natural kinds. While I am pleased that Hacking has changed his mind and now thinks that interactive kinds can also be natural kinds in those cases where the effects of feedback are limited, I would urge him to go further and to concede that feedback that goes "all the way down" can also be compatible with a condition being a natural kind.

2.3 McGinn's Arguments

In *The Problem of Consciousness* (1991) Colin McGinn presents two arguments against mental or psychological kinds being natural kinds. McGinn is mainly interested in mental kinds of the type appealed to by folk-psychology - "beliefs that it is raining", "tingling sensations", "pains", and such like. Still, despite taking his examples from folk-psychology, McGinn takes his conclusions to rule out the possibility of there being any psychological kinds what-so-ever. As such, McGinn's arguments are relevant to the issue of whether types of mental diseases might be natural kinds. Here I shall refute each of McGinn's arguments in turn.

2.3.1 McGinn's Argument From Multiple Realisation

Many philosophers are attracted towards a functionalist account of mind.[39] Functionalists claim that mental states should be characterised in functional terms, that is solely in terms of sensory inputs, behavioural outputs, and relations with other mental states. On such a view, mental states can be multiply realised - that is the same mental state can be realised by different physical, or indeed possibly non-physical, systems. A human can have the belief that chocolate is the best flavour ice-cream, for example, and this functional state be realised by brain activity. A Martian could have the very same belief and this be instantiated by some configuration of the green gunge that fills Martian heads. In order to count as beliefs that chocolate is the best flavour ice-cream, all the states have to do is behave like beliefs that chocolate is the best flavour ice-cream (the state must prompt the believer to choose chocolate when given a choice of ice-cream flavours, and so on). Multiple realisation means that examples of the same mental state don't need to have similar physical properties – at the physical level, the Martian's and the human's beliefs have nothing in common.

This leads McGinn to doubt that psychological kinds can be natural kinds. Members of prototypical natural kinds have similar determining physical properties. All samples of gold are similar in having an atomic number of 79, all cats are genetically similar. In contrast, at the physical level, members of a psychological kind can be completely different. This means that if members of a psychological kind possess similar determining properties, these properties cannot be physical properties.

[39] McGinn himself is not a functionalist, but he presents this argument from the functionalist's point of view.

What about the possibility that members of a psychological kind instead possess determining properties that are functionally defined – that the determining property of a belief that chocolate is the best flavour ice-cream just is having a particular causal role? McGinn rules out this possibility on three grounds.[40] First, we must remember that the causal dispositions of mental states operate holistically. In other words, what a particular mental state does depends on the other mental states possessed by an agent. If you know that I am afraid of bulls, for example, you cannot simply conclude that when faced with a bull I will run. Maybe I have read that running from bulls antagonises them and that it's best to stick one's ground and look them in the eye. Because what a mental state does depends on the other mental states of the agent, McGinn suggests that mental states of a kind may fail to have a similar causal role.

Second, McGinn points out that determining properties are paradigmatically properties linked to the internal constitution of entities (for example, water is characterised as being H_2O, cats all have similar genetic properties). Functionally defined properties, on the other hand, are specifically not linked to the internal constitution of entities.

Third, McGinn claims that any specification of a mental state's causal role would be definitional and a priori. It is not an empirical discovery that those who believe chocolate is the best flavour ice-cream pick that flavour when given the option (all other things being equal), rather that they do this is true by definition. In contrast the properties that characterise natural kinds can only be specified a posteriori. Empirical work was required before we knew that all samples of gold have an atomic number of 79, or that water is H_2O, or that all cats are genetically similar.

If functionalism is correct, and many philosophers find it an attractive view, then psychological kinds cannot be characterised in terms of the physical properties they possess. However, claims McGinn, kinds characterised by functionally defined properties would differ so much from prototypical natural kinds that they would not be natural kinds at all. It follows, he thinks, that psychological kinds cannot be natural kinds.

Here I will not argue with McGinn's claim that natural kinds cannot be characterised in terms of functionally defined properties. I am not myself committed to this position, but am not sure how to argue against it. Instead, I will employ a different tactic. I will accept, for the sake of argument, that natural kinds cannot be characterised in terms of functionally defined properties, but will give two arguments that show that even if this is the case types of mental disorder could still be natural kinds.

First I will argue that McGinn fails to show that types of mental disorder cannot be natural kinds because it is consistent to be a functionalist about normal mental states while holding that types of mental disorder are not functionally defined. This is because systems that are functionally equivalent when they are working properly generally display different patterns of breakdown. Consider electrical resistors: These can be made out of many different materials, for now let us restrict our attention to carbon resistors and wire-wound resistors. When these resistors work

[40] McGinn 1991 p.134

properly they are functionally equivalent and either kind of resistor can be used in an electrical circuit. However, the different types of resistor can be broken in different ways. Carbon resistors are brittle and can be smashed. Wire-wound resistors are tougher but will eventually break at very high temperatures. Although "resistor" is functionally defined, different kinds of resistor breakdown in different ways, and so types of resistor-breakdown cannot be functionally defined.

On analogy with the resistor case, it should be clear that even if normally functioning mental states can be functionally defined, this does not imply that the same can be said for types of mental disorder. We can think of humans as suffering from particular types of mental disorder because of design weaknesses in the way that humans are made. So, to offer a couple of only moderately controversial examples, in humans a serotonin deficiency tends to produce feelings of misery, anhedonia and sleep problems, while a traumatic experience can produce flashbacks and nightmares. Now, as Martians don't have serotonin they can't suffer from a disorder that is caused by a serotonin deficiency, and as they are differently wired there is no reason to expect a traumatised Martian to suffer from nightmares. Disorders that arise from weaknesses inherent in the design of human brains will be specifically human disorders.

This being said, it is possible that in some cases the minds of Martians and humans might breakdown in ways that are functionally equivalent. To use the computer analogy popular with functionalists, mental disorders might be caused not only by problems with the mind's hardware but also by problems with the mind's software. Many theorists think that autism occurs when something goes wrong with a human's "Theory of Mind Module", for example. Now, while it seems to me unlikely, it is possible that for some reason all minds tend to be functionally equivalent at all levels of design. That is, maybe the best design for any mind is one that incorporates systems that are functionally equivalent to a human theory of mind module and to whatever other modules and sub-systems human minds possess. If this were the case then Martians would also have theory of mind modules and, when such modules breakdown, they would suffer from some of the symptoms typical of autism. Would such a disorder in a Martian be a case of autism? Here my second argument against McGinn comes into play. I will argue that it is consistent to take a functionalist approach to some mental state talk, but to reject functionalism as an acceptable account of mental state talk in technical contexts.

To expand, I suggest that the functionalist is plausibly right in saying that there is a sense in which aliens, or computers, could have mental states. A Martian might have a inner state that fills the same causal role that folk psychology attributes to beliefs about chocolate ice-cream, say, and in such a case it seems right to say that in a sense the Martian has a belief about ice-cream. However, in technical contexts, claims are made about mental states that go beyond anything known to folk psychology. A psychologist may claim that desires can be modified by Pavlovian conditioning, or that true beliefs can usually be extracted under the influence of amytal, for example. While the Martian's beliefs and desires may fulfil the causal role allocated to such states by folk psychology, they are unlikely to behave entirely as the academic psychologist expects. We might thus be left wanting to claim that (a) it is true that beliefs can be extracted under amytal, and (b) there's no reason to think that Martian beliefs can be extracted under amytal. I suggest that this apparent contradiction can be resolved if we recognise two senses of "belief". As "belief" occurs in commonsense contexts it is functionally defined. In such contexts a belief can be said to be any state that acts as folk psychology says beliefs should, and robots and aliens can have beliefs. However, when "belief" occurs in the mental sciences it should be taken as shorthand for "normal human belief", and such states are not functionally defined. The same goes for all mental states.

To return to the autism case, a Martian might suffer from a breakdown in their theory of mind module (supposing they had one) but I suggest they would not be said to suffer from autism. "Autism" is a term that is not part of folk psychology, but instead belongs to the mental sciences. As "autism" occurs in such sciences it is tacitly assumed that autism is a disorder of humans, not of Martians. As a consequence it makes sense for psychologists to search for the genes that cause autism, or to look for drug treatments that alleviate the symptoms of the disorder. I conclude that even if we are functionalists about mental state talk as it occurs in non-technical contexts, we should not be functionalists about mental state talk as it occurs in the mental sciences. In such technical contexts mental states are assumed to belong to humans. This implies that mental disorders are not functionally defined, and that McGinn has given us no reason to think that they cannot be natural kinds.

2.3.2 McGinn's Argument From Language

It has come to be generally accepted that our use of natural kind terms differs from our use of other terms in various ways. McGinn examines our use of mental terms, finds that this differs from our use of natural kind terms, and takes this to be a reason for doubting that mental kinds are natural kinds. After presenting McGinn's argument in greater detail I shall argue that he has not in fact shown that mental diseases are not natural kinds. This sub-section presupposes some familiarity with debates in the philosophy of language concerning the semantics of natural kind terms. Unfortunately, these debates are too complex for me to be able to outline them quickly here. Readers who have not heard of "twin-earth" are advised to accept my word that McGinn fails to show that types of mental disorder cannot be natural kinds, and to skip this sub-section.

From the literature on natural kind terms, McGinn draws up a list of nine characteristics of their use:

> (i) our initial criteria of recognition for membership in the kind are epistemically contingent; (ii) our original naive classifications of objects into natural kinds are susceptible of revision in response to scientific investigation of the kinds; (iii) there is the prospect of eliminating (ordinary language) natural kind terms in favour of nomenclature drawn from a scientific theory of the kinds; (iv) the equivalence relation that collects objects into a given natural kind is a theoretical relation; (v) we can construct plausible "Twin earth" cases for natural kind terms; (vi) the extension of a natural kind term is not fixed by the concepts speakers associate with the term ("meanings are not in the head"); (vii) natural kind terms exhibit a high degree of division of linguistic labour; (viii) a causal-historical theory of reference seems

> applicable to natural kind terms; (ix) the extension of a natural kind term is typically fixed by ostension (natural kind terms are indexical in some way).[41]

Our talk of mental states, claims McGinn, fails to display many of these features: Twin earth scenarios cannot be constructed for folk-psychological kinds; if, in another possible world, there are beings who display all the signs of believing that chocolate is the best ice-cream flavour, then even if they don't possess neurons, they really do have this belief. No one could convince us that we use folk-psychological concepts wrongly, as we are all experts in this area. As such, there is no division of linguistic labour, and folk-psychological terms could not be eliminated in favour of more "scientific" terms.

I do not wish to take issue with McGinn's claims regarding folk-psychological terms. Instead my criticism of his argument will focus on the way in which McGinn considers examples drawn from everyday mental talk and then takes his conclusion to be applicable to all mental talk what-so-ever. In taking his conclusions to have such broad scope McGinn has made a mistake. While I accept that everyday mental talk may be as McGinn claims, in line with my previous argument against McGinn, I will claim that our use of psychiatric terms, and indeed our use of terms from scientific psychology, is quite different

In contrast with our use of folk-psychological terms, our use of psychiatric terms displays precisely the features that McGinn considers to be characteristic of natural kind terms. It is common for lay-people to allow specialists to correct their use of psychiatric terms. Indeed popular magazine articles about mental illness regularly begin by chastising readers for using "depressed" as a synonym for "miserable", or for taking "schizophrenia" to refer to a condition in which someone has more than one personality. Psychiatrists are considered to be experts concerning the use of psychiatric terms. Moreover, as psychiatric knowledge increases classifications of mental diseases become vulnerable to revision. There is the prospect that ordinary language terms may be eliminated in favour of terms drawn from a scientific theory, and mental diseases can be classified on the grounds of theoretical relations.

Constructing a "Twin-earth" thought experiment for a mental disorder is rather difficult because while everyone agrees that water is H_2O the fundamental natures of most mental disorders are unknown. Still, it seems possible to construct twin-earth scenarios for those few diseases that are well understood. The twin earth thought experiment for water depends on the intuition that unless a liquid is H_2O then, even if it has all the superficial features of water, it isn't water. Similarly it is plausible that someone with the characteristics of Down Syndrome who had no chromosome abnormality wouldn't be said to suffer from Down Syndrome, and that someone can't be an alcoholic unless their condition is caused by alcohol. That a twin earth scenario can be constructed implies that a causal-historical theory of reference seems applicable to mental disease terms. This in turn implies that the extension of the kinds is not fixed by the concepts speakers associate with the term, and that the extension of the term is fixed by ostension.

[41] McGinn 1991 p.156

Thus I conclude that McGinn has been wrong in supposing that our use of all mental terms is like our use of folk-psychological terms. McGinn may be correct in claiming that our use of folk-psychological terms is unlike our use of natural kind terms, but our use of psychiatric terms is consistent with them being natural kind terms.

I have finally refuted all the 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.

3. ARE TYPES OF MENTAL DISORDER NATURAL KINDS?

Before considering whether it is plausible that some types of mental disorder are natural kinds it will be useful to remind ourselves of the account of natural kinds developed earlier in the chapter. I argued that the best account of natural kinds is one according to which members of a natural kind possess similar, although not necessarily identical, important properties. These important properties determine many of the other properties possessed by the member of the kind. As such, I called them “determining properties”. As members of a natural kind all possess similar determining properties they will have many other properties in common.

So far the examples of natural kinds considered - biological species, chemical elements, types of fundamental particle - have all been types of thing or stuff. Diseases should not be thought of in this way. True, some diseases are caused by entities (bacteria, viruses and so on) invading the body, but the disease itself should not be identified with these entities; if one has a colony of bacteria growing in a petri dish one doesn’t have a colony of disease, but only a colony of disease causing entities. Rather diseases should be thought of as types of process. A cold, for example, is a process that occurs when the immune system fails to fight off cold viruses and a particular sequence of typical symptoms results.

The account of natural kinds I have proposed can be readily adapted to deal with natural kinds of process. To cope with natural kinds of process the dimensions of the multidimensional determining-property space must be taken to represent properties-at-a-stage. Instances of a natural kind of process will then be close together in the space. 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.

For types of mental disease to be natural kinds the determining properties of instances of the disease must all be similar. 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 determining property, the defective gene, produces characteristic symptoms.

In addition to some mental diseases being natural kinds, it is plausible that some will turn out to be partial kinds (in the sense introduced earlier in the chapter). Diseases will be partial kinds where the determining properties of instances of a disease are similar in many, but not all, stages of the disease. To take a fairly well-understood physical disease as an example, a case of meningitis caused by alcohol and a case of meningitis caused by a viral infection have different causes although the remainder of the disease process is very similar. As another example, the set of determining properties of a particular case of A.I.D.S. might consist of infection with H.I.V., producing a reduction in the efficiency of the immune system, leading to infection with tuberculosis, leading to death. Again the determining properties of cases of A.I.D.S. will not be similar at all stages; sufferers will succumb to different infections. Where cases of a disease share some, but not all, determining properties the disease will not be a natural kind, but only a partial kind. Inductive inferences based on those determining properties that are similar in all cases will be sound, those that are based on determining properties that differ will not. Thus it is safe to infer that all A.I.D.S. sufferers will have low white blood cell counts, but not that all A.I.D.S. sufferers will develop red rashes.

It is plausible that some types of mental disorder are natural kinds, and that others are partial kinds. In addition there will almost certainly turn out to be some categories of mental disorder that are neither, that is mental disorders where cases do not possess any similar determining properties at all. Most obviously "rag-bag" diagnoses included in the D.S.M, 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. Many researchers hold that this is likely to be the case with schizophrenia, for example.[42]

To conclude this chapter, I have produced an account of natural kinds on which it is possible for types of diseases to be natural kinds. 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.

Many authors writing about kinds have worried about whether our classifications cut nature "at the joints".[43] On my account of natural kinds seeing the problem primarily in these terms is something of a red herring. The key worry is whether our classifications group together entities that are genuinely similar to each other. To stick with the meat metaphor, these days we know there are far worse things a butcher can do than cutting the joints poorly. If the meat we're given was originally some continuous piece of animal, cut at the joints or not, then we're doing quite well. The more worrying possibility is that our "joint" is made from reformed off-cuts, originating from numerous different beasts, and with water, rusk and additives thrown in. Similarly, we should primarily worry whether categories, such as

[42] *Schizophrenia Research* 1995 **17** pp.133-175 is devoted to the question of whether schizophrenia is a heterogeneous disorder.

[43] Haslam 2002 and Zachar 2000, for example, see the question of whether mental disorders are natural kinds in these terms.

"schizophrenia", group together cases that are actually similar to each other at a fundamental level, or whether we are lumping together cases that are fundamentally radically different. If schizophrenia fades into some other category, such as schizotypal personality disorder, then it will of course be as well to know this, but so long as cases of schizophrenia are fundamentally similar, schizophrenia is still a natural kind on my account. Even if schizophrenia fails to be discrete, knowing that someone suffers from schizophrenia could still be used as the basis of inductive inferences and function as an explanation.

This point can be made clearer by considering an analogy. British degrees are classified as third, second, or first class. There are two possible worries about this practice. The first, and radical worry, is that degree classifications might fail to group like with like. Marking might be fundamentally subjective. One marker might award a first, where another would award a third. If this were the case, degree classifications would be unsound. The second potential worry is that distinctions between degree classifications are artificial in the sense that they split a continuous range of student marks into arbitrary categories. Degree classifications fail to "cut nature at the joints". I suggest that this second worry is far less serious than the first. Admittedly dividing degrees into categories carries some risks – there is a danger that people come to believe that a second class and first class degree are radically different, when at least at the boundary they are not. Still, so long as marking is sound, all those who obtain a first are alike.

Whether and when it is a best to have a classification system that reflects the natural structure of a domain (that reflects the true joints, where there are any) will typically depend on a range of factors. It should be remembered that classification systems should not only provide information about the entities they categorise, but also need virtues that will enable them to be used in practice. In some cases it may be best to reflect the natural structure of a domain, in other cases it will be better to employ categories that make sharp divisions where naturally there are none. Debates over whether degrees should be classified, or raw marks recorded, illustrate that many factors may be involved in such decisions, that the factors to be considered will be particular to each case, and also, I suggest, that the issues will be largely empirical and pragmatic rather than philosophical. Here I have been concerned to show that in at least some instances it is plausible that all cases of a mental disorder are fundamentally similar. This implies that types of mental disorder can support inductive inferences, and function in explanations. They can be natural kinds on my account. Whether the best classification of mental disorders should be categorical or dimensional is a separate question from this, and not, I think, one that a philosopher can contribute much to answering.

Plausibly, I suggest, there are natural kinds of mental disorder. On my account of natural kinds, for a mental disorder to be a natural kind entails only that cases of that disorder are fundamentally similar. I take this to be a plausible, and fairly weak, claim. Despite this many people find it controversial. On several occasions when I have presented papers at conferences, people have come up to me afterwards and suggested that it is dangerous to hold that some mental disorders are natural kinds. Largely this seems to be because they think that claiming that mental disorders are natural kinds has unacceptable political consequences.

I accept that if types of mental disorder are natural kinds this may have political and social implications. Whenever it is discovered that some class of people behave in a characteristic way our reaction depends heavily on whether we think that these people form a natural kind. Consider the public reaction to statistics showing that children in some particular area do poorly at school and get into trouble with the police. We presume these children are fundamentally much the same as other children. We do not think that children in local authority X form a natural kind. As such, if children in local authority X are found to differ from other children in certain respects the cause of this difference is thought to lie in their circumstances rather than in them. Thus, statistics that show that such children do poorly are taken to show that there is something wrong with the local education system rather than that there is something wrong with the children.

In contrast, when it is reported that, say, seven-year old girls get better exam-results than seven-year old boys we think that there is at least a possibility that the cause of this difference lies in the girls themselves. Seven-year old girls and seven-year old boys are different kinds of person and so there is the possibility that their different results are the result of some intrinsic difference between them - maybe, for example, girls' brains mature faster than boys'. Often, of course, we won't actually be able to tell whether the difference is due to differences between the kinds of people or to a difference in circumstances. Thus, the difference between girls and boys may be due to girls' brains maturing faster, but then again it may be due to parents having greater expectations for girls, or to teachers spending more time with girls, or whatever. Still, that boys and girls are members of different natural kinds makes it possible that the inequalities between them are the result of their intrinsic differences. To put it crudely, when it is discovered that the members of a particular natural kind have different life-experiences from other people there is at least the possibility that this is no one else's fault. In contrast, if some disadvantaged group of people do not form a natural kind then it is likely that the cause of their differences lies not in them, but in their circumstances.

If types of people form natural kinds this has political implications as differences between members of distinct natural kinds may be due to intrinsic differences between them. These political implications are not as great nor as sinister as some have suggested, however. On occasion one is given the impression that those who believe that there are natural kinds of human have begun to travel down a short and slippery slope leading straight to fascism. In *The Disorder of Things*, for example, Dupré claims that when types of people are considered to form distinct natural kinds "it is inevitable that any systematic differences that are found will be taken to be explained, or explicable, in terms of the intrinsic differences between members of the two kinds."[44] This leads to the "legitimation of conservative politics and to the discouragement of proposals for significant social change".[45] Here Dupré is overstating the link. It is perfectly consistent to hold that men and women, say, form distinct natural kinds and to think that some of the differences between them are produced by sexist social structures. Thus someone who thinks that men and women

[44] Dupré 1993 p.253
[45] Dupré 1993 p.256

form distinct natural kinds might claim, quite sensibly, that men suffer from a far higher rate of testicular cancer because they have testicles, but also hold that sexism results in women being paid less, and sexually harassed more, than men. If types of people form natural kinds this opens up the possibility that differences may be due to their intrinsic natures, but it is by no means that case that all differences must be so explained. Similarly, it is possible to hold both that types of mental disorder may be natural kinds, and also to think that many of the problems faced by psychiatric patients are caused by social prejudice.

To sum up, there are plausibly natural kinds of mental disorder. As such, it is reasonable for psychiatrists to use empirical data in an attempt to find categories that group together cases that are fundamentally similar. The D.S.M. project of using empirical research to guide classification makes sense.

The upshot of the argument of the book so far is that we should think of mental disorders in a way analogous to the way we think about weeds. Weeds are unwanted plants, thus whether a daisy is a weed is at least in part a value-judgement. Still, types of plant that are generally considered to be weeds – daisies, buttercups, stinging nettles – are natural kinds. Similarly, I argue that the claim that schizophrenia is a disorder is in part a value-judgement, but that it may well be the case that schizophrenia and depression are natural kinds.

Still, there may be difficulties constructing a classification that reflects the natural similarities between types of mental disorders. In the next two chapters 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 D.S.M. is shaped by pressures emerging from the various ways in which it is used in practice.

CHAPTER 3

THE PROBLEM OF THEORY-LADENNESS

I have argued that at least some types of mental disorder may well be natural kinds. Still, it may be difficult to produce a classification system that reflects their natural structure. This chapter examines the potential problems for the D.S.M. if observation is theory-laden. The next chapter examines the ways in which the D.S.M. has been shaped by pressures that arise from the ways in which it is used.

If observations are affected by the observer's theoretical beliefs this can be expected to reduce our chances of gaining empirical knowledge. Theory-ladenness implies that we can never have direct access to the world, and that appeals to "observations", "experimental results", and "facts" can never be definitive. Whenever someone reports seeing something we will always be left with the possibility that another observer, armed with different theoretical beliefs, might have reported something quite different. As such, we will have less reason to believe our hypotheses to have been reliably confirmed, or falsified, by empirical data. The ultimate consequence is that we will have less reason to believe that our theories are true.

Theory-ladenness would cause the most problems where our theories are worst. Theory-ladenness implies that when an observer has a false theory their observations will be distorted by their false beliefs. Unfortunately it is likely that much current psychiatric theory is mistaken. There are many competing frameworks for understanding mental illness – biomedical, psychoanalytic, behavioural, relational, and so on. At many points these approaches are inconsistent, so not all of them can be correct. Thus many practitioners will hold false theories. Even within the biomedical framework, the approach that's arguably most closely linked with the D.S.M., competing theories abound. With few exceptions, the causes of specific mental disorders are contested. Again, this competition implies that the odds are that the theories that a psychiatrist holds are mistaken. If observation in psychiatry is theory-laden, then psychiatrists' false beliefs can be expected to distort their observations of their patients and prevent them from seeing the true similarities and differences between types of mental disorder. There will thus be reason to doubt that the categories included in the D.S.M. will correspond to natural kinds of disorder.

In addition to these epistemic problems, if observation is theory-laden there will be ethical and political implications, at least within the human sciences. People's lives are often affected by the ways in which professionals perceive them. If Mrs Jones is seen as deeply unhappy and as possibly suffering from clinical depression, and Mr Smith is seen as angry and potentially dangerous, then these judgements are likely to have practical consequences. If psychiatrists' perceptions of patients are affected by their theoretical beliefs (which may be false) then there is a danger that patients will be perceived inaccurately and treated wrongly as a result. What's worse, evidence from the history and sociology of science suggests that in many cases errors in theories will be systematic as opposed to random. All too often, when we look back at scientific theories that in their day were believed by good and clever people, we can see that the theories were systematically biased against the disadvantaged.[1] The theories of the eugenicists constitute only the most glaring example.[2] We have no reason to think ourselves better or cleverer than our predecessors. A pessimistic induction thus suggests that the theories that we believe today will also frequently turn out to be not only false but systematically biased against the less powerful. If our observations are theory-laden, all too often we will see others falsely and unfairly.

In addition to these wide-ranging epistemic and political problems, if observation is essentially theory-laden there will be problems specific to the D.S.M. The D.S.M.-III committee set out with the intention of creating an atheoretical classification system.[3] Mental health professionals operate within numerous different theoretical frameworks, and there were fears that a D.S.M. based on any one particular theory would alienate many practitioners. The D.S.M.-III committee sought to avoid this problem by basing the D.S.M. on no theory at all.[4] Instead they set out to produce a purely descriptive classification system that makes no reference to hypotheses regarding the aetiology of disorders. However, if observation is necessarily theory-laden then it is impossible to construct an atheoretical classification system. The aim of the D.S.M.-III committee will simply be unattainable.

Within the mental health literature the D.S.M.-III's claim to be atheoretical has come in for much criticism. Much sport has been derived from pointing out the various ways in which the D.S.M.-III has manifestly failed in its aim. In their review of the D.S.M.-III, Cooper and Michels point out that many of the diagnostic criteria included in the D.S.M. require fairly complicated and prima facia theory-laden inferences to be made.[5] They cite "identity disturbance", which is a key symptom of Borderline Personality Disorder, as an example. Cooper and Michels also note that the way in which diagnostic categories are grouped into higher-level categories is informed by theoretical commitments. For example, the D.S.M.-III considers "dysthymic disorder" to belong with the affective disorders, whereas some psychodynamically inclined clinicians would consider it more akin to the personality disorders. To add to Cooper and Michels observations, many D.S.M. diagnoses contain exclusion clauses, which are also clearly informed by theory. Agoraphobia, for example, can only be diagnosed if the symptoms cannot be better explained by a major depressive episode, Obsessive Compulsive Disorder, Paranoid Personality Disorder, or Schizophrenia. These disorders trump agoraphobia because the pathological process underlying them are thought to be more "serious" or "deep-

[1] See, for example, Bleier 1984, Keller 1985, Harding (ed.) 1993.
[2] Gould 1983 discusses these debates.
[3] A.P.A. 1980 p.7
[4] A.P.A. 1980 p.7
[5] Cooper and Michels 1981

rooted" than that underlying agoraphobia. Such judgements quite clearly assume a theory of mental disorders.

In addition to noting that the D.S.M.-III is not actually theory-free, it has become commonplace for commentators to point out that the possibility of constructing an atheoretical classification system can be doubted on the grounds that observation is in some sense theory-laden.[6] Under this barrage of criticism the claim to be atheoretical was dropped from the introduction to the D.S.M.-IV. Still, many of the sets of diagnostic criteria included in the D.S.M.-IV remain the same as in the D.S.M.-III, and new criteria sets follow the same style as those that have been inherited. As such, if the aim to be atheoretical led to mistakes in the approach of the D.S.M.-III these mistakes are likely to also be present in the D.S.M.-IV.

1.THE PROBLEMS OF THEORY-LADENNESS CLARIFIED

1.1 What Is A Theory?

The question of what counts as "theory" casts its shadow over the whole of this chapter. Providing an account of what makes something a theory could easily take a book in itself, and so will not be attempted here. In any case, prototypical theories are easy enough to recognise. They are characteristically acquired through learning or invention, and are vulnerable to replacement by successor theories. Thus we have Newton's theory of gravitation, psychoanalytic theory, and so on.

Philosophers writing about theory-ladenness have sometimes worked with a notion of "theoretical knowledge" that is far broader than this, however. For example, it seems that our perception of the world depends on innate, hard-wired brain mechanisms that process the raw sensory inputs. One such mechanism "recognises" the kinds of 2D projections that 3D objects produce on the retina and enables us to perceive the 3D object. Paul Churchland considers the operations of such mechanisms to provide an example of one way in which our perceptions are theory-laden.[7] In a similar vein, Karl Popper writes, "there is no sense organ in which anticipatory theories are not genetically incorporated".[8]

I think that Churchland and Popper have made a mistake in thinking that such mechanisms provide evidence of theory-ladenness. The existence of a hard-wired brain mechanism that "recognises" the kinds of 2D projections that 3D objects produce on the retina no more implies that a perceiver possesses a theory of 2D projections than the fact that an organism respires proves that it possesses theoretical knowledge concerning its need for oxygen. Generally it is true that biological mechanisms are such that much theoretical knowledge would be required to design an artificial replica. However, this does not mean that the organism itself possesses

[6] Faust and Miner 1986, Carson 1991 p.306, Millon 1991, Morey 1991 p.291, Goodman 1994
[7] Churchland 1988 p.170
[8] Popper 1972 ch.2 §18

theoretical knowledge. That the mechanism that recognises 2D projections is located within the brain makes no difference and should not mislead us.

Other writers have taken "theoretical beliefs" to include beliefs such as "There is an external world filled with physical objects" and "Other people have minds".[9] I'm not sure whether such beliefs should be classed together with the theoretical beliefs that I wish to consider here. It seems to me possible that such common-sense beliefs might differ from prototypical theories in being hard-wired, whereas prototypical theories are learnt or invented, and are vulnerable to replacement by successor theories. In any case, the fact that such common-sense beliefs are universally held means that their possession will not make some scientists observe things that others do not. As all scientists hold such beliefs their effects on perception (if any) will be uniform across all observers. As such, the discussion here will be restricted to considering whether observation is affected by more prototypical theoretical beliefs, such as "All masses fall at the same rate" or "Chronic schizophrenics often have flattened or inappropriate emotional responses".

1.2 Three Kinds Of Theory-Ladenness

Three distinct, but inter-related, claims can be teased out from the general thought that observation is in some sense theory-laden. First, it may be claimed that perception itself is theory-laden, where "perception" here refers to what an organism actually sees, hears, or senses in some other way. Perceptions are neither wholly determined by stimuli nor wholly determine observers' judgements of what they have seen; when looking at the duck/ rabbit picture, for example, two observers are presented with the same stimulus, one may perceive a rabbit while the other perceives a duck, and both may judge what they have seen to be an ambiguous figure. Those who hold that perception is theory-laden claim that when scientists view the same stimulus their theoretical disagreements may cause them to perceive different things.

Second, it can be claimed that the language in which observation statements are couched is theory-laden. This would imply that even if scientists holding different theories had the same perceptions they would produce reports of their perceptions that differ in meaning.

Third, it can be claimed that where scientists choose to direct their attention will be influenced by their theoretical beliefs about what is important. A psychoanalyst will choose to investigate childhood experiences, sexual fantasies, and so on, while a biologically-orientated psychiatrist will choose to investigate brain scans and neurotransmitter levels. As the two scientists seek out different stimuli they will end up making different observations.

If observation is theory-laden in any of these three ways there will be implications for the D.S.M. Although, as we shall see in the next chapter, the D.S.M. can be shaped by economic, social, and political considerations, to a large extent it is

[9] For example Quine 1960 p.22 "The positing of those extraordinary things [molecules] is just a vivid analogue of the positing or acknowledging of ordinary things: vivid in that the physicist audibly posits them for recognised reasons, whereas the hypothesis of ordinary things is shrouded in prehistory."

based on empirical research. The committees that decide the categories to be included in the D.S.M., and write the sets of diagnostic criteria that define these categories, rely on empirical studies to inform their decisions. This is directly the case when the committees review the empirical literature. It is still indirectly the case when they rely on expert opinion (as expert opinion will itself be based on knowledge of the empirical literature). If observation is theory-laden in any of the three ways outlined above, then this empirical literature will be questionable. One will be left with the possibility that if investigators with different theoretical beliefs had performed the studies different results would have been obtained.

In what follows, I will examine each of the three ways in which observation might be theory-laden in turn. In each case, my aim is to determine whether it is true that observation is theory-laden in the specified way, and if so the extent to which this is likely to reduce the chances that the categories included in the D.S.M. will correspond to natural kinds of disorder.

2. IS PERCEPTION THEORY-LADEN?

Here I take "perception" to refer to what an organism sees, hears, or otherwise senses. Philosophers brought up on the work of Thomas Kuhn and Norwood Hanson tend to believe that perception is theory-laden.[10] They hold that scientists with different theories see the world in different ways. The claim that perception is theory-laden has become so entrenched within the philosophy of science that the evidence supporting it is seldom subjected to scrutiny. Here, however, I will re-examine the evidence that has traditionally been offered, and argue that it is insufficient to demonstrate that psychiatrists' perceptions of mentally ill people are theory-laden.

Whether perception is theory-laden is commonly held to be an empirical question. As such, philosophers who hold that perception is theory-laden back up their claims by citing experiments that are supposed to indicate that what a subject perceives is affected by their expectations under certain conditions. Jerome Bruner and Leo Postman's anomalous playing card experiment and George Stratton's inverting lenses experiment are the classic examples.[11] Bruner and Postman asked subjects to identify briefly presented playing cards. Some of the cards used were anomalous, for example, a black four of hearts. It was found that subjects took an average of 28 milliseconds to correctly identify normal cards and 114 milliseconds to identify anomalous ones. Anomalous cards presented for shorter periods of time tended to be mistaken for normal cards. For example, someone shown a black four of hearts might report seeing a black four of clubs. The conclusion often drawn is that people see what they expect to see,[12] and that perception is thus influenced by theory. George Stratton wore an inverting lens over one eye for 87 hours over eight days. His other eye was covered, and when not wearing the lens he was blindfolded.

[10] Kuhn 1970, Hanson 1969

[11] Bruner and Postman 1949, cited by Kuhn 1970 p.113, Goodman 1978 p.14. Stratton 1897 cited by Kuhn 1970 p.112.

[12] For example Kuhn 1970 p.113

At first Stratton found that everything looked to be upside-down. He had problems guiding his actions and bumped into things. However, after a few days he was able to control his actions, and by the eighth day Stratton's world at least occasionally appeared normal to him. Thomas Kuhn takes this experiment to show that the way we see the world is not fixed; we can learn to see the world differently.[13]

It is debatable whether Bruner and Postman's experiment actually shows that perceptions of playing cards are affected by beliefs about playing cards, or whether Stratton's experiment shows that perceptions of the environment can be affected by beliefs about orientation.[14] Here, however, for the sake of argument, I shall accept that these experiments demonstrate cases of theory-ladenness but argue that there are problems with extrapolating from these experiments to claim that perception in psychiatry is theory-laden.

I will argue that it is unjustifiable to extrapolate from the experiments discussed above to the claim that perception in psychiatry is theory-laden by arguing that perceiving people is importantly unlike perceiving inanimate objects, such as playing cards. There are various ways in which this argument could be made. Hopefully, the relationship between a psychiatrist and a patient (even between a research psychiatrist and an experimental subject) is quite different from that between a participant in an experiment and a playing card. In the first case two human beings interact. In the second case a disinterested observer views a passive stimulus. On many accounts of perception, such as that put forward by J.J.Gibson, and those linked with the extended or embodied/embedded cognition movement, interaction is important for perception and this difference will be key.[15] I have some sympathy with these accounts of perception. However, they are controversial. While I am sympathetic to these accounts, those who are tempted to claim that perception in psychiatry is theory-laden need not be, and so I will not rely on them for my argument here. Instead, I will take a different tack, and argue that even *if* one thinks of a psychiatrist as viewing "stimuli" there is reason to think that perceptions of patients need not be theory-laden. I will support this claim via a consideration of neuropsychological evidence that suggests that different types of perception are dependent on different areas of the brain, and are probably processed differently. This means that it is possible that perception is theory-laden in some domains but not in others. In particular, it might be the case that our perceptions of playing cards are theory-laden, but that our perceptions of people are not.

[13] Kuhn 1970 p.112, Churchland 1988 makes similar use of this experiment.

[14] Gilman 1992 p.294, ftnt 4 suggests that the implications of the Bruner and Postman experiment are limited. Gilman also provides a detailed analysis of two other "New Look" experiments cited in footnotes by Kuhn (Bruner, Postman and Rodrigues 1951 "Expectation and the perception of color", and Hastorf 1950 "The influence of suggestion on the relationship between stimulus size and perceived distance"). Some experimenters have repeated Stratton's experiment and do not interpret the results as showing that the perceptions of the lens-wearer change. G.Brown (1928) and I.Kohler (1964) think the lens-wearer's world always appears distorted to them, but that after a while they get used to this and learn techniques to enable them to control their bodily movements. C.Harris (1963) holds that adaptation is proprioceptive not visual. Jerry Fodor (1988 p.193) suggests that even if the perceptions of the lens-wearer change, this need not be taken to show that perceptions are in general flexible

[15] Gibson 1979, for an outline of the extended cognition approach see Clark 1996.

Some of the strongest support for the claim that different types of perception are processed in different areas of the brain comes from clinical case studies of people who have suffered brain lesions. Depending on the location of the lesions, certain types of perception may remain while other types are lost. One of the most famous such cases concerns a patient, D.F., who suffered brain damage as a result of carbon monoxide poisoning.[16] D.F. is unable to discriminate size, shape or orientation and is thus unable to recognise objects, places, or people. Despite this disability, D.F. is still able to perform actions that require perceptual information. For example, although when she is presented with a letter-box type slot D.F. is unable to say what orientation it is in, if she is asked to insert her hand through the slot D.F. can reach for it with her hand correctly positioned. It appears that D.F. can make use of perceptual information to guide her actions, although she is unable to recognise objects. Conversely, some other brain-damaged patients have problems grasping objects, but are able to recognise them.[17] This suggests that the type of perception involved in recognising objects ("perception-for-recognition") and the type of perception involved in guiding actions ("perception-for action") may be processed in different parts of the brain.

Not only is it plausible that different types of perception are processed in different areas of the brain, there is the possibility that some types of perception make more use of "top-down" processing than others. In "top-down" processing, higher cognitive information is used in processing lower-level data. In sentence recognition, for example, it seems that information about context enables us to "hear" appropriate words even when the incoming noise is somewhat degraded. Processing is "bottom-up" if no feedback from higher to lower levels is involved. Perception can only be theory-laden if it involves top-down processing. This is because theory-ladenness requires theoretical beliefs (which rely on higher cognitive processes) to affect perceptions. This being said, "involving top-down processing" and "theory-laden" are not synonymous. It might be the case that perception makes use of some higher cognitive information that yet falls short of counting as theoretical knowledge. Such an account has been put forward by Jerry Fodor in *The Modularity of Mind* (1983). Fodor holds that some top-down processing is required for perception. There are brain mechanisms that enable 2D retinal images to be interpreted as 3D scenes, for example. However, the perceptual "module" that contains such mechanisms is still isolated from theoretical knowledge and so perception is not theory-laden on Fodor's account. That a type of perception involves top-down processing is necessary but not sufficient for it to be theory-laden.

Certain illusions are generally thought to occur as a side-effect of top-down processing. These include the Müller-Lyer illusion, in which the apparent length of lines varies with the arrangement of arrows at the ends of the lines, and the Titchener circles illusion, in which the apparent size of a central circle is affected by the size of the circles surrounding it. To examine whether perception-for-action involves top-down processing, experimenters have examined what happens when

[16] Milner 1997
[17] Milner and Goodale 1995 ch.4

subjects are asked to grasp the central line of a Müller-Lyer figure, or the central circle in a set of Titchener circles. If perception-for-action is not fooled by the illusions this would suggest that perception-for-action does not employ top-down processing, and so could not be vulnerable to theory-ladenness. Unfortunately, the results of such experiments have been mixed. In a widely cited study Aglioti et al (1995) found that when subjects reach to grasp the central circle in a set of Titchener circles their grasp aperture corresponds to the actual rather than to the perceived size of the target. This would indicate that perception-for-action does not employ top-down processing. Similar results were obtained by Haffenden and Goodale (1998), and, using the Müller-Lyer illusion, by Post and Welch (1996), and Otto-de Haart, Carey and Milne (1999). More recently, however, it has been suggested that these results were merely artefacts of the experimental set-up, and other experimenters, using slightly different set-ups, have found that perception-for-action can be fooled by illusory effects.[18] These results, in their turn, have also been contested, and recent review articles argue that the issue can only be decided via further research.[19]

Whether or not it turns out that perception-for-action is vulnerable to illusory effects, for our purposes the central message is clear: It seems there are different types of perception that are processed differently. This opens up the possibility that some types of perception might be theory-laden while other types are not. As such, it is a mistake to simply ask whether perception as a whole is theory-laden, rather one must specify the types of perception one is interested in and then review the evidence that is relevant to that case.

Perception-for-action and perception-for-recognition appear to be distinct types of perception. There are also perceptual systems dedicated to other tasks. It is likely that face recognition (e.g. recognising Tony Blair) is dependent on one such system. Evidence for this hypothesis comes chiefly from the clinical condition of prosopagnosia, in which patients are unable to recognise faces but can recognise other stimuli. As is so often the case, the evidence is not clear-cut. Prosopagnosia is a rare condition, often the damage to the brain is diffuse, and most patients suffer from other disabilities besides an inability to recognise faces. There is also some debate whether prosopagnosia should be considered as primarily a perceptual or a memory deficiency; it might be the case that patients can't recognise faces because they can't perceive them, or it might be because they can't remember what their friends and relatives look like. However, there is also other evidence that suggests that the perception of faces is significantly different from the perception of other stimuli. Behavioural studies have found differences between face and object recognition, for example, inverted faces are far harder to recognise than inverted objects.[20] Moreover, various brain-imaging studies show that face-perception and object-perception are processed in different areas of the brain in normal subjects.[21] When considered together the evidence strongly suggests that face recognition is processed by a dedicated system.

[18] Franz et al. 2001.
[19] Bruno 2001, Carey 2001.
[20] Yin 1969
[21] Biederman and Kalocsai 1997

There are also cases of brain-damaged people who can recognise faces but are unable to recognise facial expressions.[22] These people can recognise a photo as being of Tony Blair, say, but they are unable to tell whether he is happy, angry, or bored. This suggests that the perceptual-system that recognises facial expressions is different to that which recognises faces. The situation gets still more complicated, as the recognition of different emotions may depend on several distinct systems. Damage to the amygdala can result in a specific inability to recognise fear,[23] whereas people who suffer from Huntington's chorea can have specific difficulties recognising disgust.[24]

That different types of perception are processed differently in different areas of the brain is important for the discussion here because it opens up the possibility that some types of perception might be theory-laden while other types might not be theory-laden. Thus, rather than asking whether perception in general is theory-laden we must be more specific and ask whether a particular type of perception is theory-laden.

2.1 Is Perception In Psychiatry Theory-Laden?

Here I am interested in theory-ladenness in so far as it may affect the D.S.M. Given that different types of perception may be processed differently by the brain, any evidence suggesting that, say, perceptions of electron tracks are theory-laden will only be of peripheral interest. Instead it is necessary to consider specifically whether the types of perception involved in the collection of the data on which the D.S.M. is based are theory-laden. For the most part, the descriptions of conditions included in the D.S.M. 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, there is a series of studies that examine whether perceptions of facial expressions are theory-laden, which shed some light on this question.

2.1.1 Studies Of Emotion Perception

Whether our perceptions of facial expressions are theory-laden is relevant to evaluating the importance of theory-ladenness within psychiatry. Psychiatrists often make judgements about patients' emotional states, and these judgements will in large part be dependent on the psychiatrists' perception of the patients' facial expressions. In addition, psychiatrists have theoretical beliefs concerning the types of emotions particular types of patients may be expected to exhibit. Chronic schizophrenics are expected to show flattened or inappropriate emotional responses, patients suffering from depression are expected to be miserable, those suffering from mania to be cheerful or irritable, and so on.

[22] Young et al 1993
[23] Adolphs et al 1994
[24] Spregelmeyer et al 1996

Unfortunately, although there are some studies that examine whether perceptions of facial expressions are affected by our expectations, they are scarcer and harder to interpret than one might have hoped. A number of experimenters have examined the effects of contextual information on subjects' perceptions of emotions. In such studies, subjects are shown a face, for example a woman smiling, and are given information regarding the context, for example they might be told that the woman's son has just died, and then they are asked to judge the emotion that the person is experiencing. Such experiments are relevant to determining the extent to which our perceptions of emotions are theory-laden. If the contextual information is found to influence the subjects' perceptions this will be because they have certain expectations regarding the emotions that people will feel in certain situations, for example, people are expected to be sad on the death of a relative, happy when they've been given a present, and so on. Thus, if the subjects' perceptions are affected by the contextual information this will indicate that the perception of emotions is theory-laden, and if they are not this will suggest that the perception of emotion is not theory-laden. Unfortunately the results of experiments have been inconsistent. Fernández-Dols and Carroll (1997) review eighteen studies. Of these, seven found that subjects' perceptions were unaffected by the contextual information, while the rest found that the contextual information had some effect. As the results of the experiments are mixed, no conclusion can be drawn.

A possible weakness of the studies reviewed by Fernández-Dols and Carroll is that they lump together scores for emotion recognition that are achieved when viewing various different emotions. As mentioned earlier, there is some evidence that the recognition of different emotions may depend on different neurological systems. This would mean that our perception of some emotions might be theory-laden, while our perception of other emotions might not be theory-laden. There is already evidence that the extent to which our ability to remember facial expressions is theory-laden depends on the facial expression in question. Woll and Martinez (1982) found that labelling pictures with inconsistent labels, for example labelling a picture of someone smiling as "angry", affected subjects' ability to recognise the picture later if the emotion depicted was positive or neutral, but not when the pictures were of negative emotions. Woll and Martinez's results may be due to memory for various emotions being differentially theory-laden, rather than to perception of various emotions being differentially theory-laden, but they are at least suggestive. An ability for observers to accurately perceive negative emotions regardless of their expectations might be expected on evolutionary grounds; plausibly those humans who failed to recognise their neighbour's fear because they expected the Sabre Tooth Tiger to be safely asleep got eaten straight after their neighbour.

2.1.2 Summary

In this section I have shown that the evidence traditionally cited by philosophers in support of the claim that perception is theory-laden is unsatisfactory. Determining whether perception is theory-laden is a task for empirical science and is far harder than many philosophers of science have thought. There is evidence that different types of perception are dependent on different areas of the brain and may be processed in different ways. This opens up the possibility that some types of perception might be theory-laden while other types might not be theory-laden. The most plausible examples of theory-laden perception come from highly technical domains - where experimenters look at microscope slides, electron tracks, or sun spots, for example. However, even if such perception is theory-laden, it is possible that perception in other domains is not. Research psychiatrists will spend their time looking at a variety of different types of stimuli. Importantly, much of their time will be spent looking at patients Unfortunately, evidence relevant to the question of whether psychiatrists' perceptions of their patients are theory-laden is insufficient to allow a judgement one way or the other. Still, it is worth noting that on evolutionary grounds one might expect even those who lack any relevant theory to be able to directly perceive facts that are of relevance to survival. Thus it should come as no surprise if humans need a theory to recognise cells on a microscope slide, but are innately disposed to recognise emotions, or indeed, symptoms of illness. In the absence of conclusive empirical studies, however, the final conclusion of this section can only be that the question of whether our perceptions are theory-laden is not closed, as is often assumed, but on the contrary should continue to be a live issue.

Even if scientists with different theories perceive the same thing, however, it may be that as soon as they formulate observation reports theory-ladenness creeps in. To see whether this would be the case, we must turn to consider whether language is necessarily theory-laden.

3. ARE OBSERVATION REPORTS NECESSARILY THEORY-LADEN?

Many philosophers have claimed that the language in which observation statements are couched is necessarily theory-laden.[25] As a consequence, even if scientists with different theoretical orientations perceive the same thing, their observation reports will have quite different meanings. Regardless of whether perception itself is theory-laden, as soon as scientists try to communicate their findings problems with theory-ladenness would emerge.

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, suppose someone reports "Here is a glass of water".[26] This might seem like a straightforward observation statement. However, Popper claims that it is part of the meaning of terms such as "glass" and "water" that these are kinds of stuff that show law-like behaviour. If it turned out that the stuff in the glass could be ignited with a match, for example, then that would show that it was not water after all. Popper concludes that "Here is a glass of water" is not merely a report of what is seen, but assumes much theoretical knowledge.

[25] For example, Popper 1959, Kuhn 1970, Fleck 1979, Churchland 1988.

[26] Popper 1959 Ch.V §25. Fleck 1979 p.90 gives a similar discussion of the statement "Today one hundred large, yellowish, transparent and two smaller, lighter, more opaque colonies have appeared on the agar plate.".

3.1 Three Possible Ways Of Sidestepping The Problems Of Theory-Ladenness

3.1.1 Nagel's Suggestion

In his 1971 paper, "Theory and Observation", Ernst Nagel shows that the theory-ladenness of observation statements need cause no problems in practice, however. 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. Nonetheless, 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 also implies that scientists with different theoretical orientations can often still 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. To illustrate, suppose a psychoanalyst and a biologically-orientated psychiatrist are discussing a patient. One says "Mrs Jones has been crying". It can be accepted that this is a theory-laden statement that assumes, for example, that Mrs Jones is a human being with mental states rather than a cunningly constructed robot. Still, following Nagel, even if "Mrs Jones has been crying" is theory-laden the psychoanalyst and biologically-orientated psychiatrist can mean the same thing by the statement. Admittedly lots of their theoretical beliefs and assumptions will be different, but others will be the same. So long as the beliefs assumed by "Mrs Jones has been crying" are amongst those they share, they should experience no problems in communicating.

3.1.2 Using Other Forms Of Communication

Philosophers who argue that observation statements are theory-laden tend to assume that this implies that scientists with different theories will necessarily have problems communicating with each other. This need not be the case, however, because we have other ways of communicating with people apart from linguistically. It may turn out that these other forms of communication are not theory-laden. If so, it might be possible to employ non-linguistic forms of communication to side step any problems that might result from using observation statements.

To take an example, suppose our psychoanalyst and biologically-orientated psychiatrist are discussing a patient's symptoms. The biologically-orientated psychiatrist is about to describe the patient as being anxious, but then he remembers that this description may have a different meaning for his colleague than it does for himself. So, instead of describing the patient's symptoms to his colleague and risking misunderstanding, he invites the psychoanalyst to have a look at the patient, so that the psychoanalyst can see the patient's symptoms for himself. No observation statements are used by this method of communicating information, and so whether they are theory-laden is irrelevant. The biologically-orientated psychiatrist hopes that by showing the patient to the psychoanalyst he has ensured that both of them have the same information regarding the patient's symptoms.

Will the biologically-orientated psychiatrist succeed in his aim? Amongst other things this depends on whether perception is theory-laden. If perception is theory-laden then, as the psychoanalyst and the biologically-orientated psychiatrist have different theoretical beliefs, they will probably perceive the patient differently. As discussed in the previous section, at present there is insufficient information for it to be possible to judge whether perceptions of people are theory-laden. Still, until it is shown that perceptions of people are theory-laden, the possibility that the biologically-orientated psychiatrist has succeeded cannot be ruled out on the basis of the theory-ladenness of perception.

In addition, whether the biologically-orientated psychiatrist can get the psychoanalyst to see the same symptoms as himself depends on whether the two clinicians look at the same aspects of the patient's behaviour. People are complicated stimuli, and it is plausible that an observer cannot take in all aspects of their appearance and behaviour. When the psychiatrist invites his colleague to "see for himself" the psychoanalyst still has to decide whether he is supposed to be looking at the way in which the patient is shaking, at their facial expression, at their freckles, or at the way they've tied their shoelaces - amongst other possibilities. Now it is plausible that as human beings we naturally find certain features of a person's behaviour or appearance salient. For example, people normally notice when others have facial twitches or dodgy looking rashes. However, there may well be other aspects of the patient's appearance or behaviour that observers will not naturally find salient but that they may be primed to be alert to if they possess the right theoretical beliefs. Whether the scars on someone's wrists go across or up, for example, will probably only be noticed by someone who knows that slashing upwards is a lot more dangerous. As observers with different theoretical beliefs may well be primed to notice different things, the different theoretical beliefs of the biologically-orientated psychiatrist and the psychoanalyst may well result in them noticing different aspects of the patient's behaviour and appearance.

Thus, in many cases, if a scientist is to get a colleague with different theoretical commitments to notice the same features of reality as himself he is going to have to use language in order to specify what his colleague should look at. Rather than just saying "See for yourself" and leading his colleague to Mrs Jones, he's going to have to add a more specific instruction, such as "Look at the way she's walking". In addition, the patient herself is likely to tell the observers about some of her symptoms. However, if we take Nagel's suggestion on board, so long as the terms used in what is said are not amongst those that are laden with the theories under debate no problems should result. The language may not be theory-free but it can be theory-neutral, in that it is neutral between the theories about which the observers disagree.

The possibility of using non-verbal communication to minimise the problems posed by the theory-ladenness of language is particularly relevant in the case of psychiatry. In psychiatry there is a long tradition of showing patients to colleagues and students so that they can see them for themselves. In the past, and to some extent today, students and colleagues are shown patients on ward rounds and at case conferences. Nowadays, video-clips showing students what symptoms look like are shown in lectures. Indeed, in the early 1970s there were some suggestions that the written D.S.M. could be accompanied by a "library of audio-visual definitions tapes which will be a visual definition of the terms used".[27] The idea was that for each symptom or disorder there would be a "piece of behavioural recording on 16mm film or videotape in the American Psychiatric Association 'Bureau of Standards' from which we may judge a patient's degree of 'anxiety', 'manic behaviour' or 'la belle indifference'." Such a library was never actually produced. I am tempted to think that producing such a library now would be unfeasible. The D.S.M. is currently in such wide circulation that videoing a real individual and treating them as a prototype for some psychiatric disorder would be ethically problematic. No-one would want to become known to most of the world's mental health professionals as "Mr Anti-Social Personality Disorder" or "Miss Bulimia". For this reason, one possible means of side-stepping problems caused by theory-ladenness in psychiatric classification must probably be ruled out.

3.1.3 Adopting A Causal Account Of Reference

Some writers have suggested that if a causal account of reference is adopted then there is no reason to believe that observation statements are theory-laden.[28] Causal accounts of reference claim that the meaning of a term depends on the causal history linking uses of the term with an initial baptism.[29] In the case of proper names, the causal theorist claims that the name refers to the person who was originally baptised with the name; so "Rachel Cooper" refers to me because that's the name I was christened with. In the case of natural kind terms ("Great Crested Grebe", "Dandelion", and so on) the term refers to things of the same type as an originally named specimen. A key feature of the causal theory is that a speaker's mistaken beliefs about a person or natural kind have no effect on the term's reference. Even if I think St Nicholas owns a sleigh pulled by flying reindeer, so long as some causal history links my utterances of "St Nicholas" to an original baptism of "Nicholas", my utterances refer to the man St Nicholas. Similarly, a natural kind term continues to refer to things of the type baptised no matter how our theories may change. "Whale" continues to refer to whales despite the discovery that whales aren't fish. On a causal account, the extensions of terms are not affected by changes in the theoretical beliefs of language users. It is this that has led to claims that terms for

[27] Froelich 1972
[28] For example Fodor 1984 pp.27-30
[29] Causal accounts of reference are most closely linked with Kripke 1980 and Putnam 1970.

which a causal account is appropriate will not be theory-laden. In the last chapter I argued that at least some types of mental disorder are plausibly natural kinds. Many philosophers think that some causal account of reference will be appropriate for natural kind terms.[30] If so, and if terms for which a causal account is appropriate are not theory-laden, this would suggest that a theory-free D.S.M. is a real possibility.

However, in his book *Theory and Meaning* (1979) David Papineau argues that terms will be theory-laden even on a causal account of reference. Imagine, he asks us, that a group of travellers have discovered a new country. Their leader points at a native and says "Let's call this one 'Hamlet'". Now, Papineau claims, this on its own is not sufficient to ensure that it's a person that is being named. It needs to be specified what kind of "one" is here being dubbed. Depending on the type of "one" being baptised, the leader may be naming a person, a race, a species, or a skin-colour. Similar problems arise when the causal theorist attempts to name a natural kind or property. A chemist can point at a sample and say "Let's call this 'Flash Silver'", but unless she specifies what kind of thing she is trying to name it remains unclear whether "Flash Silver" is to be the name of a kind of liquid, of a colour, or of an element. In both cases the namer must make their intentions clear before one can know what it is that's being named.

Let's suppose that the namers make it clear that "Flash Silver" is to be the name of a kind of element and that "Hamlet" is to be the name of the race. Still problems can arise. Different scientists can have different ideas about what it takes for two samples to be of the same element, or for two people to be of the same race. As Papineau points out, early 20th century scientists disagreed as to whether the term "lead" applied only to stable matter with atomic weight 206, 207, or 208, or whether it also applied to radioactive substances with atomic weights 210, 211, 212, or 214. Here the argument was over what it took for something to be the same kind of stuff as the samples originally baptised "lead".

Different scientists can have different ideas about what it is for one kind of stuff, or property, or thing, to be of the same kind as another. When different groups have different ideas about such identity principles they will extrapolate from the dubbed original samples in different ways. As a result, theoretical disagreements about the identity principles for types of stuff can lead different groups of scientists to use terms like "lead" to refer to different things.

The reason why it seemed that a causal account of reference might imply that terms need not be theory-laden was that it was hoped that by pointing at a sample and naming it the reference of terms such as "lead" could be fixed independently of any assumptions about lead. The thought was that the original sample plus the concept of "same stuff" might be sufficient to fix the reference. However, because scientists with different theories may have different concepts of "same stuff" this will not do. Even on a causal account, theories are required to specify the identity

[30] The causal account of reference for natural kind terms has traditionally been linked with essentialist accounts of natural kinds. In the last chapter I argue that essentialist accounts of natural kinds should be rejected. However, I think that the account of natural kinds proposed in the last chapter is also compatible with a causal account of natural kind terms.

conditions for the types of thing being named. As such, terms remain theory-laden even on a causal account of reference.

At this point, however, Nagel's suggestion can be employed once again. We can accept that theories are required in order to tell us what the general identity conditions are for various types of thing. Still, so long as scientists in any particular dispute do not disagree about *these* theories, if a causal account of reference is adopted it seems that scientists with different theoretical commitments can mean the same thing by their observation statements. If, for example, two scientists agree about the general identity conditions for biological species they can still disagree about whether badgers spread tuberculosis. When they talk of "badgers" this may assume theoretical beliefs concerning the identity conditions for species, but as these beliefs are not in dispute, the scientists can still communicate.

3.1.4 Summary

To conclude: All observation statements are theory-laden. But, it is still possible for scientists with different theoretical commitments to mean the same thing by an observation statement. As Nagel points out, although observation statements assume theories, often the theories assumed by the observation reports will not be the theories that are in dispute. As such, scientists who disagree about the correct theory of depression may both mean exactly the same thing by the report "Mr Smith has lost weight", so long as they agree about those theories that are assumed by talk of weight-loss.

In some cases the observation statement made by a scientist will assume a theory that is under dispute, for example a biologically-orientated psychiatrist might report that a patient is anxious to a psychoanalyst. Still, it may be possible for the problems caused by theory-ladenness to be sidestepped. The psychiatrist may invite his colleague to come and see the patient for himself. Plausibly, he will have to tell his colleague what to look at. However, the psychiatrist may be able to guide his colleague to direct his attention at the features to be considered without using terms that are under dispute, for example, he may say "Look at the way Mrs Jones keeps fidgeting".

Adopting a causal account of reference may also help to side step the problems caused by theory-ladenness. As we have seen, terms are theory-laden even on a causal account of reference. Theories concerning the general identity conditions for different types of stuff are required in order to get naming off the ground. In general, however, these theories will not be in dispute. Again, often the theories that are assumed by the observation statements are not those at issue, and so scientists with different theoretical commitments will be able to communicate with each other. In general the theory-ladenness of language does not prevent scientists with different theoretical commitments from communicating, as 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 D.S.M. committee were wrong to describe their classification system as "atheoretical". Still, it may yet be possible for it to be neutral between the various competing accounts of mental disorder. If the D.S.M. can be neutral between different theories in psychiatry this will be a reassuring conclusion. The possibility that observation statements in psychiatry might be laden with psychiatric theory was especially worrying because there is reason to suspect that much psychiatric theory is wrong (if only because there are so many competing theories and they cannot all be right). On the other hand, if observation statements in psychiatry are laden with theories from some other and better established domain, for example biology, this is not so problematic, as we will have more reason to expect these theories to be correct.

Still, we have yet to consider the third way in which observation may be theory-laden, and the effect this could have on the D.S.M. In the next section I examine whether a theory is needed to determine which features of mental disorders are important enough to be worth studying, and consider whether theory-ladenness of this type might cause problems for the D.S.M.

4.PROBLEMS WITH DECIDING WHAT TO OBSERVE

Now we must turn to consider the third way in which observation might be theory-laden. Commonly it is claimed that phenomena are too numerous and too rich for a scientist to be able to set about observing everything. Rather, before being able to start collecting observations, the scientist must decide which features of the world are worth looking at. It is claimed this choice will invariably be informed by the theoretical beliefs of the scientist. For example, a psychoanalyst will choose to spend his time collecting data regarding the childhood experiences and fantasies of his patients, while his biologically-orientated colleague will spend time looking at brain scans and taking blood measurements. As scientists with different theoretical beliefs spend their time looking in different places it is only to be expected that they will end up seeing different things. Even team-based research can't avoid this problem. Team-work enables a group of scientists to examine more than would be possible for an individual researcher. Still, it is claimed, only a minute proportion of what could be looked at can ever be examined. Those who are 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 decide what to look at, there is a tradition in taxonomy that denies that this is necessarily 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.[31] Here I will examine the use of numerical taxonomy within psychiatry and consider whether the techniques employed allow classification systems to be constructed without a theory being needed to select the features of mental disorders that are to be considered important.

[31] For example Sneath and Sokal 1973 p.11

4.1 Cluster Analysis: The Techniques And Their Problems

Numerical taxonomists employ various statistical techniques to construct classification systems. Some of these techniques lend themselves to the construction of categorical classification systems (that is classification systems with discrete categories, such as the D.S.M.); other techniques are best used to construct dimensional classification systems (that is classification systems with dimensions, such as classifications of personality types that have dimensions of extroversion-introversion etc.).

"Cluster analytic" techniques are those most often used in the creation of categorical classification systems. These methods group entities into classes on the basis of their average similarity to each other. The intuitive idea of cluster analysis can be grasped by imagining data on many variables of the entities being analysed 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.

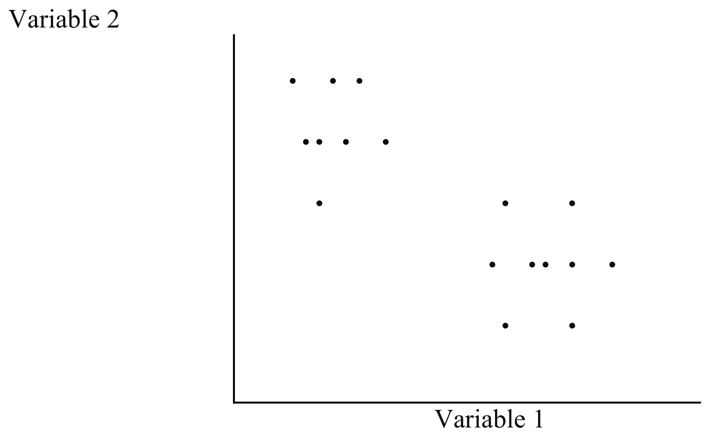

Figure 1. Two dimensions of a cluster solution showing two clusters

It is difficult for cluster analysts to achieve robust cluster solutions because the solution achieved is sensitive to decisions made at several stages in the clustering process. If different variables are measured, or a different sample of entities is studied, different cluster solutions may be obtained. Obviously, if the variables needed to characterise a cluster are not analysed that cluster cannot be found. For example, studies of depression employing purely cross-sectional variables are incapable of finding clusters of bipolar and unipolar depression, as these would be distinguished by differences in the history of the illness.[32] Similarly, excluding a sub-population from the sample will limit the clusters that can be obtained. For example, many studies of alcoholism have used male inpatients from U.S. Veterans Administration hospitals. As most of the subjects were middle-aged Vietnam veterans, there was no possibility of finding clusters of characteristically female or young alcoholics.[33]

Most clustering methods begin by calculating a matrix of similarities between entities. There are many different measures of similarity (Roger Blashfield writing in 1984 counts over fifty[34]), and the cluster solution eventually achieved may be sensitive to the measure used. The results of a cluster analysis also vary with the clustering method employed. There is as yet no consensus concerning the mathematical definition of "cluster" and many techniques (Blashfield counts 150[35]) have been developed. For example, some methods add an entity to a cluster on the basis of it being highly similar to a single member of the cluster, some methods require that an added entity be highly similar to all entities in the cluster, and some methods seek out regions of the multidimensional plot which have a high density of points. Especially when the data analysed lacks a well-defined structure the different methods may produce different solutions.

Different clustering solutions can also be produced using the same method. Regardless of the structure of the data, clustering methods either start with all the entities considered separately and finish with all the entities in one cluster, or vice versa. It is left to the analyst to decide which, if any, of the hierarchy of clustering solutions is acceptable. Various mathematical measures and clinical judgement are usually used in deciding which is the best solution produced.

The problems caused by the sensitivity of cluster solutions and ways of getting round these problems have long been recognised in the psychiatric literature.[36] It is generally accepted that the difficulties can be minimised if investigators reproduce their solutions using a second sample (or sub-sample of the original sample) and using a second clustering method. Analysts may also repeat the analysis using a second set of variables or a sub-set of the original variables. In addition, it is accepted good practice to attempt to "externally validate" clusters, that is to show that they are predictive of significant differences in variables that were not employed in the analysis. For example, clusters of types of depression constructed from an analysis of symptoms might be externally validated by showing correlations with responses to drug treatment.

4.1.1 Cluster Analysis Within Psychiatry

The rhetoric employed by the D.S.M.-III committee and by the early proponents of cluster analysis is strikingly similar. Biologists first developed modern cluster

[32] As noted by Blashfield and Morey 1979
[33] As noted by Skinner 1982
[34] Blashfield 1984 p.230
[35] Blashfield 1984 p.217
[36] For example Everitt 1972, Everitt 1975, Everitt et al. 1971, Strauss et al. 1973, Blashfield 1980

analytic techniques in the late 1950s.[37] The "Numerical Taxonomists" rejected attempts to construct classification systems based on evolutionary descendence on the grounds that in most cases it is impossible to determine the ancestral relationships between taxa. Instead they tried to develop means of classifying organisms based solely on the degree of similarity of their present characteristics. The numerical taxonomy movement in biology made much of the supposed "objectivity", "empiricism", and "naturalness" of the classes produced. Similarly, the D.S.M.-III committee called for a rejection of theory-based classification on the grounds of the paucity of theoretical knowledge. Like the Numerical Taxonomists, they also aimed at a classification system constructed on empirical, atheoretical grounds.

The apparent affinity between numerical taxonomy and the D.S.M. approach has been noted in the psychiatric literature. William Corning and Richard Steffy (1979) note that cluster analysis was developed by biologists in an attempt to overcome precisely the problems caused by a lack of theoretical knowledge that were perceived to be plaguing psychiatric classification.[38] Meehl (1986) considers the idea that a D.S.M. committee truly committed to the creation of a purely descriptive taxonomy should "have proceeded by applying an appropriate formal cluster algorithm to a huge batch of carefully gathered clinical data, 'letting the statistics do the whole job for them'".[39] The idea that cluster analysis might be suitable for constructing atheoretical classification systems would thus almost certainly be familiar to the committees developing the D.S.M.

Cluster analytic studies of mental illness only start appearing in any numbers in the late 1960s when computers became increasingly available. Thus when the D.S.M.-I was published in 1952 very few cluster analytic studies existed. However, cluster analytic studies did have some influence on the D.S.M.-II, published in 1968. In particular, the D.S.M.-II section for childhood disorders was heavily based on a 1966 cluster analytic study by Richard Jenkins. Categories for "hyperkinetic reaction", "withdrawing reaction", "overanxious reaction", "unsocialised aggressive reaction", and "group delinquent reaction" correlate with clusters in Jenkins' study. Only the category "runaway reaction", for children who run away from home, was added.[40] Methodologically, Jenkins' study is not impressive. Jenkins makes no attempt to check his cluster solution by using a second method, or a second sample, or by seeing whether the clusters correlate with variables not used in the analysis. Jenkins did have a seat on the D.S.M.-II committee, however - which doubtless goes some way to explaining the influence of his study.[41]

I have found no evidence that the D.S.M.-III committees were influenced by cluster analytic studies. Indeed by the early eighties some of the one-time proponents of cluster analysis had become disappointed that cluster analysis had not proved as influential as they had hoped. Writing in 1982, Harvey Skinner and Roger

[37] Sokal and Sneath 1963 is the key early text outlining the use of cluster analysis in biology.
[38] Corning and Steffy 1979 p.296
[39] Meehl 1986 p.224
[40] A.P.A. 1968 pp.50-1
[41] A.P.A. 1968 unnumbered first page

Blashfield attribute the failure of cluster analysis to methodological worries and a lack of "salesmanship" on the part of authors.[42]

Blashfield tries to explain the perceived failure of quantitative approaches in psychiatric nosology more fully in *The Classification of Psychopathology* (1984). Here he considers an understanding of inter-professional rivalry between psychiatrists and psychologists to be key to understanding the relations between the D.S.M. and cluster analysis. According to Blashfield the orientation of the D.S.M.-III was heavily influenced by a small "invisible college" of psychiatrists he terms the "Neo-Kraepelinians". Characteristically the Neo-Kraepelinians were keen to operationalise psychiatric diagnostic criteria, were deeply concerned with the reliability of psychiatric diagnosis, and strictly adhered to a medical model of mental disorder (that is they believed that mental diseases are fundamentally diseases like any other, and that psychiatry is a branch of medicine). When Robert Spitzer, a key member of this invisible college, became chair of the D.S.M.-III committee he was allowed to appoint its other members. For the most part he filled the positions with people who shared his theoretical outlook, and thus the Neo-Kraepelinians gained control of the D.S.M.

Medical models of mental illness, such as that adopted by the Neo-Kraepelinians, are sometimes considered a threat to the status of non-medical mental health workers. The fear is that if mental disorders are medical problems then this may imply that only medical doctors should treat them. According to Blashfield, and as previously discussed in Chapter One, the association of the D.S.M. with the medical model resulted in the D.S.M. being perceived as a threat by some psychologists.[43] Part of the American Psychological Association's response to this perceived threat was to try to develop its own, alternative classification scheme through the use of statistical techniques.[44] As a consequence, claims Blashfield, cluster analysis came to be thought of as a psychological method by psychiatrists, and thus came to be ignored by them. On Blashfield's account, the cluster analytic movement and the D.S.M. are opposed to each other. As the D.S.M. approach became ever more dominant, cluster analysis became less and less significant.

I have found some circumstantial evidence that suggests that Blashfield overstates the role of professional rivalries in determining the influence of cluster analytic studies on the D.S.M. In the archives of the A.P.A. and of the American Psychological Association I found correspondence relating to a conference on Classification and Nosology held in 1965.[45] Much of this conference was devoted to discussion of the use of numerical techniques in psychiatric nosology. The National Institute of Mental Health asked both associations if they would be interested in sponsoring the conference. The American Psychological Association was not interested, and so the conference was instead organised by the A.P.A. Thus in the 1960s the A.P.A. actively supported work in cluster analysis. I also went to speak to E.S. Paykel, who published some of the best-known cluster analytic studies on

[42] Skinner and Blashfield 1982
[43] Blashfield 1984 pp.73-4
[44] Blashfield 1984 p.74, p.139
[45] A.H.B. 1963, A.P.A. 1964

depression in the 1970s.[46] Paykel told me that his impression was that psychiatrists and psychologists often worked together on cluster analytic studies and that he thought it unlikely that psychiatrists would ignore studies because they used a "psychological" method. This implies that in the 1970s any tensions between psychiatrists and psychologists in the U.S. were not so great as to be evident to a British psychiatrist interested in cluster analysis. Finally, the D.S.M.-IV is accompanied by a *Sourcebook* that lists all the literature reviewed by the various Workgroups.[47] All cluster analytic studies cited are listed in Table 1.[48] Although the number of cluster analytic studies cited is quite small, several of them are by psychologists. Blashfield claimed that the A.P.A. rejected cluster analysis when the D.S.M.-III was being developed, that is from 1973 to 1980, because it was perceived as being a "psychological" method. Given that the A.P.A. actively supported cluster analysis in the 1960s, and that cluster analytic studies by psychologists were used in the development of the D.S.M.-IV, and that any tensions between American psychologists and psychiatrists during the 1970s were too slight to be noticed by a British cluster analyst, this seems unlikely.

This being said, it does seem to be the case that cluster analytic studies had little influence on the D.S.M.-III and, if Blashfield's explanation for this is to be rejected, it would be nice to be able to provide some alternative explanation. Those responsible for the D.S.M.-III have never publicly discussed cluster analysis. However, one plausible reason why more use was not made of cluster analytic studies is that the cluster analytic movement in psychiatry has constantly been dogged by allegations that the studies are methodologically poor. The clusters found in a cluster analysis need to be validated if the results are to be useful, but many published studies do not attempt to do this. In a review of studies on alcoholism, for example, only two of twenty-five studies were considered well validated by the reviewers.[49] In many cases, even those studies that are cited in the D.S.M.-IV *Sourcebook* are methodologically poor. Most fail to use a second clustering method or sample, and in some cases no attempt has been made to validate the cluster solution obtained (see Table 1 for details). I suggest that Blashfield is probably wrong in thinking that inter-professional rivalries explain the poor reputation of cluster analysis in psychiatry. Rather, a sufficient explanation is plausibly that many studies were methodologically poor, and that this was seen to be the case.

Whatever the true reason, it is clear that cluster analytic studies in psychiatry have had little influence on the D.S.M. Still, the question of whether it would be possible to use methodologically sound cluster analytic studies to construct an atheoretical classification of mental disorders remains. This is the question that must now be addressed.

[46] Paykel 1971, 1972

[47] Widiger et al 1994, 1996, 1997.

[48] The *Sourcebook* runs to thousands of pages. I have read it all once, but once only, thus it is possible that there are some cited cluster analytic studies that I have missed. Even if this is the case, however, there is no reason to think that the studies I found would be disproportionately likely to have been by psychologists.

[49] Morey and Blashfield 1981

Table 1. Cluster Analytic Studies Referred to in the D.S.M.-IV Sourcebooks (Explanatory notes follow)

Disorder and study (1st author and year)	**Checks on cluster solution**			**Author's professional affiliation**
	2nd method	**2nd sample**	**External validation**	
Insomnia				
Hauri 1983	No	No	No	Clinical psychologist
Dysthymia				
Paykel 1971	No	No	Strong	Psychiatrist
Melancholia				
Paykel 1971	No	No	Strong	Psychiatrist
Overall 1966	No	Yes	Strong	?
Post-Partum psychosis				
Hays 1978	Yes-4	Yes	Strong	Prof. of psychiatry
Specific (simple) phobia				
Curtis1990(unpublished)	No	No	Weak	Psychiatrist
Social Phobia				
Pilkonis 1977	No	No	Weak	Psychology PhD, now in psychiatry dept.
Fremouw et al 1982 Gross and Fremouw 1982	No	Yes & 2 sets variables	Weak	Psychology dept.
Turner and Beidel 1985	No	No	No	Psychiatric Institute
Mixed anxiety & dep.				
Blazer 1988(GOM)	N/A	Yes	Weak	Prof. of psychiatry
Davidson 1988 (GOM)	N/A	No	Strong	Psychiatrist and mathematician
Somato-form pain disorder				
Costello 1987	No	Yes	Strong	Psychologists in a dept. of psychiatry
Female Orgasmic Disorders				
Derogatis 1989	No	No	No	Psychologists and psychiatrists
PDDNOS				
Prior 1975	No	Yes	No	Psychologist and psychiatrist
Dahl 1986	No	No	No	Psychology dept.
Rescorla 1988	No	No	No	Psychologist
Siegel 1986	No	No	Weak	Psychologist
Attention-Deficit without hyper-activity				
Lahey 1988	No	No	No	Psychologists and psychiatrist
Hart (1991 unpublished)	No details given.			
Physical Abuse and Neglect of Children				
James and Boake 1988	No	No	No	Psychologists
Oldershaw et al 1989	Yes	No	Strong	Psychiatry dept.

Notes to table: External validation of cluster solutions is classed as "strong" if the clusters were found to correlate with variables unconnected with those used in the study, for example if clusters based on patients' symptoms are found to correlate with their response to drug treatment, and as "weak" when the clusters are found to correlate with variables connected to those used in the study, for example if clusters based on patients' ratings of symptoms are found to correlate with ratings of symptoms made by observers.

The studies by Dan Blazer and Jonathan Davidson use a technique called "Grade of Membership" analysis and are marked (G.O.M.) in the table. Grade of Membership analysis is a technique developed by Max Woodbury, a biomathematician at Duke University, in the early 1980s.[50] It differs from traditional cluster analysis in that instead of patients being exclusively assigned to one category they can be assigned a quantitative grade of membership for all categories. This means that the technique can cope with the possibility that one person may have more than one disorder. Despite this difference, Grade of Membership analysis is similar enough to cluster analysis for such studies to be considered here.

4.1.2 Could Cluster Analysis Be Used To Construct An Atheoretical Classification System?

As explained earlier, in cluster analysis data is collected on many variables of the entities being analysed. This data is then used to calculate how similar the various entities are to each other. The measurements of the average similarities between entities employed in cluster analysis can be thought of in two distinct ways:

1. As measurements of average similarity with respect to the variables actually utilised in the study.

2. As estimates of the overall true similarity between entities.

Cluster analysts who conceptualise "average similarity" in the first way commit themselves to nothing that is philosophically contentious. For them, cluster analysis is merely a tool that enables many variables to be taken into account when entities are classified. All cluster analysis allows them to do is to consider more variables simultaneously than they would be able to by other means. For these analysts there is no reason to expect that the classifications they obtain are unique. They will accept that someone conducting an analysis on the same entities but using different variables might well get different results. [51] On such a view, cluster analysis cannot be considered a means of producing atheoretical classification systems. Cluster analysis allows many variables to be considered, but these variables will still just be a sub-set of those that might have been analysed. As such, a theory is needed to select those variables that are scientifically interesting.

On the other hand, cluster analysts who conceptualise "average similarity" as being an estimate of the overall true similarity between entities commit themselves to particular metaphysical claims. "Overall true similarity" can be defined roughly as being the proportion of properties that two entities share (slight complications arise when two entities share no property but are somewhat similar because they

[50] See Woodbury and Manton 1982 for further details.

[51] View held by Jardine 1969 – p.211 asserts that there exists no optimal classification, p.216 condemns the idea "...that as increasing numbers of attributes are selected so the 'true' underlying dissimilarities between populations are estimated with increasing accuracy."

each possess a similar property, for example, one weighs 48.9kg while the other weighs 48.8kg). According to Robert Sokal and Peter Sneath (1963) the measures of average similarity provide an estimate of the true overall similarities because "we are taking a random sample from a very large number of characters which we could in theory sample and which would yield us a single, definite proportion of matches if we were able to sample all the characters."[52]

As has often been noted, "overall true similarity" is only a meaningful concept if the number of properties possessed by any one entity is finite.[53] Some writers have claimed that entities "obviously" possess infinitely many properties and have used this point to argue that the project of the analyst who is concerned with "overall true similarities" is incoherent.[54] I suggest that it is far from obvious that entities do possess an infinite number of properties. The plausibility of this claim depends on the metaphysical account of properties that one adopts, and there are many different accounts of properties currently on the philosophical market.[55] The claim that entities have a finite number of properties will seem plausible to those who adopt some realist account of properties – if properties are objective features of the world then their numbers will be fixed. The claim that entities possess an infinite number of properties is more likely to appeal to nominalists – if properties are in some sense generated by the human mind then their supply will plausibly be unlimited.

Debates over the nature of properties are too complex to enter into here, so for the sake of argument let's suppose that entities do have a finite number of properties. Could the cluster analyst who uses a large enough sample of variables then succeed in obtaining an atheoretical classification system? I suggest not. The measures of average similarity calculated by a cluster analyst are based on the *variables* analysed, but the true overall average similarity would depend on the proportion of *properties* that two entities share. The cluster analyst who aims to gain estimates of true overall similarities is committed to a realist account of properties. Only on such an account will there be some "overall similarity" between entities that can be measured. On a realist account, however, 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.[56] Only if the cluster analyst's variables measure genuine properties will the measures of similarity be estimates of the overall average similarity.

If we want to pick variables that measure true properties our best bet is to rely on our best scientific theories. Those predicates found necessary in our best scientific theories provide our guide as to what properties actually exist. The variables employed in a cluster analysis should aim to measure these properties. As such, if the measures of average similarity are thought of as estimates of the true overall

[52] Sokal and Sneath 1963 p.114, Lorr 1982 takes a similar view and states that cluster analytic techniques "make possible the discovery of natural groupings" p. 461

[53] Sokal and Sneath 1963 pp.91-92, Ehrlich 1964 p.117, Johnson 1968 p.18

[54] Ehrlich 1964 p.117, Johnson 1968 p.18

[55] See Mellor and Oliver 1997 for an overview.

[56] Schizophrenogenic mothers were thought to induce schizophrenia in their children

average similarity, then a theory is needed to guide the choice of variables. As a consequence, these theorists are simply wrong if they claim that cluster analysis can be used to produce atheoretical classification systems.

This point can be made clearer by considering what happens if a variable that does not correspond to a property is included in an analysis. Let's call such a variable a "junk variable". Suppose we have a cluster analysis in which the variables are measures of various psychopathological symptoms - measures of feelings of worthlessness, hallucinations, speed of speech, and so on. Amongst other clusters such cluster analyses of general psychopathology usually produce a cluster that corresponds roughly to psychotic depression. The patients in this cluster feel worthless, they have thought about killing themselves, and their sleep-patterns are disturbed. Now let's consider what would happen if into this analysis we added a junk variable. The junk variable we will add codes for the day of the week on which the patient was born. Adding this variable will tend to split the depressive cluster into seven (whether it actually will split into seven as the result of this one variable depends on the clustering algorithm employed). These clusters will correspond to Monday-born depression, Tuesday-born depression, Wednesday-born depression, and so on. However, we don't believe that Monday-born depression is different in any interesting respect from Tuesday-born depression. Adding junk variables tends to produce cluster solutions that we do not believe corresponds to the natural structure of mental disorders.

Similar problems arise if variables are used that measure properties that are genuine properties but of no theoretical interest in the context. Suppose, for example, that the sex of the patient makes no difference to the nature of the mental disorder that afflicts them. Sex may be a genuine property, but if used in a cluster analysis it will tend to split groups into female and male schizophrenics. In such a case once again the effect of including the variable is to produce a cluster solution that we do not believe reflects the true structure of mental disorders.

As mentioned earlier, it is accepted good practice to repeat a cluster analysis using a second set of variables, or a sub-set of the original set. If this is done, then problems caused by the inclusion of the odd junk or irrelevant variable can be avoided. However, if a significant proportion of the variables are poorly chosen then even this practice won't protect against the generation of worthless cluster solutions. We can conclude that junk variables and irrelevant variables should not be included in cluster analyses, and the only way to try and avoid them is to use our best theories to guide the selection of variables. Thus, regardless of the way in which the analyst conceptualises the measures of average similarity a theory is needed to guide the selection of variables.[57]

Might it be possible to use Nagel's suggestion again at this point? Although the selection of variables requires some theory, might it be possible to use a theory that is not amongst those about which different mental health professionals disagree? I suggest not. Deciding whether a property is relevant requires a theory of the domain in question. For example, in a cluster analysis of psychopathology, biologically-

[57] Fleck 1979, p.92 also notes that the choice of characteristics to be analysed "depends upon the habits of thought of the given scientific discipline; that is, it already contains directional assumptions."

orientated psychiatrists will want to include biological variables but may well consider variables linked to "defence styles" to be irrelevant. 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. Here we have only looked at cluster analysis because this is the numerical technique that can be used to produce categorical classification systems such as the D.S.M. However, it is worth noting that the discussion here is sufficient to show that no other numerical techniques are capable of producing atheoretical classification systems either. In all cases, a theory is going to be required to inform the selection of the variables that will be subjected to analysis. I conclude that classification systems must always draw on some theory or other, as a theory must be used to decide which features of the entities under study are of scientific interest. Classification cannot be theory-free. Furthermore, as the theory used must be a theory of mental disorders, classification cannot even be theory-neutral in Nagel's sense.

5.IMPLICATIONS FOR THE D.S.M.

All three of the ways in which observation might be theory-laden have now been examined. In the first section I concluded that there is insufficient evidence to determine whether or not perception in psychiatry is theory-laden (it will be remembered that by "perception" I mean what an organism sees, hears, or otherwise senses).

In the second section it was concluded that all observation statements are theory-laden but that the implications of this are limited. As Nagel points out, although observation statements assume theories, often the theories assumed by the observation reports will not be the theories that are in dispute. The scope of Nagel's observation can be maximised if a causal account of reference is appropriate, and through employing non-linguistic forms of communication. I conclude that while observation statements cannot be theory-free they can often be neutral between the theories that are in dispute. As theory-ladenness will only lead us astray when our theories are wrong this thought is comforting. Assuming that science has indeed made some progress, theories about which there is consensus are more likely to be right than those that are in dispute. Thus, if we can use language that assumes only widely accepted theories the epistemic problems caused by the theory-ladenness of language can be minimised.

In the final section I argued that a theory is always required for classification because a theory is required to guide a scientist in deciding which features of the world are relevant. Despite the claims of some numerical taxonomists this problem renders even so called "empirical" techniques, such as cluster analysis, theory-laden. To know whether or not some property is relevant one needs a theory of the domain in question, and so classification cannot even be theory-neutral in Nagel's sense.

If the D.S.M. cannot be theory-free what theory does it use? I suggest, as have many writers before me, that the D.S.M. tacitly assumes that some biological account of mental illness will prove to be correct. The *Sourcebook* published alongside the D.S.M.-IV reveals that the studies appealed to by the D.S.M. 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.

As mentioned at the beginning of this chapter if the D.S.M. is theory-laden this may give us reason to doubt that the categories it contains will reflect natural kinds of disorder. Biological accounts of mental illness are by no mean uncontroversial. As the D.S.M. tacitly assumes some biological explanation for mental disorder the D.S.M. categories stand, or quite possibly fall, with such an account.

CHAPTER 4

THE D.S.M. AND FEEDBACK IN APPLIED SCIENCE

The D.S.M. is used for many purposes. For example, D.S.M. diagnoses are required for medical insurance forms, they are recorded on hospital records, they often form a basis for deciding treatment, and they may be appealed to in legal cases where the sanity of the defendant is at issue. In this chapter I examine whether, and if so how, the D.S.M. is affected by pressures that arise from the ways in which it is used. The bulk of the chapter consists of two detailed historical case studies, examining the use of the D.S.M. by the pharmaceutical industry, and by the medical insurance industry. These provide contrasting cases; while the A.P.A. has been proud to present the D.S.M. as a classification system designed to facilitate treatment with psychoactive drugs, it has sought to distance itself from the use of the D.S.M. by insurance companies. In my case studies I demonstrate that the D.S.M. has been moulded by pressures arising from both the insurance and pharmaceutical industries. The final part of the chapter examines the epistemic significance of this feedback. In particular I examine whether such feedback makes it more or less likely that D.S.M. categories will correspond to natural kinds of disorder.

Readers especially interested in the ways in which the D.S.M. has been shaped by social and financial pressures might usefully read this chapter alongside Herb Kutchins and Stuart Kirk's (1997) *Making Us Crazy*. Kutchins and Kirk adopt a different approach to that taken here, and focus on particular, high-profile D.S.M. diagnostic categories – homosexuality, borderline personality disorder, and post traumatic stress disorder, amongst others. In their case studies, Kutchins and Kirk demonstrate that these categories have been affected by political, social and financial pressures. *Making Us Crazy* is an interesting and important book. However, my case studies are needed in addition to their work. The D.S.M. is a huge classification system, constructed by committee. However lamentable, a few questionable decisions are bound to be made in such a process, and Kutchins and Kirk demonstrate that this has indeed been the case. In my case studies I seek to go beyond this, in that I explore how the D.S.M. as a *whole* has been systematically affected by pressures stemming from the pharmaceutical and insurance industries

1.FIRST CASE STUDY: THE PHARMACEUTICAL INDUSTRY AND THE D.S.M.

Modern psychoactive drugs became available during the 1950s. Smith Kline and French started marketing chlorpromazine in 1954, tests on reserpine began in the same year, and antidepressants such as iproniazid and imipramine were available by

1956.[1] The new drugs radically changed psychiatry. Not only could the "untreatable" now be treated, but an army of researchers were needed to perform clinical trials, and pharmaceutical companies acquired a profitable new market. Following the introduction of the new drugs, the 1970s witnessed a surge of interest in psychiatric classification that culminated in the publication of the D.S.M.-III in 1980. It is often suggested that these two trends are linked and that psychiatric classification systems such as the D.S.M.-III had to be constructed to meet the needs of those developing and prescribing the new drugs.[2] A central question to be addressed in this case study is whether this hypothesis can be supported. Is the D.S.M. a classification system designed to facilitate drug treatment?

1.1 Preliminaries: Three Models Of Drug Efficacy

Understanding the ways in which the new psychoactive drugs were used requires us to understand the ways in which drug effects were conceptualised. The ways in which researchers and physicians thought of the action of the new drugs were coloured by their experience with older medications. When the new psychoactive medications were introduced at least three models of drug efficacy were available. I will call these "The Barbiturate Model", "The Target Symptom Model", and "The Magic Bullet Model". In outlining these models I do not mean to suggest that these were the only models in use, nor that researchers or clinicians necessarily thought of themselves as adhering to one or another of them. My claim is far weaker: These three models were in the background when psychiatrists thought about the new drugs, and a consideration of them can help make sense of the ways in which drug trials were designed and of the strategies for treating patients that were developed.

1.1.1 The Barbiturate Model

Prior to the introduction of chlorpromazine, psychiatrists used barbiturates to sedate patients who were agitated or violent. It was accepted that barbiturates did not cure patients, but only quietened them. A drug conceptualised on the barbiturate model is thought to work on anyone who takes it. As such, a patient's diagnosis is unimportant in determining their suitability for drug treatment. It follows that studies of drug efficacy are seen as having nothing to contribute to psychopathology; the drug is thought to merely suppress the expression of symptoms rather than to correct some underlying biological problem.

Many early drug trials were based on the barbiturate model. In these trials, drugs are given indiscriminately to patients with any, or no, diagnosis, and when analysing their results the researchers make no attempt to see whether the treatment is more successful in patients with some particular diagnosis or with particular symptoms. A 1957 paper in the *American Journal of Psychiatry*, for example, reports a study in which Marsilid (iproniazid) was given to fifty patients suffering from various neuroses, manic-depression or schizophrenia. The author reports that "5 were

[1] Grob 1991 pp.148-149
[2] For example Klerman 1986 p.20, Sadler and Agich 1995 p.220

improved, 19 partially improved, 15 were unimproved."[3] No analysis is given of the characteristics of those patients who benefited from the treatment. Another 1957 paper, presented at the International Symposium on Psychotropic Drugs, is even more explicit in its use of the barbiturate model.[4] In the study, promazine was given to the residents of an old people's home and of a centre for juvenile delinquents. Many of the subjects had no psychiatric diagnosis. Indications for "treatment" ranged from a fractured hip, to wanderlust, to using bad language. The authors felt their study to be a success. The elderly people "became readily manageable, more co-operative and...it was felt the results significantly reduced the required nursing care".[5] Promazine was an improvement over the barbiturates that had been used previously as "even when actually sleeping from the effects of the medication the patients could readily be aroused for feeding, bathing and caring for vital functions".[6] The juvenile delinquents also showed "significant improvement" and the authors felt that their study would go some way to resolve what had become a "major social and economic as well as medical problem".[7]

Some early studies in psychopharmacology reinforced the barbiturate model of drug efficacy. Chlorpromazine and meprobamate were amongst the first drugs to be studied and both were seen to help many patients regardless of their diagnosis. Meprobamate is a tranquilliser and so at least helped to calm the experimental subjects. Chlorpromazine happens to be a drug that has a "broad action", and helps in lots of conditions. A 1968 textbook lists its psychiatric indications as: schizophrenia, mania and hypomania, depression (no direct effect, but can control associated agitation), delirium tremens, post-traumatic states, G.P.I., senile dementia, withdrawal symptoms of alcohol addicts, epileptic disturbed behaviour, Huntingdon's chorea and spastic paralyses (reduces associated tension and over-activity), anorexia nervosa (useful as an appetite stimulant).[8] With such a wide range of indications it is not surprising that the majority of a random selection of psychiatric patients improved on chlorpromazine.

The barbiturate model also suited the realities of treatment provision in the underfunded and overcrowded state mental hospitals. Conditions in these hospitals made it difficult to provide treatment that was individually tailored to patients' needs. As such, the possibility being able to give the same drug to all patients was highly attractive. In some hospitals psychoactive drugs seem to have been administered on such a basis. Klein and Davis (1969) write,

> These [state mental] hospitals have moved from custodial care and little active psychiatric treatment to undiscriminating treatment which relies primarily on psychotropic drugs...however...with the remarkable effectiveness of the phenothiazines

[3] Ayd 1957 p.459
[4] Klein et al. 1957
[5] Klein et al.1957 p.519
[6] Klein et al. 1957 p.523
[7] Klein et al. 1957 p.515
[8] Shepherd et al 1968 p.106

> in a wide spectrum of acute psychoses, there is no doubt that many patients are being helped even by these relatively primitive methods.[9]

Within a few years of the introduction of psychoactive drugs the barbiturate model fell out of favour with researchers. With the rise of the anti-psychiatry movement the use of drugs as "chemical straitjackets" became unacceptable.[10] In addition, some of the new drugs coming onto the market simply could not be made to fit the barbiturate model. Barbiturates sedate anybody who takes them. Chlorpromazine fitted the barbiturate model fairly well in that it was broad acting and produced a sense of serene detachment even when taken by normal subjects. In contrast the antidepressants were found to be far more specific in their action and had no effect on people with normal mood. The antidepressants could only be understood using a different model of drug efficacy.

1.1.2 The Target Symptom Model

Aspirin is a drug that acts on the target symptom of pain. It reduces pain whatever its cause. This is characteristic of drugs that fit the target symptom model - they suppress a particular symptom whatever its etiology. While a drug that fits the barbiturate model will affect anybody, whatever their symptoms or lack of them, a drug that fits the target symptom model will only affect people suffering from a particular symptom - if someone who has no pain takes an aspirin then nothing much happens to them.

The target symptom model was popular with clinicians and researchers throughout the 1950s and 60s. Throughout this period many American textbooks encouraged psychiatrists to adopt a target symptoms model when prescribing drug treatments. A 1967 textbook suggests that treatment with neuroleptics might be directed "at the following symptoms regardless of diagnosis: tension, agitation, hyperactivity, agitation, restlessness, impulsiveness, aggressiveness, assaultive and destructive behaviour, and auditory or visual hallucinations".[11] The 1966 *American Textbook of Psychiatry* advises general practitioners to familiarise themselves with a small selection of psychoactive drugs and then to learn to "apply them in terms of 'target symptoms' rather than in terms of diagnostic categories".[12] On the target symptom model the actual diagnosis a patient receives is unimportant in determining how that patient should be treated, as patients in different diagnostic categories may suffer from the same symptom. For example, both someone suffering from bipolar depression and someone with schizophrenia may suffer from depressed mood, and on the target symptom model they can both be treated with antidepressants.

Researchers adopting a target symptom model select subject groups on the basis of symptoms rather than by diagnosis. For example, Eisenberg and Rozan (1960) investigated the mood-elevating effects of clorphenoxamine HCl. All their subjects suffered from depressed mood, although they had diagnoses ranging from neurotic

[9] Klein and Davis 1969 pp.148-9
[10] Shepherd et al. 1968 p.96
[11] Denber 1967 p.1257
[12] Cantrell and Frazier 1966 p.606

depression, to undifferentiated schizophrenia, to organic brain disorders. As late as 1970, a textbook advised researchers to select subject groups on the basis of symptoms as "Patients with the same diagnosis may be quite heterogeneous, and their symptoms and their disabling features may require different therapy."[13]

Although the target symptom model fell from prominence during the 1970s it continues to have some influence on the thinking of psychiatrists up to the present day. Some researchers still call for a return to the target symptoms approach, [14] and whether they like it or not clinicians are forced to select treatments on the basis of symptoms when treating patients who fit no diagnostic categories.

1.1.3 The Magic Bullet Model

Magic bullet drugs, such as the antibiotics, work by attacking the cause of a disease. The drugs are highly specific in that each drug only attacks specific kinds of causal agents. Thus precise diagnosis is essential if these drugs are to be used successfully.

The magic bullet model has coloured the thinking of both researchers and clinicians since the introduction of psychoactive drugs, and has been the dominant model since the early 1970s. Drug trials now almost universally select a subject group on grounds of diagnosis, and most textbooks are set out so as to suggest that treatment should largely be determined by diagnosis, for example, separate chapters may be devoted to a consideration of treatments suitable in depression, and in the anxiety disorders, and so on.

On the magic bullet model, drugs are assumed to attack some underlying pathological causal mechanism. As such, information regarding the efficacy of drugs in particular patient populations provides valuable data for psychopathological theorising. When it is a matter of debate whether two forms of disease are really distinct, if both patient groups respond to a drug this is taken as evidence that the disease forms are fundamentally the same, whereas a differential response is taken to suggest that the disease forms are really different.

Attempts to use such "pharmacological dissection" to inform the classification of mental disorders began almost as soon as psychoactive drugs became available. Kline and Stanly conclude a 1959 trial of iproniazid with the claim that "a preliminary analysis of the characteristics of the patients showing the different types of response would seem to argue strongly that there exist basically three different types of depression which cut across the traditional divisions".[15] More influentially, from the early 60s, Donald Klein, the most vocal advocate of pharmacological dissection, attempted to use patterns of drug response to pick out new disorders. Most famously, Klein and Fink (1962) argued that anxiety characterised by panic attacks is distinct from other anxiety disorders on the grounds that only anxious patients with panic attacks respond to imipramine. They argued that imipramine acts specifically on the pathological process underlying the panic, rather than on any underlying depression because, unlike depressed patients, patients suffering panic

[13] Okun 1970 p.386
[14] For example Hippius 1996 p.210
[15] Kline and Stanly 1959 pp.612-3

attacks do not respond to convulsive treatment. The study was particularly influential, no doubt in part because Klein sat on the D.S.M.-III committee, and anxiety characterised by panic attacks came to be included as a separate disorder in the D.S.M.-III.[16] Klein also argued that "emotionally unstable character disorder" and "school phobia" should be recognised as distinct disorders on the basis of drug effects, but these ideas never gained such widespread support.[17]

More recently, pharmacological evidence has tended to be used to support calls to group together currently distinct categories. Hudson and Pope (1990) argue that there is a family of "affective range disorders" that includes major depression, bulimia, panic disorder, obsessive compulsive disorder, attention deficit disorder with hyperactivity, cataplexy, migraine, and irritable bowel syndrome. All these disorders respond to a variety of chemically unrelated antidepressants and so "may share a pathophysiologic 'step' in the etiologic chain of steps required for their expression".[18] As another example, there have been proposals that trichitillomania, nail biting, and similar illnesses should be grouped with obsessive compulsive disorder because all these conditions respond similarly to S.S.R.I. treatment. [19]

There is nothing inconsistent in a psychiatrist accepting that different psychoactive drugs are best conceptualised using different models of drug action. Indeed, this would seem the most sensible line to take, as it is likely that different psychoactive drugs just do act in different ways. In theory it should be possible to empirically discover which model best fits a particular drug: A drug that fits the barbiturate model affects everybody, including normal subjects. A drug that fits the target symptom model affects symptoms whatever their etiology. A drug that fits the magic bullet model affects the root cause of a disease. Some researchers were indeed willing to revise how they thought of a drug in the light of empirical research. Holt, Wright and Hecker (1960) designed a drug trial on the target symptom model and gave antidepressants to patients with a variety of diagnoses all of whom suffered from depressed mood. After analysing their results, they shifted towards a magic bullet model of antidepressant action. They concluded that "Despite the similarity of depressive symptomatology of all the patients, it would appear that individual improvement depended… on whether the patient had a primary depressive reaction or depressive symptomatology associated with another psychotic reaction."[20]

Many psychiatrists, however, clung tenaciously to one particular model of drug action and viciously attacked psychiatrists who adopted models other than their own. Take Klein and Davis (1969) on the target symptom approach: They complain, "This procedure is exactly as rational as prescribing penicillin for all cases of fever. That is, it is occasionally effective, frequently useless and occasionally results in needlessly incurred iatrogenic complications."[21] Against the magic bullet model,

[16] Shorter 1997 p.320
[17] On emotionally unstable character disorder: Rifkin, Levitan, Galewski and Klein 1972. On school phobia: Gittelman-Klein and Klein 1973.
[18] Hudson and Pope 1990
[19] Pichot 1996 p.21
[20] Holt et al. 1960 p.535
[21] Klein and Davis 1969 p.13

Irwin (1968) states, “The psychoactive drugs available for use do not appear selective and surely cannot distinguish between a neurosis and a psychosis... Such terms are but diagnostic constructs of expedience that defy precise definition even for the psychiatrist.”[22]

That psychiatrists felt the need to claim that all psychoactive drugs acted in one way or another is probably best seen as a symptom of the ongoing struggle between biologically-oriented psychiatrists and psychoanalysts. Psychoanalysts tended to be attracted to a target symptom model of drug action. By maintaining that drugs acted only on symptoms, analysts could maintain that true cures can be obtained by psychoanalytic means alone. This appears to have been the dominant view in the late 1950s and early 1960s. Many adverts for drugs in this period market even powerful drugs such as chlorpromazine and prochlorperazine as being useful adjuncts to psychotherapy.[23]

The target symptom model also helped to justify the view that drug treatments are easy to administer. On this model a practitioner merely has to spot the symptoms to be altered and prescribe the appropriate drug. Thus Irwin (1968) could suggest that, “In general, the patient history required is minimal; an interview of ten to 15 minutes is usually sufficient to obtain the information required for drug therapy.”[24]

Finally, claiming that drugs only suppress symptoms implies that data concerning the efficacy of a drug has little to contribute to psychopathology. If patients from different diagnostic groups respond to the same drug this does not indicate some fundamental similarity in the underlying pathology of the two disorders, but only shows that they have similar symptoms. In short, adopting the target symptom model allowed psychoanalysts to view drug-prescribing psychiatrists as second-class psychiatrists: they could not cure patients, their job was comparatively unskilled, and they had little to contribute to psychiatric theory.

Conversely, biologically-orientated psychiatrists were, and continue to be, attracted to a magic bullet model of drug action. On this model, biologically-orientated psychiatrists can claim to be able to cure patients. Their job can be seen as skilled because precise diagnoses are required before treatments can be selected, and “pharmacological dissection” can be seen as a tool for understanding psychopathology.

That there are different models of drug efficacy − the barbiturate model, the target symptom model, and the magic bullet model − helps to clarify the links between psychiatric classification and the testing and use of drugs. It is now routinely claimed that precise diagnoses are essential for research and treatment. However, my review of different models of drug efficacy illustrates that has not always been thought to be the case. The use of D.S.M-style diagnoses in testing and using drug treatments can be seen to be based on a magic bullet model of drug action. As there are other possible models, it was by no means the case that once the psychotropic drugs were developed something like the D.S.M. had to follow. If we are to discover how the D.S.M. has been shaped by drug treatments we must look in

[22] Irwin 1968 pp.3-4
[23] *American Journal of Psychiatry* adverts 1956 and 1960
[24] Irwin 1968 p.16

greater detail at the links between the D.S.M. and the various parties who used the new drugs. Here I shall examine in turn the relations between the D.S.M. and the needs of those involved in drug research, in drug treatment, and in drug marketing.

1.2 Research And Psychiatric Classification

Since the D.S.M-III, the A.P.A. has been proud to present the D.S.M. as a classification designed with the needs of researchers in mind. The first page of the D.S.M.-III tries to link the increased interest in diagnosis with the needs of researchers. In particular, it claims that a classification like the D.S.M-III is required as "The efficacy of various treatment modalities can be compared only if patient groups are described using diagnostic terms that are clearly defined."[25] Here I aim to assess this claim. To what extent has the D.S.M. been shaped by the needs of researchers in psychopharmacology?

1.2.1 The D.S.M.-II and Drug Trials

The D.S.M.-II was published in 1968. By the time the D.S.M.-II was being developed effective psychoactive medication had been available for a decade. As such, it would have been possible for the D.S.M.-II to have been shaped by the needs of researchers in psychopharmacology. Unfortunately, the A.P.A. archives contain no documents that indicate whether or not this was the case. Thus, one is forced to rely on circumstantial evidence to determine whether the DSM-II was designed with psychopharmacology in mind.

The D.S.M.-II and the mental disorders section of the Eighth Edition of the International Classification of Diseases (I.C.D.-8), the classification used by the W.H.O., were developed in tandem. Members of the A.P.A. sat on the U.S.-U.K. committee that played a key role in developing the mental disorders section of the I.C.D., and the D.S.M.-II and I.C.D.-8 classifications are very similar. Many of the people who sat on these committees are now dead or untraceable, but I managed to track down Clive Spicer, a British medical statistician who sat on the U.S.-U.K. committee. I asked him what purposes that committee hoped the new classification would fulfil, and specifically whether they hoped it would be of use to researchers in psychopharmacology. Spicer wrote,

> I can say categorically that the committee hoped their work would be important in providing well defined and comparable statistics for administrative and epidemiological use. At that time the administrative side was paramount. I do not think that it was in any way influenced by research in psycho-pharmacology.

Thus it appears likely that those responsible for the D.S.M.-II did not attempt to fit the classification system to the needs of the psychopharmacologists. Despite this, members of the committees were enthusiastic supporters of drug use. Henry Brill, the chairman of the D.S.M.-II committee between 1960 and 65, introduced chlorpromazine and reserpine into New York public hospitals, and wrote influential

[25] A.P.A. 1980 p.1

papers claiming that hospital numbers fell as a result.[26] Benjamin Pasamanick, the chairman of the U.S. W.H.O. committee and a member of the A.P.A. committee, published a classic study demonstrating the efficacy of drug treatment in schizophrenia.[27] Another member of the U.S. W.H.O. committee, Leon Eisenberg, supported the introduction of Ritalin as a treatment for hyperactive children.[28] As such, if the committees did not concern themselves with meeting the needs of psychopharmacologists, this disinterest probably indicates that they felt they had nothing to offer, rather than that they didn't want to help.

An examination of the drug studies undertaken by the committee members suggests that they did not consider the accurate diagnosis of subjects to be essential for drug trials. In Brill and Patton's 1957 and 1959 studies, patients with diagnoses ranging from Dementia Praecox to manic-depression to senile psychoses were given reserpine or chlorpromazine. Brill and Patton do not report success rates by diagnosis, nor even by drug received, but rather provide statistics detailing the fall in the total mental hospital population following the introduction of drugs. The take home message of their paper is that (any) drugs have a good chance of helping (any) patients.

In Pasamanick's 1959 study, the subject group consisted entirely of schizophrenics. However, it seems that Pasamanick did not pick out schizophrenics because he believed that different drugs will best treat different disorders; as in Brill and Patton's study, different subjects were treated with different drugs, but are then lumped together in the outcome statistics. Rather, it seems that Pasamanick selected schizophrenics because he was interested in examining whether chronically ill patients could be treated in the community and, for Pasamanick, a diagnosis of schizophrenia ensured that a patient was chronically ill.

Finally, Leon Eisenberg explicitly states in a 1971 paper on the use of stimulants for treating hyperactive children that in his view "drugs treat symptoms not diseases".[29] As already mentioned, those who adopt a target symptom model do not consider there to be a direct link between a patient's diagnosis and the treatment that they will require. On such a view, the subject group of a drug trial do not all need to suffer from the same disorder, and D.S.M.-style diagnoses are not necessary for research.

If I am right, and the committee members indeed saw no need for explicit diagnostic criteria to be used in drug trials, this would not have been an unusual view for the time. Pre-1960 drug trials often involved giving a drug to a large group of patients with heterogeneous diagnoses. Researchers sometimes justified this practice by claiming that they didn't know what a drug would do and so had no grounds on which to select a subject population.[30] Depending on how the results were analysed this kind of trial is consistent with any of the three models of drug

[26] Brill and Patton 1957, Brill and Patton 1959
[27] Pasamanick et al. 1967
[28] Eisenberg 1971
[29] Eisenberg 1971 p.378
[30] For example Rickels et al. 1959 p.664 "we chose a heterogeneous patient group as we had no basis for limiting our patient population."

action discussed earlier. Researchers holding a barbiturate model of drug action needed to pay no attention to the specific symptoms or diagnoses of the class of drug responders. They tended to merely report that a certain percentage of patients improved. Researchers following a target symptoms model would analyse the symptoms of patients who responded to the drugs, while those following a magic bullet model would analyse their diagnoses. When researchers noted patients' diagnoses, or symptoms, they did so in a way that appears slap-dash compared with the practices of today. Now, researchers employ formal, explicit criteria when deciding whether someone suffers from a particular disorder, or displays a particular symptom. In contrast, in early drug trials, the patient's diagnosis was usually simply taken from hospital records. Judging a patient's symptoms and whether or not they had responded was assumed to be unproblematic. Researchers merely noted the proportion of patients who suffered from depressed affect, suspiciousness or whatever, and recorded the number of patients who were "slightly" or "greatly" improved.

Within a few years it became more common for drugs to be tested on a subject population selected on grounds of diagnosis or symptoms. I suggest this change occurred for several reasons. First, many of the drugs tested later were chemically closely related to those that had been tested before, for example promazine and chlorpromazine differ from each other only by one chlorine atom. When testing these drugs researchers expected them to work for the same class of patients as the parent drug. This meant that researchers had some grounds on which to select a particular subject group of patients.

Second, it was increasingly being recognised that drugs could have toxic side-effects. The effects of thalidomide on foetuses were reported in 1961. Of particular relevance to psychiatry, from the early 1960s reports emerged that M.A.O.I.s, a class of antidepressants, could interact with particular foods and produce potentially fatal rises in blood pressure. This meant that giving drugs to everyone on the grounds that they wouldn't do any harm and might do some good was no longer justifiable.

Third, and linked with the second point, researchers came to be expected to gain their subjects' consent before experimenting on them.[31] As a consequence, patients' fears of drug side-effects also became important in determining whether drug trials could go ahead. Presumably, patients would more readily enter a trial aimed at treating their particular disorder than one where drugs would be given to everyone the researcher could find because he had no idea what its effects might be.

Although researchers now often selected patient groups on the grounds of diagnosis, the diagnoses continued to be made on the basis of clinical impression rather than on the basis of explicit, formal criteria. Nevertheless, it was well known that psychiatric diagnoses could be unreliable. Hunt et al (1951) had found that psychiatrists only agreed on the specific diagnosis a patient should receive 32.6% of the time. Cameron (1951) writes in passing that "A patient referred from one

[31] Examples of papers where explicitly no consent, Ayd 1957 "Marsilid was administered without comment as to what it was, or the expected clinical response", Beresford Davies 1957 p.538 "They were told as little as possible but were given to understand that the tablets were supposedly beneficial." By 1970 Okun p.385 instructs researchers to gain informed consent, preferably in writing.

hospital to another...can by no means be sure that he will be accorded the same diagnosis."[32] However, while in the 1970s the unreliability of psychiatric diagnosis became a public scandal, in the 1950s and 60s no one much cared.

In part this indifference can be explained by the fact that in the 1960s many researchers saw a patient's diagnosis or their symptoms (depending on whether the magic bullet or target symptoms model was being employed) as being merely one amongst many factors that might conceivably affect treatment outcome. All the factors that might influence treatment outcome would have to be analysed by the researcher. Klein and Davis (1969) suggest that minimum sample data should be given,

> in terms of the distribution of such characteristics as age, sex, social class, education, age at onset of illness, number of previous episodes, length of illness, legal status, response to past treatment, duration of illness episode at time the patient entered the study, whether spontaneous remissions could be expected or whether he entered the study after other modes of therapy had been tried and failed, developmental history, familial history, signs of brain damage, specification of degree of illness, and psychiatric diagnosis.[33]

Hamilton (1965) gives a similar list. Many of these factors would be difficult to measure reliably. As such the unreliability of diagnoses was just one problem amongst many.

My claim that psychopharmacologists had little interest in formal diagnostic systems such as the D.S.M. is supported by the fact that they made little use of the D.S.M.-II once it was published. I looked at articles published in the *American Journal of Psychiatry* in 1970 and in 1975 that reported studies in which patient populations were selected on the grounds of diagnosis. The criteria used in these studies to select subjects are shown in Table 2. Although I examined too few papers for this survey to be conclusive, these results suggest that the use of diagnostic criteria increased greatly between 1970 and 1975, but that no particular scheme managed to establish itself in this period. Notably very little use was made of the D.S.M.-II by researchers. As mentioned earlier, many have suggested that the drive to create standardised diagnostic criteria that occurred in the 1970s might have been motivated by the needs of researchers in psychopharmacology. However, in both 1970 and 1975 there was no tendency for papers reporting drug trials, as opposed to papers reporting the results of other types of experiments, to provide more explicit details of the diagnostic criteria employed – if anything the reverse is the case. This suggests that the move to produce standardised and explicit diagnostic criteria was not driven by the needs of early researchers in psychopharmacology. To sum up: the D.S.M-II was neither designed for, nor wanted by, the early psychopharmacologists.

[32] Cameron 1951 p.41
[33] Klein and Davis 1969 p.423, for a similar list see Hamilton 1965 p.40

Table 2. Use of the D.S.M.-II and other diagnostic schemes in psychiatric research

I examined all articles published in the *American Journal of Psychiatry* in 1970 and in 1975 that reported studies in which patient populations were selected on the grounds of diagnosis. The criteria used to select the subject group are shown in the table. Papers in which the subject group was selected long before publication, for example follow-up studies of patients treated twenty years previously, are not included in the table as such studies could not make use of diagnostic systems first published in the late 1960s.

		Criteria employed in study to select subject group						
		Simply states that subjects had disorder	Only "certain" cases included	2+ psychiatrists diagnosed	Symptoms described in paper	D.S.M.	Other diagnostic system	Book/paper characterising patients is cited
1970	Drug trials (18 papers)	12		2	3		1	1
	Others (19 papers)	10		2	5	2 (DSM-I)	1	1
1975	Drug trials (14 papers)	4		2	5	1 (DSM-II)	3	
	Others (15 papers)	2	1	1	3	1 (DSM-II)	7	

1.2.2 D.S.M.-III And Drug Trials

Many of the diagnostic criteria included in the D.S.M.-III were modelled on those contained in Spitzer et al's 1975 Research Diagnostic Criteria (R.D.C.) and, in their turn, the criteria included in the R.D.C. were modelled on those designed by Feighner et al (1972). Both the Feighner criteria and the R.D.C. were designed specifically for research. As the Feighner criteria, the R.D.C., and the D.S.M.-III are so closely related their reception by researchers is best considered together.

As already mentioned, the introduction to the D.S.M.-III claims that the classification is intended to be used by researchers. Given this intention, it is remarkable how little researchers and D.S.M.-committee members have to say about links between the D.S.M. and the needs of research. The only comment regarding research that I found in the D.S.M.-III archives noted that larger numbers of categories of mood disorder and personality disorder were favoured because this provided a greater opportunity to gather data and to evaluate treatment procedures.[34]

[34] Bluestone et al. 1976

This being said, researchers began using the Feighner criteria, the R.D.C., and the D.S.M.-III as soon as they were available. In the first ten years after its publication the Feighner paper received about 1,650 citations.[35] Most of these citations were in papers using the Feighner criteria to select subject populations. Researchers began using D.S.M.-III criteria even before the classification system was officially published. Currently use of the D.S.M.-IV is almost universal.

It is a fact that in the late 1970s and the early eighties the Feighner criteria, the R.D.C., and the D.S.M.-III all became hugely successful. However, their popularity is too late to be directly linked with the development of psychoactive drugs. By the late 1970s drug trials had been taking place for two decades and, as we have seen, psychopharmacologists had not previously felt a need for explicit diagnostic criteria. Rather, I suggest, the success of systems such as the D.S.M.-III should be seen as part of a general drive to standardise diagnosis and measures of symptomatology.

The conditions that allowed this movement to gain momentum in American psychiatry during the 1970s are described by Roger Blashfield in *The Classification of Psychopathology* (1984), and by Stuart Kirk and Herb Kutchins in *The Selling of the DSM* (1992). Both books consider the activities of an "invisible college" of psychiatrists dubbed the "Neo-Kraepelinians" as being central to this story. As mentioned in Chapter Three, the "Neo-Kraepelinians" adopted a biological approach to mental illness, were interested in psychiatric nosology, and wished to bring psychiatry closer to the rest of medicine. They co-authored papers, and cited each other's work, and developed and backed both the Feighner criteria and the R.D.C. When Spitzer, who was one of their group, became chairman of the D.S.M.-III he choose the other members of the committee. Many of those he appointed were also Neo-Kraepelinians.

Members of the Neo-Kraepelinian group were the initial users of the Feighner criteria and the R.D.C. Once underway, the drive to use diagnostic criteria and to quantify symptomatology seems to have developed into something of an obsession amongst psychiatrists. While in the 1960s psychiatrists merely recorded their clinical impression regarding subjects' diagnoses and symptoms, by the 1980s researchers used batteries of diagnostic criteria and measures of symptomatology. As a typical example, White et al (1980), used the R.D.C. to select a subject population and then monitored changes in symptomatology using the Hamilton Depression Scale, the Brief Psychiatric Rating Scale, the Clinical Global Impression Scale, the Zung Self-Rating Depression Scale, and the Side Effects Checklist.

Those who think it implausible that the current universal use of ratings scales could be the result of a program instantiated by a small number of psychiatrists should bear in mind that it needn't take much for a practice to become adopted by all those conducting research in some area. Getting research papers published in psychiatry is a competitive business, and this means that researchers are forced to play safe. Papers are only accepted for publication in journals once they have been peer-reviewed, in other words once they have been checked for quality by referees who are expert in the field. Even if only a few of the potential referees of a research

[35] Blashfield 1984 ch.2

psychiatrist's work start demanding the use of explicit diagnostic criteria and of symptom ratings scales, a prudent researcher will start employing such methods.

Along with all other researchers in psychiatry, researchers in psychopharmacology used the D.S.M.-III. However, I suggest that they started to use diagnostic criteria not to meet any needs specifically associated with research on drugs, but rather because they, like all other American researchers in psychiatry, had come to think of diagnostic reliability as a prerequisite for successful research. To sum up, there is no evidence that the D.S.M.-II or the D.S.M.-III were developed to meet the needs of researchers specifically in psychopharmacology.

1.3 Treatment And Psychiatric Classification

The development of modern psychoactive drugs greatly increased the range of treatments available to clinicians. As a result, ways of deciding which treatment to give each patient had to be developed. The models of drug action discussed earlier suggest different ways in which a clinician might decide how to treat his patients. On a target symptom model a clinician should select a treatment on the basis of the patient's symptoms, while on a magic bullet model a clinician should select a treatment on the basis of the patient's diagnosis.

As discussed earlier, during the 1950s and 60s clinicians were often encouraged to treat patients on the basis of target symptoms. When the target symptom model is used to decide treatment a patient does not need to be diagnosed before they can be treated (although their symptoms do have to be clearly identified). The target symptom model fell from favour amongst psychiatrists during the 1970s, and the view that a patient's diagnosis is crucial to how they should be treated became dominant. At present, textbooks for psychiatrists, and the treatment guidelines produced by the A.P.A, are set out in a way that implies that treatments should largely be decided by the diagnosis that a patient receives, for example, chapters may be devoted to the treatment of various disorders. The introduction to D.S.M.-III states that "Making a D.S.M.-III diagnosis represents an initial step in a comprehensive evaluation leading to the formulation of a treatment plan."[36] Could a classification such as the D.S.M.-III have been developed to enable clinicians to make treatment choices?

In a few cases diagnoses of the level of specificity encouraged by the D.S.M.-III are linked to particular treatments; the use of lithium in mania is the obvious example. When such specific treatments exist this encourages psychiatrists to use criteria to carefully pick out patients suffering from the treatable disorder. However, the number of disorders that are linked to specific treatments is a tiny proportion of the 200 or so disorders that are included in the D.S.M.-III. For the most part whole families of disorders can be treated with the same drug, for example chlorpromazine can be used to treat all psychotic disorders. As such it is not necessary to decide whether the patient suffers from D.S.M.-III diagnosis 295.10 Schizophrenia Disorganized Type, or 295.90 Schizophrenia Undifferentiated Type, or whatever,

[36] A.P.A. 1980 p.11

but merely to decide that they are suffering from some form of psychosis. As most drugs treat whole families of disorders, the vast majority of the detailed diagnoses permitted by the D.S.M. are not needed to help clinicians decide which drug to give their patients. For this reason, it is implausible to claim that the hugely detailed D.S.M.-III was developed in order to help clinicians use drug treatments.

Recently D.S.M. categories have come to be more closely related to treatment choices, though by a somewhat indirect route. Increasingly health care in the U.S. is being administered by managed care companies. Managed care companies agree to provide for all of a subscriber's medical needs for a fixed yearly fee. The company owns medical centres, and often hospitals, and employs salaried medical staff. In order to curtail its costs the companies often have strict guidelines setting out the treatment that patients can receive for particular disorders. A company may lay down, for example, that its physicians will treat all patients that meet the criteria for a major depressive episode with X treatments and Y drugs.[37] However, any pressures resulting from the use of D.S.M. categories in such treatment guidelines cannot really be said to stem specifically from the use of drugs in treatment. Even when talking therapies are employed, company guidelines can link the amount and type of treatment to the patient's diagnosis.

Nevertheless, I suggest that the use of drug treatments has had some effect on the D.S.M. When an effective treatment for a disorder becomes available, that disorder will sometimes come to appear more prevalent This happens partly because marketing campaigns and discussion in the psychiatric literature prime psychiatrists to be on the lookout for cases of the disorder. In addition, many patients are borderline between two or more diagnoses and when an effective and relatively safe treatment is available for only one of these diagnoses it is in the patient's interest to be given the diagnosis that is treatable. When such patients are given the treatment they may get better and, providing the treatment has few side-effects, they have little to lose by trying. Two examples of disorders where diagnosis rates have increased following the introduction of an effective treatment are mania and depression. After the introduction of lithium in the late 1960s, an "epidemic of mania appeared to sweep through the eastern United States".[38] Diagnoses of mania also increased in the U.K. Symonds and Williams (1981) analysed the increase in British hospital admission rates for mania between 1970 and 1975. Following the successes of lithium treatment, they suggest that a "positive mental set with regard to the treatability [had been] induced, thus increasing the probability of diagnosis".[39] More recently, there has been a huge increase in diagnoses of depression following the introduction of serotonin selective antidepressants (Prozac and similar drugs). Prozac came on the market in 1987 and, at least until very recently, was thought to have few side-effects compared to the older antidepressants. In the U.S., patient visits to psychiatrists and primary care physicians for depression increased from 10.99 million in 1988 to 20.43 million in 1993.[40] When a successful drug treatment

[37] Tucker 1998 p.159, Luhrmann 2000 ch.6.
[38] Kendell 1975b. p.75
[39] Symonds and Williams 1981 p.195
[40] Pincus et al. 1998

for a disorder is developed the perceived boundaries of the disorder will expand. In time this change may come to be reflected in official diagnostic criteria. Even when no changing in the wording in the criteria occurs, the perceived boundaries of the category may shift as the criteria can come to be interpreted more or less strictly.

To summarise this section: A D.S.M.-style classification system is not required to enable clinicians to decide how to treat their patients. The vast majority of treatments are effective for whole families of disorders and so even when physicians employ a magic bullet model of drug treatment only rough diagnoses are normally required before a drug can be prescribed. Still, the use of psychoactive drugs to treat patients can produce shifts in the perceived boundaries of diagnostic categories. When being diagnosed as suffering from a particular disorder carries practical benefits, the perceived boundaries of that disorder will expand.

1.4 Marketing And Psychiatric Classification

Pharmaceuticals are big business in the U.S. In 2000 prescription drug sales in the U.S. reached $145 billion, up 14.9% on the previous year.[41] The drug companies spend, and always have spent, huge amounts on advertising. In 1961 pharmaceutical companies were spending $5000 per doctor per year on drug promotion[42]. The marketing bill in 1999 came to $13.9 billion[43], and topped $8 billion for the first six months of 2000.[44] In addition to money spent on explicit advertising, drug companies also spend huge amounts sponsoring conferences, journal supplements, American Psychiatric Association projects, and so on. Some of the projects that are sponsored can be seen to further the aims of drug companies fairly directly. Drug companies have sponsored conferences on drug treatments since the First International Congress on Neuro-Pharmacology. [45] In other cases, for example by sponsoring Fellowships in Psychiatry and, in the past, even Fellowships for theological students who wish to become chaplains in mental hospitals,[46] drug companies can only benefit indirectly, via gaining the good-will of mental health professionals.

In *The Antidepressant Era* (1997) and *The Creation of Psychopharmacology* (2002) David Healy claims that marketing by the pharmaceutical industry, and the regulations designed to control that marketing, have affected how both drug treatments and mental disorders are conceptualised. He thinks that many of the pressures stemming from marketing and its regulation have helped promote classification systems such as the D.S.M.

Before a drug can be marketed in the U.S. it has to be given a license by the Food and Drug Administration (F.D.A.). The F.D.A. will only grant a license if the drug can be shown to be effective and safe. Since 1951 the F.D.A. has granted

[41] I.M.S. Health 2001
[42] Wortis 1961
[43] I.M.S. Health 2000a.
[44] I.M.S. Health 2000b.
[45] Bradley et al. 1959
[46] News and Notes. 1958

licenses only for compounds that treat conditions for which medical experts agree medications are needed. Healy claims that in practice this means that the F.D.A. is in the business of licensing medications for specific diseases. Thus schizophrenia, diabetes, or depression would be appropriate indications, while nervousness and agitation would not. This, Healy claims, has "powerfully reinforced categorical and medical models [as exemplified by the D.S.M.] as opposed to dimensional models of psychiatric disease".[47]

It is hard to know what to make of this claim. Many medicines are for indications other than specific diseases - consider pain-killers, diuretics, and so on. In the light of this, Healy must be interpreted as claiming that the F.D.A. only grants licenses for psychoactive drugs, as opposed to drugs in general, if they are for the treatment of a specific disease. If this is true, it is certainly a more recent development than Healy's date of 1951 suggests. A 1970 article by employees of the F.D.A. notes that drugs were being produced for indications such as the "amelioration of morbid anxiety, elevation of depressed spirits, restoration of natural sleeping cycle, alleviation of pains, aches and discomfort, relief of fatigue, facilitation of the capacity to bear stress".[48] The authors explain that the F.D.A. is needed to ensure that such drugs are as safe and effective as their manufacturers claim, but there is no suggestion that there is anything suspect about the indications for which the drugs are being developed. Unfortunately, Healy provides no references to support his claim that F.D.A. regulations have forced drugs to be produced only for the treatment of specific diseases, and I have been unable to find any evidence that supports him.

Healy also claims that pressures stemming from pharmaceutical companies have reinforced models that either lump together or split up disorders, as best serves the drug companies interests. On occasion, the interests of the pharmaceutical industry have been served by models that lump conditions together. In *The Creation of Psychopharmacology* (2002) Healy discusses the case of catatonia. Catatonia, he argues, is a distinct disorder from other types of schizophrenia, and furthermore it is a disorder for which there are cheap and effective treatments. However, as these treatments are old and unpatentable, the interests of pharmaceutical companies have been served by lumping catatonia with other types of schizophrenia. As a result, drugs companies sell more antipsychotics, but patients suffering from catatonia are being denied effective treatments.

While on occasion the interests of drugs companies have led them to promote models that lump disorders together, for the most part, Healy thinks, their interests have been served by models that create more and more niche diagnoses. When a drug company is trying to market its drug in the face of competition there are several strategies it may employ. It may try to claim that its drug is a more effective treatment than the competitors. If this fails, the company may claim that its drug is just as effective as the competitors but also has certain other advantages, for example the drug might be marketed as having fewer side-effects, or as being cheaper, or as coming in a form that is more acceptable to patients. Alternatively, the company may try to find, or create, a niche market for the drug. Maybe the drug is

[47] Healy 1997 p.103
[48] Goddard and Alan 1970 p.451

no better than any other antidepressant for depression in general, has no fewer side-effects and is no cheaper, but if the company can claim that the drug is better than the others for treating some specific sub-type of depression - reactive depression, atypical depression, depression in the elderly, or whatever - then it may still be able to make a profit from the drug.

Healy claims that niche marketing by pharmaceutical companies has played a key role in establishing particular disorders.[49] As well as marketing their drugs directly, pharmaceutical companies market them indirectly, via promoting awareness of the conditions that their drugs treat. By sending psychiatrists reprints of papers, and sponsoring conferences and screening programs, a pharmaceutical company can raise the profile of a disorder, and raising the perceived prevalence of a disorder will increase the sales of the drugs that treat it. For example, Eli Lilly the manufacturers of Prozac advertise Prozac directly by placing adverts in journals, but they also increase sales by sponsoring projects that increase awareness of depression, such as depression screening for World Mental Health Day.[50]

Healy's main example of a disorder that has become prominent as the result of drug company marketing is depression itself. Healy claims that when the first antidepressants became available there was practically no U.S. market for them. In the late fifties almost all severely ill psychiatric patients were thought to suffer from schizophrenia and depression was rarely diagnosed. Thus, Healy claims, in order to make a profit from the antidepressants, the drug companies first had to make a market for them. For example, Merck, the manufacturers of amitriptyline, promoted the diagnosis of depression by buying and distributing 50,000 copies of Frank Ayd's 1961 book *Recognizing the Depressed Patient.*

This is Healy's least convincing example. Healy bases his claim that drug companies needed to create a market for antidepressants on the fact that depression was rarely diagnosed amongst American hospital inpatients when the antidepressants became available. I will argue that despite this at the time the antidepressants were introduced markets for them already existed amongst both inpatient and office-patient populations.

A market for antidepressants existed amongst inpatients despite the fact that few were diagnosed as suffering primarily from depression. This situation could arise because although depression was perceived as occurring rarely as a disease-entity in its own right, it was thought to occur often as a syndrome associated with other mental diseases. While few patients were diagnosed as suffering simply from depression, depression as a syndrome was often seen in patients with schizophrenia, neuroses, organic brain disorders, alcoholism, mental deficiency, and every other type of mental disorder as well as physical health problems. [51] As discussed earlier, in the 1950s and 60s drugs were often administered in accordance with a target

[49] Healy 1997 ch. 6, Healy 1996.

[50] Eli Lilly 1999

[51] Malitz 1966 p.477 "A syndrome rather than a specific disease entity, depression may be present in every type of psychiatric disorder", p.480 imipramine can help depression that occurs in manic-depression, involutional depression, psychoneurosis, Parkinsonism, Huntington's, schizophrenia, alcoholism and mental deficiency, a similar list is given by Cole and Davis 1967.

symptom model of drug efficacy. As such, all these patients were candidates for treatment with antidepressants.

In any case, in the 1950s and early 60s, drugs companies did not consider inpatients to make up the main market for new drugs. Inpatients tended to be poor and so could not afford to pay high prices. As late as 1966, some state hospitals were still using the obsolete alkaloids of rauwolfia because they could not afford phenothiazines.[52] As inpatients were considered to constitute a poor market, pharmaceutical companies often preferred to market drugs as being for the treatment of outpatients. In the 1960s, even powerful neuroleptic drugs such as Thorazine (chlorpromazine), Compazine (prochlorperazine) and Stelazine (trifluoperazine) were advertised as being for the treatment of office patients rather than for the treatment of inpatients.[53] Meprobamate, a minor tranquilliser with 1955 sales totalling nearly 2 million dollars, was the paradigm big-earning drug.[54] In the late fifties, the tranquilliser was reportedly being served at fashionable parties and addiction was widespread.[55] When the antidepressants were developed big money could be made by selling drugs for office patients.

Nathan Kline's recollections of the excitement that followed his 1957 discovery that iproniazid could be used as a "psychic energiser" further support my claim that sizeable markets existed for antidepressants on their introduction. Kline writes, "Probably no drug in history was so widely used so soon after the announcement of its application in the treatment of a specific disease."[56] About 400,000 depressed patients were treated in the first year following the discovery, and *The New York Times* followed the story.[57]

I have argued that a sizeable market already existed for antidepressants when they were introduced. As such Healy's story, according to which drug companies were forced to establish the disorder of depression in order to create a market for their drugs, fails to stand up to scrutiny.

Healy's other examples may be considered together. As previously discussed, Panic Disorder came to be included in the D.S.M.-III largely as a result of Donald Klein's studies showing that anxious patients with panic attacks respond to imipramine, while other anxious patients do not. Later these patients were shown to also respond to Xanax (alprazolam) a benzodiazepine which, unusually for drugs in its class, has antidepressant effects. Upjohn, the manufacturers of Xanax, was faced with a difficult task in marketing the drug because clinicians were becoming increasingly wary of prescribing benzodiazepines for anxiety disorders. Faced with this problem, Upjohn decided to market the drug for panic disorder, which was then rarely diagnosed. In order to increase the market for their drug, Upjohn marketed panic disorder by sending reprints of relevant articles to physicians and sponsoring

52 Malitz and Hoch 1966 p.460
53 *American Journal of Psychiatry* adverts 1960
54 Greenblatt and Shader 1971 p.1298
55 Wortis 1961
56 Cited by Kramer 1994 p.48 from Kline "Monoamine Oxidase Inhibitors: An Unfinished Picaresque Tale" pp.194-204 in Frank Ayd and Barry Blackwell (eds.) (1970) *Discoveries in Biological Psychiatry* (Philadelphia: J.B.Lippincott)
57 Kramer 1994 pp.47-48

studies on the condition. Eventually panic disorder came to be regarded as relatively common.

Geigy, the manufacturers of clomipramine, had originally intended to market it as an antidepressant. However, faced with evidence that the drug is no more effective than other antidepressants and has a worse side-effect profile, the F.D.A. refused to grant a license for clomipramine for this indication. Following some reports that the drug might have beneficial effects in anxiety, obsessional, and phobic states Geigy decided to market the drug for the treatment of Obsessive Compulsive Disorder. Again, following marketing by the company, the perceived prevalence of the disorder greatly increased.

Social phobia is one of the latest disorders to be promoted by the pharmaceutical companies. Companies with reversible inhibitors of monoamine oxidase-A (R.I.M.A.s) are particularly interested in the disorder. R.I.M.A.s are a new class of antidepressant, developed just after the S.S.R.I.s. This meant that their obvious potential market, depression, had already been taken. Following reports that the M.A.O.I., phenelzine, is effective in treating social phobia, the R.I.M.A.s were also tested for this indication and were found to be effective. At the time Healy wrote his book, Roche were seeking a license for their R.I.M.A., moclobemide, to be used to treat social phobia. Roche had already begun work on increasing the potential market for their drug by sponsoring W.H.O. campaigns aimed at educating physicians about the disorder.

In these three cases, antidepressants that could not compete in the main depression market were, or are being, marketed at a particular niche. Such marketing helps bring the niche disorder to prominence. Healy is right in claiming that this helps to draw attention to the disorder - for a time. Healy, however, has failed to examine what happens after marketing has increased the perceived prevalence of a disorder to the extent that a sizeable market exists.

Once the niche market is enlarged sufficiently, other companies try to gain licenses so that they can also market their antidepressants for the treatment of the niche disorder. When successful this results in drugs that are licensed for the treatment of one or more of the niche diagnoses as well as for depression. Thus Prozac (fluoxetine HCl) is licensed for the treatment of depression, obsessive compulsive disorder, and bulimia; Zoloft (setraline HCl) is licensed for the treatment of major depression, panic disorder, and obsessive compulsive disorder; and Paxil (paroxetine HCl) is licensed for depression, panic disorder, and obsessive compulsive disorder.[58] The existence of drugs that treat a variety of disorders tends to encourage the lumping together of the disorders. For example, Hudson and Pope (1990) suggest that because major depression, bulimia, panic disorder, obsessive-compulsive disorder, attention deficit disorder with hyperactivity, cataplexy, migraine, and irritable bowel syndrome all respond to a variety of antidepressants they might all be considered as manifestations of "Affective Spectrum Disorder".

Healy has given us some examples where the marketing of drugs for niche diagnoses helped to establish disorders. He sees niche marketing as being one factor that has promoted classification systems like the recent editions of the D.S.M. I have

[58] From adverts in the *American Journal of Psychiatry* January and February 1999

suggested that in addition to niche marketing being able to help establish disorders, the marketing of drugs for a broad range of disorders creates pressures for disorders to be lumped together. To find out which kind of marketing occurs more often I looked at adverts published in the *American Journal of Psychiatry* since the 1950s. I found that although pharmaceutical companies do sometimes advertise their drugs as being for niche diagnoses, this is a relatively unusual marketing ploy. Far more frequently adverts market drugs at a broad range of disorders, most commonly "psychotic disorders" or "depression". Drugs are more frequently aimed at broad ranges of disorders than at niche diagnoses.

However, although drugs are more frequently marketed as treating broad ranges of disorders, on balance I suggest that Healy is right and that overall marketing will have tended to reinforce the tendency for ever more disorders to be included in the D.S.M. I suggest that this can happen because once diagnoses come to be included in the D.S.M. they are rarely removed. Although the classification system is revised at regular intervals, the default position is that categories remain from edition to edition. Thus, once a diagnosis has been brought to prominence via niche marketing, and has become locked into the D.S.M., even if it then ceases to be a focus of attention it will tend to remain in the D.S.M. As a result, over time, the D.S.M. collects categories like an attic collects junk.

Of course, marketing strategies will vary depending on the audience at which the adverts are aimed. Recently U.S. pharmaceutical companies have started advertising psychoactive drugs directly to potential patients. In 1998 $1.3 billion was spent on adverts aimed at the general public, and such advertising is increasing at such a rate that the same sum only covered the bill for the first six months of 2000.[59] As a marketing strategy, advertising directed at patients makes some sense in the U.S. context. Although the treatments available to patients treated under Managed Care schemes are limited, patients who are willing to pay for treatment out-of-pocket often have far more power in the doctor-patient relationship than do patients in many other healthcare systems (such as the U.K.'s National Health Service, for example). U.S. patient support groups on the internet assume that patients will be able to determine who treats them and which drugs they are prescribed. The "Anxious Advocate", for example, advises patients to try taking Neurontin, a drug usually used as an anti-epileptic.[60] The advocate admits that "doctors are laughing at my 0.5 mg/day", and yet he manages to find a doctor willing to prescribe it for him and assumes that other patients will be able to do likewise. Another web-site provides questions that patients can ask their doctors in order to determine whether they're truly expert psychopharmacologists - the implication is that doctors who fail the test should be replaced.[61]

As drugs come to be increasingly advertised directly to patients they will doubtless come to be marketed differently. This may result in the pressures that marketing exerts on psychiatric classification changing. Certain mental diseases, most obviously schizophrenia, are heavily stigmatised, and few people would be

[59] I.M.S. Health 2000b.
[60] Anxious Advocate 1997
[61] Dr Ivan 1998

prepared to go and ask their doctor for a drug that is marketed for such an indication. An interesting possibility is that this may lead to drug companies avoiding marketing drugs for such disorders and, in the long term, to these disorders being less frequently diagnosed. Healy also draws attention to the fact that the users of psychoactive drugs may not wish to see themselves as being ill at all. Instead of being marketed for the treatment of disorders, drugs may come to be marketed for vaguer indications. This trend already appears to be underway. At least in the U.K., St John's Wort, which is taken for depression and can be bought over the counter, is frequently sold in packets that either give no information as to its use, or only offer vague hints. For example, the Kira brand of St John's Wort tablets are sold "To help achieve emotional wellbeing".[62] An increased move towards marketing of this kind, Healy suggests, would tend to break up the medical model within which psychiatric classification currently operates.

To summarise this section: niche-marketing by pharmaceutical companies, as described by Healy, can help establish mental disorders. Drugs are also frequently marketed for a broad range of disorders which, to some extent, encourages disorders to be lumped together. However, as once categories are included in the D.S.M. they are rarely removed, over time the marketing practices of pharmaceutical companies have tended to reinforce the trend for the D.S.M. to include ever more disorders.

Recently pharmaceutical companies have started advertising psychoactive drugs directly to potential patients. As advertising strategies will vary depending on the intended audience, in time this may lead to marketing affecting psychiatric classification in new ways.

1.5 Conclusions

I have shown that the D.S.M. has been shaped far less by the needs of those developing and prescribing psychoactive drugs than has commonly been supposed. Researchers working on the development of psychoactive drugs make no more use of psychiatric classification systems than other researchers in psychiatry. Detailed psychiatric classification systems, such as the recent editions of the D.S.M., were not developed to meet the needs of researchers in psychopharmacology.

The D.S.M. has been shaped to some extent by the use of drug treatments. On occasion disorders have been distinguished on the basis of their response to drugs. Thus, anxiety disorder characterised by panic attacks was distinguished from other types of anxiety disorder on the basis of its response to imipramine. Sometimes the perceived boundaries of a disorder expand when a successful drug treatment is found - thus depression has come to be diagnosed far more frequently following the development of Prozac. Even when no changing in the wording of diagnostic criteria results from such increased diagnosis, the perceived boundaries of the category will shift as the criteria come to be interpreted more or less strictly. However, in general, a classification system as detailed as recent editions of the D.S.M. is not needed to inform drug treatment decisions. Although some particular diagnoses are tied to

[62] Think Natural Shop 2002

particular treatments, in general drugs can be used to treat broad classes of disorders and so there is no need to for a specific D.S.M. diagnosis to be given before treatment can commence.

As Healy has argued, niche marketing by drug companies can help establish diagnoses. Once included in the D.S.M., the default position is that categories remain from edition to edition. Thus, even once marketing ploys shift, a diagnosis once brought to prominence by niche marketing and included in the D.S.M., will tend to remain. Over time marketing by drug companies will have supported the tendency for more and more disorders to be included in the D.S.M.

Later in this chapter I will consider whether the pressures on psychiatric classification stemming from drug treatments make it more or less likely that the categories in the D.S.M. will correspond to natural kinds of disorder. First, however, we will turn to my second and contrasting case study, which examines the effects of medical insurance on the D.S.M.

2.SECOND CASE STUDY: INSURANCE AND THE D.S.M.

While the A.P.A. claims to have designed the D.S.M. with the needs of psychopharmacologists in mind, they have sought to distance themselves from the use of the D.S.M. by the medical insurance industry. Those responsible for the most recent editions of the D.S.M. present it as a scientific classification system based on sound empirical evidence. They claim that conditions are included in the D.S.M. if there is sufficient evidence to suggest that they are valid disorders, and excluded otherwise. Social, political, and fiscal needs, and in particular, the requirements of medical insurance, have been ignored, they maintain. In this case study I will show that this is not true. Rather, the D.S.M. has been, and continues to be, substantially shaped by pressures arising from its use as a nomenclature for completing insurance forms.

2.1 The Forging Of Links Between Insurance And The D.S.M.

Up until the 1930s, Americans paid the vast majority of medical costs out of pocket. Prior to the development of high-tech medicine, medical care was cheaper in real terms than it is today, making out-of-pocket payment feasible for a larger proportion of the population. Those who could not afford to pay for treatment were either accepted as charitable cases or went without. Poor psychiatric patients were in a somewhat better position than others as the states funded mental hospitals that provided free care.

During the Second World War employers began offering health insurance and health services as fringe benefits in lieu of wage increases, which were forbidden by wartime incomes policy.[63] After the war the percentage of the population possessing medical insurance for at least emergency hospital care grew rapidly, increasing from

[63] Glaser 1978 p.182

10% in 1940 to 74% in 1961.[64] Insurance for mental health care failed to develop as quickly as insurance for general medical care, however. As the states were traditionally responsible for the care of the mentally ill, insurance appeared unnecessary. In addition, insurers doubted whether insurance coverage for mental health care was financially viable. Psychiatric treatment was perceived as consisting either of expensive psychotherapy for persons whose illness was debatable, or of custodial care for the incurably insane, neither of which were attractive to insurers.[65] As a result, when the D.S.M.-I was published in 1952, insurance for mental health care was extremely rare.

By the late 1950s the A.P.A. was becoming concerned that insurance for mental health care was lagging behind insurance for general medical care. With funding from the National Institute for Mental Health, the A.P.A. organised a pilot scheme aimed at demonstrating the viability of insurance for short term mental health care.[66] The scheme was intended to offer a model of good practice for future schemes. As such, the scheme's conditions on coverage and the mechanisms of reimbursement give some indication of the direction organised psychiatry wished insurance for mental health care to take. The scheme permitted up to fifteen sessions of individual psychotherapy for any condition treated by a psychiatrist. Patients contributed 25% of the cost of treatment out of pocket. Leaflets informing participants of the benefits offered by the scheme adopted a social approach to mental health problems - vignettes described how "chats" with a psychiatrist helped people with problems at work, with their children, and with alcohol abuse. Most interestingly for my argument, the form used for claiming reimbursement, which had been designed by the A.P.A., requested a D.S.M. diagnosis.

Although the official position of the A.P.A. was pro-insurance, many of the psychiatrists involved in the scheme were more ambivalent. The majority of the psychiatrists adopted a psychoanalytic approach to mental illness. They found it difficult to label their patients with a diagnosis and, despite the scheme requiring that D.S.M. diagnoses be given, over 50% of the patients were undiagnosed. Many analysts found the fifteen sessions permitted by the scheme inadequate; others worried about the malignant effects third-party interference might have on the analyst-patient dyad. Traditionally psychoanalysts have believed that patients should make a financial sacrifice in order to reap the full benefits of treatment. Presumably following this line of thought, 10% of the psychiatrists thought that the scheme patients were not as well motivated as those who have to pay entirely out of pocket, and 98% thought it would be a bad idea if treatment was provided free at source. These concerns would be echoed for decades to come.[67]

Following the pilot scheme, private schemes including mental health coverage began to develop and it became standard practice for a D.S.M. diagnosis to be required for reimbursement. The use of diagnoses on insurance forms produced new incentives and disincentives for psychiatrists to record particular diagnoses.

[64] Scheidemandel et al. 1968 p.5
[65] Sharfstein 1987 p.532
[66] Avnet 1962
[67] For examples of similar worries see Chodoff 1972, Gabbard and Lazar 1997.

Unlike the A.P.A.'s pilot scheme, commercial schemes often excluded certain conditions from coverage. For example, many insurance schemes would not pay for problems couched in terms of "interpersonal difficulty", or for the treatment of alcoholics. Psychiatrists found ways to get round these restrictions. Patients with marital problems or alcoholism could be reported as suffering from a "depressive" or "anxiety" neurosis. Such practices appear to have been widespread. When the coverage of one plan was changed to include alcoholism, thus removing the need for such cover-up diagnoses, alcoholics were found to already make up over five percent of the case load.[68] During the development of the D.S.M.-III one regional branch of the A.P.A. argued that the category "neurosis" should be retained to be used as a "cover-up diagnosis" for patients with personality disorders as these patients would not otherwise be covered for psychotherapy.[69]

Insurance forms were generally returned via the patient's employer or by the patient, thus psychiatrists did not want to record a severe or socially unacceptable diagnosis.[70] Studies found that the diagnoses submitted to insurance companies tended to downplay the severity of patients' disorders. A 1977 study comparing insurance claim and confidential diagnoses found that 5.4% of the patients were schizophrenic according to the insurance forms compared with 10.4% for the confidential diagnoses. Neuroses made up 70.6% of the insurance claim diagnoses, but only 28.4% of the confidential diagnoses.[71] Although by the late eighties the psychiatric literature would condemn such diagnostic manipulation as unethical, in the sixties such practices were often depicted as pragmatic necessities justified on compassionate grounds.[72]

In their study of the International Classification of Diseases (the I.C.D.), Geoffrey Bowker and Susan Star have found evidence of somewhat similar effects. Categories from the I.C.D. are used in the tables of causes of death that each country submits to the W.H.O. In some countries the diagnosis used for statistical purposes is also recorded on the death certificate and is thus known to relatives. In other countries it is confidential, and a different cause of death may be publicly announced. Whether the diagnosis is confidential makes a difference to the diagnoses that are made. In 1927 the Netherlands switched over to a system in which the statistical cause of death was confidential. As a result,

> There was a considerable increase in Amsterdam of cases of death from syphilis, tabes, dementia paralytica, aneurysm, carcinoma, diabetes, diseases of the prostate, and suicide, while deaths from benignant tumors and the secondary diseases such as encephalitis, sepsis, peritonitis, and so forth showed a falling off.[73]

[68] Green 1969 p.684

[69] Laufe 1979

[70] Chodoff 1972, Grossman 1971

[71] Sharfstein et al. 1980

[72] Compare Green 1969 and the discussion of diagnostic manipulation in Spiegel and Kavaler 1986 pp.128-131

[73] Bowker and Star 1999 p.141 Citing League of Nations, "International Lists of Causes of Death adopted by the Fifth International Conference for Revision, Paris, October 3-7, 1938" In *Bulletin of the Health Organisation*, (1938), 944-987.

Clearly, it is not only psychiatrists who are willing to massage diagnoses to protect their clients.

In some cases psychiatrists intentionally record diagnoses that are not clinically warranted in order to help their patients. In addition, I hypothesise that economic and social considerations can influence the diagnoses psychiatrists make in more subtle ways. Psychiatrists regularly disagree over diagnoses and many patients are borderline cases. Thus financial and social incentives will have an effect not only when a psychiatrist records a false diagnosis, but also when a psychiatrist who could equally well make one of several diagnoses is motivated to favour one possible diagnosis over the others. Under such conditions, when finding cases of a particular type is rewarded, cases of that kind will be found. In time this will result in diagnostic boundaries changing, as once one case is perceived as a case of, say major depression, similar cases encountered later will come to be grouped with it. Sometimes these changes will come to be reflected in official diagnostic criteria. Even where no changing in the wording of criteria occurs, the effective boundaries of the category may still expand or shrink over time, as criteria can come to be interpreted more or less strictly. In this way the use of D.S.M. diagnoses in completing insurance forms could lead to new pressures on diagnosis that in time could feedback and have an effect on accepted diagnostic criteria.

Throughout the sixties, insurance for mental health care became more common and the A.P.A. continued to encourage the inclusion of D.S.M. diagnoses on insurance reimbursement forms.[74] Prior to the D.S.M.-III those responsible for the D.S.M. freely admitted that its primary functions included providing a nomenclature for completing insurance forms. Indeed Henry Brill (chairman of the A.P.A.'s Committee on Nomenclature and Statistics) seemed to have difficulties thinking of many other potential uses for the D.S.M. In his chapter on Classification and Nomenclature included in the 1959 *American Handbook of Psychiatry* Brill notes that a sound knowledge of the D.S.M. nomenclature may aid clear thinking and provides access to the psychiatric literature, and then continues "classification often directly involves the welfare of the clinician's patients in such matters as eligibility for insurance, compensation or other disability allowances, or legal responsibility and competence."[75] Brill publicly considered altering the D.S.M. to tailor it more closely to the requirements of the insurance industry. At a 1965 conference he noted that the A.P.A. should consider "the need for a functional classification as a supplement to the diagnosis in medical reports to insurance companies and to the Social Security Agency for determining eligibility for disability benefits."[76] Others involved in the development of the D.S.M. viewed the growth in insurance coverage optimistically, in part because it would lead to greater usage of the D.S.M. nomenclature.[77]

[74] Grossman 1971 p.68 cites American Psychiatric Association (1969) *Guidelines for Psychiatric Services Covered Under Health Insurance Plans. 2nd ed.* (Washington DC, A.P.A.)

[75] Brill 1959 quote p.5

[76] Visotsky and Bahn 1965

[77] Kramer 1965 p.114

By the 1970s the fact that insurance companies required D.S.M. diagnoses was beginning to pay dividends for the A.P.A. Through publishing the D.S.M. the A.P.A. now controlled part of the mechanism for funding mental health care - a development that was viewed with considerable alarm by the other mental health professions. First, it meant that all mental health professionals had to buy and use the D.S.M. A study of clinical and counselling psychologists, for example, revealed that although only 17% considered the D.S.M. to be a satisfactory classification system, 90.6% used the D.S.M.-II, 86.1% noting that they were required to use it to obtain insurance reimbursement.[78] While psychologists and other mental health professionals resented being forced to use a nomenclature based on fundamental assumptions foreign to their own, sales from the D.S.M. contributed significantly to the A.P.A.'s revenue.

Second, control over the nomenclature used for reimbursement potentially allowed the A.P.A. to define disorders in such a way as to help justify the claim that only psychiatrists should treat them. As discussed in Chapter One, when the American Psychological Association learnt that the A.P.A. was considering defining mental disorders as a sub-set of medical disorders in the introduction to D.S.M.-III they were so concerned they immediately sought legal advice as to the possible repercussions of such a definition.[79] They also began to develop an alternative manual "to promote the professionalization of psychology through a system of economic reimbursement free of medical dominance".[80]

Ultimately, the development of an alternative classification scheme proved to be beyond the means of the American Psychological Association,[81] but the reports of the Task Force responsible for the manual demonstrate how important it had become for any potential competitor to the D.S.M. to be accepted by the insurance industry. The Task Force recognised that the manual's widespread use would be conditional on insurance company acceptance, and were thus sensitive to the needs of the insurance industry. At one point the Task Force discussed constructing a manual based on a crisis model of behaviour according to which there is a crucial period 4-6 weeks after a life event during which behaviour stabilises at a new level of functioning. One of the advantages of such a model was that "specification of an average probable duration of treatment would be a package which would be ultimately saleable to insurance carriers"[82].

To summarise this section: Prior to the development of the D.S.M.-III the A.P.A. publicly encouraged insurance company use of the D.S.M. By the late sixties much mental health care was funded by medical insurance and insurance carriers routinely required a D.S.M. diagnosis for reimbursement. This created new pressures on diagnoses. Psychiatrists began massaging diagnoses in order to secure insurance reimbursement for their patients, or to protect them from stigma. In addition, the integration of the D.S.M. into the fabric of the economic support of mental health

[78] Miller et al. 1981
[79] Carter 1977
[80] Miller et al. 1981 quote p.389
[81] Board of Directors Minutes 1974-1978 December 1-2 1978
[82] Task Force on Descriptive Behavioural Classification 1977

care bolstered the position of the D.S.M. as *the* classification of mental disorders and provided the American Psychiatric Association with a potentially powerful weapon in power struggles with the other mental health professions.

2.2 The Construction Of The D.S.M.-III

In the 1970s organised psychiatry felt itself attacked on all sides. The growth of the other mental health professions was seen as threatening psychiatry's dominance of the mental health sector and the anti-psychiatry movement was in full swing. While the D.S.M.-I and D.S.M.-II had been peripheral to the self-image of organised psychiatry, a newly defensive psychiatry would make extensive use of the D.S.M.-III in its efforts to reassert itself as a scientifically based branch of medicine.

From 1973, when work began on developing the D.S.M.-III, the new manual was intended to be used to help bolster the image of psychiatry. Amongst other intended functions the A.P.A. hoped that the D.S.M.-III would help to define the boundaries of psychiatry.[83] In this way the D.S.M. would simultaneously help to defend psychiatry against anti-psychiatric claims that psychiatrists were treating "problems in living" rather than genuine disorders, and also help protect the psychiatric field from encroachment by other mental health professionals.

The increasing importance of the D.S.M. to organised psychiatry is reflected in the physical appearance of the different editions. While the D.S.M.-I and D.S.M.-II were cheap, spiral-bound, and soft-backed, the D.S.M.-III was a well produced hard-back, and the D.S.M.-IV is available in a leather bound edition designed for desk top display. By 1994 the D.S.M. had become sufficiently symbolic of the A.P.A. that it chose to celebrate its 150th birthday by reproducing the D.S.M.-I in a leather-bound limited edition.[84]

As the D.S.M. became central to the self-image of organised psychiatry asserting the scientific status of the D.S.M.-III became an integral part of asserting the scientific status of psychiatry. As such, those responsible for the D.S.M.-III felt compelled to publicly affirm its scientific purity. Over and over again they claimed that economic factors in general, and insurance considerations in particular, had no influence on their thinking. For the first time in the D.S.M. series, the introduction to the D.S.M.-III contains a warning that the classification is not intended to be used to justify third-party payment.[85] At an A.P.A. conference participants "closely involved" in the construction of the D.S.M.-III "emphasised that administrative and fiscal considerations had never entered their thinking and that the integrity of the nomenclature might be endangered if it were to be constructed with thoughts of reimbursement policy."[86] In an interview with *Psychiatric News*, Spitzer, the Chairman of the D.S.M.-III committee, claimed that "none of the changes [to the

[83] Executive Committee 1973
[84] A.P.A. 1952b.
[85] A.P.A. 1980 p.12
[86] Michels 1987 p.540

D.S.M.], 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."[87]

Despite this new, public face of the D.S.M., documents in the archives of the A.P.A. show that the committees responsible for the classification system continued to be interested in insurance. The A.P.A. Task Force on Nomenclature and Statistics met in 1974 to draw up a list of the functions they hoped the D.S.M.-III would serve. Amongst other functions they included acting as a basis for insurance reimbursement.[88] When asking the National Institute of Mental Health to financially support the development of the D.S.M., the A.P.A. was happy to stress its importance for private and public insurance.[89]

Those lobbying to have changes made to drafts expected appeals to insurance requirements to be accepted as appropriate grounds for altering the D.S.M. In many cases they were not disappointed. For example, worries were voiced that the inclusion of diagnostic criteria in the D.S.M.-III might lead to insurance companies refusing to pay for cases where not all criteria were met.[90] As a result, the forward to D.S.M.-III includes a warning that the criteria are not to be employed mechanically and affirming the primacy of clinical judgement.[91] Similarly, fears that information recorded on axes IV and V of the D.S.M. multiaxial diagnosis (psychological stressors and level of adaptive functioning) might undermine patient confidentiality if recorded on insurance forms resulted in the use of these axes being left to the discretion of the psychiatrist.[92] The name of "Chronic Minor Affective Disorder" was changed on the grounds that it made no economic sense to call a disorder either "minor" or "chronic".[93] Representatives of insurance companies were consulted to see if they predicted any problems arising in the transition from D.S.M.-II to D.S.M.-III.[94]

Talk of insurance had merely gone underground, and as a result two radically different discourses concerning the development of the D.S.M. emerged. While in the letters of the D.S.M.-III committee members a private discourse involved insurance considerations at every turn, simultaneously, in psychiatric journals and at open conferences, a public discourse depicted the D.S.M. as a pure scientific classification system, unsullied by economic considerations.

The fact that third-party payers would only reimburse treatment for patients with a D.S.M. diagnosis provided an incentive for psychiatrists and patients to lobby for new disorders to be included in the D.S.M. When this lobbying was successful new disorders came to be included in official psychiatric classification systems as a direct result of the pressures imposed by the mechanisms of insurance reimbursement. In *The Harmony of Illusions* Alan Young argues that Post Traumatic Stress Disorder

[87] H.M.G. 1977
[88] Task Force on Nomenclature and Statistics 1974.
[89] Sabshin and Spitzer 1976.
[90] Jaso 1978; Kenney 1979, Fink 1978.
[91] Research and Development Council 1978b.
[92] Beigler 1978; Research and Development Council 1978a.
[93] Washington Psychiatric Association 1979.
[94] Jaso 1978.

was included in D.S.M.-III partly as a result of such lobbying. As the Veteran's Administration pays for the treatment of all combat related disorders, the inclusion of P.T.S.D. in the D.S.M. enabled thousands of mentally disturbed Vietnam veterans to obtain treatment in V.A. centres.

Since the late 1970s family therapists have been lobbying to have diagnoses suitable for describing the family problems they treat included in the D.S.M. This lobbying is explicitly motivated by a desire to obtain insurance coverage for their patients. Speaking for family therapists at the 1976 "Conference to Critically examine D.S.M.-III in Mid Stream", Rachel Gittelman stated "Interactional familial disturbances should receive diagnostic labels to enable reimbursement for family therapy from third party payment."[95] More recently The Coalition on Family Diagnosis claimed that family diagnoses were required because "American therapists want submissions for insurance reimbursement to be factually correct and ethical."[96] This formulation is of interest because it implicitly accepts that psychiatrists and therapists are willing to massage the diagnosis given to their patients if this is necessary to enable them to claim reimbursement for treatment. To date, family diagnoses are not included in the D.S.M., although efforts to develop such diagnoses for the D.S.M. continue.[97] The enterprise is difficult because the interactional model of mental illness adopted by family therapists is radically different to the medical/biological approach adopted by the D.S.M., making the development of D.S.M.-style family diagnoses problematic.

To summarise: with the development of the D.S.M.-III, selling the D.S.M. as a scientific classification system became an integral part of the A.P.A.'s efforts to boost the status of psychiatry. To some extent these tactics proved successful. An A.P.A. Task Force examining the use of psychiatric diagnoses in law courts found that requests for psychiatric reports increased after the publication of the D.S.M.-III.[98] This indicates that following the publication of a manual that was perceived to be reliable and accepted, psychiatric expertise came to be valued more highly by the legal profession.

By the time of its publication in 1980 successive drafts of the D.S.M.-III had been revised in the light of insurance considerations. Although the committees responsible for the D.S.M.-III were responsive to insurance considerations they denied that this was the case. As the D.S.M. had become increasingly important to the self-image of organised psychiatry, the A.P.A. could no longer allow talk of insurance to enter into the public D.S.M. debate.

2.3 Beyond The D.S.M.-III

Ironically, at the very time that the public image of the D.S.M. could no longer accommodate talk of insurance requirements, behind the scenes the financial pressures on psychiatric diagnoses were soon to become stronger than ever before.

[95] Sletten et al. 1976 p.20
[96] Quote from Kaslow 1993 p.259. Also Wyne 1987.
[97] See, for example, Kupfer et al. (eds) 2002. Ch.4.
[98] Task Force Report 32 1991

At the global level U.S. health care has been in crisis since the mid-eighties. Throughout the eighties, health care costs rose at two to three times the rate of inflation. By 1992 health care costs consumed 13.3% of the G.N.P.,[99] which compared with around 6% in Britain.[100] In part the astronomical cost of U.S. medicine is the result of an insurance system which until recently tended to reimburse for the cost of care. At best this provided no incentive for increasing efficiency, at worst it encouraged over treatment.

Since the mid-eighties both public and private insurers have introduced various mechanisms in an attempt to curtail their costs. These have included: increasing deductibles (the amount the patient must pay before insurance kicks in) and co-payments (the percentage of costs paid by patients); introducing peer-review, whereby an insurance company employed physician keeps an eye on the care provided; increasing Health Maintenance Organisation (H.M.O.) based health care, whereby physicians are contracted to provide for all the patient's needs for a set yearly rate; and introducing pre-payment for treatment based on diagnosis. Many of these measures make a patient's diagnosis of crucial importance in determining the care a patient receives. For example, a peer-review physician will consider whether the proposed treatment is appropriate given the diagnosis recorded, and H.M.O.'s tend to make the number of sessions of care provided depend on the diagnosis a patient receives.

In considering the effects of medical insurance on psychiatric classification the introduction of pre-payment based on diagnosis is particularly interesting. The Diagnosis Related Groups (D.R.G.'s) on which payment is based form, in effect, a diagnostic system especially created with the needs of third-party payers in mind. Developed at Yale in the early eighties,[101] D.R.G.'s were designed with the aim of predicting the amount of money a patient should cost to treat. Each patient is allocated to one of 467 classes, of which nine cover mental disorders, and six cover disorders related to drug and alcohol abuse. Each class is basically an amalgamation of several similar diagnostic categories from the International Classification of Diseases (I.C.D.), although D.R.G.'s may also be split on the grounds of secondary diagnosis, surgical procedures, age, sex, and complications. The payment system based on D.R.G.'s is designed to promote efficiency. Hospitals are paid a set amount per patient falling in each D.R.G. If the patient can be treated for less than the predicted amount then the hospital keeps the difference as profit. If the patient's treatment proves more costly than anticipated the hospital must absorb the cost, although extra money is provided for excessively complicated and expensive cases.

Medicare (the public insurance scheme for elderly people) introduced D.R.G.'s for general medical patients and psychiatric patients treated in non-specialty beds in 1983. Psychiatric patients treated in specialty hospitals, or psychiatric units within general hospitals, were granted an at first temporary and finally permanent exemption from the D.R.G. system. The exemption was based on fears that

[99] Reinhardt 1996 p.489

[100] In 2000 the U.S. spent 13.0 of G.D.P., and the UK 7.3 of G.D.P. on healthcare, indicating that the U.S. has had some success in at least controlling increases in health spending. (Anderson et al 2003)

[101] Fetter et al. 1980

psychiatric D.R.G.'s would be poor indicators of the cost of care. These worries proved to be well founded. Studies found that only 3-16% of the variation in patients' length of hospital stay was accounted for by their D.R.G.[102] Although largely abandoned by Medicare, psychiatric D.R.G.'s are being adopted by other insurers.[103] As D.R.G.'s do succeed in reducing costs, albeit in an arguably unfair manner, this trend will doubtless continue.[104]

As discussed earlier, payment mechanisms can encourage certain diagnoses to be made more than others. While in the sixties and seventies psychiatrists commonly down-played the severity of their patients' illnesses in order to protect them from stigma, as insurance companies have cut back on payments psychiatrists have started emphasising how sick their patients are in order to get paid. Recording more severe diagnoses most obviously pays when payment is based on D.R.G.'s. In such a system the amount of money the hospital receives is directly dependent on the diagnosis. Even with non-D.R.G. based payment there are incentives to stress the severity of patients' conditions; a psychiatrist who does so is less likely to have the appropriateness of the treatment questioned by reviewers. As stressed earlier, economic and social considerations can influence the diagnoses psychiatrists make more subtly than merely via encouraging fraud, as when certain diagnoses are rewarded borderline cases will receive those diagnoses rather than equally appropriate alternatives.

In addition to being motivated by their own desire to obtain insurance coverage for their patients, psychiatrists may be put under pressure to record diagnoses in particular ways by patients. In the U.S. well-informed patients know how they want their insurance forms to be completed. The patient support group C.H.O.I.C.E. (Consumers Helping Others in a Caring Environment), for example, informs patients with a history of drug or alcohol abuse that if they are to qualify for disability allowances they must make sure that their psychiatrist states that their mental illness was the cause, and not the effect, of drug use.[105] In Dr Bruce Hamstra's *How Therapists Diagnose: Seeing Through the Psychiatric Eye. Professional Secrets You Deserve to Know ...and How they Affect You and Your Family*, patients can read that "therapists sometimes 'overdiagnose' in order to justify treatment to insurance companies. Other time they 'underdiagnose' because they don't want the patient to be labelled by a more severe diagnosis."[106] Hamstra advises patients to ask what diagnosis will be recorded on their file and to discuss any possible repercussions with their therapist. Similar advice can be found on patient support web sites on the Internet.[107] At least in borderline cases, psychiatrists whose patients request that they fill in forms in a particular way are likely to comply.

There is some evidence that mental health professionals have started to respond to financial incentives to record more serious diagnoses. A 1988 survey of clinical

[102] Taube et al. 1984 p.603, Frank and Lave 1985, English et al. 1986 p.134
[103] Bowen 1997 reports that Indiana Medicaid has introduced D.R.G.s into psychiatric hospitals.
[104] Taube et al. 1988
[105] C.H.O.I.C.E. no date
[106] Hamstra 1994 p.1
[107] e.g. Mentor Research Institute, no date.

social workers found that 72% were "aware of cases where more serious diagnoses were used to qualify for reimbursement", and 86% knew of occasions when individuals were diagnosed in order to secure insurance funding despite the primary problem lying in the family system.[108] Other evidence comes from increases in Case Mix Indices, a measure of the average sickness of patients being treated that can be derived from records of patients' Diagnosis Related Groups. A study of Medicare patients found that between 1987 and 1988 the Case Mix Index had increased by 3.3%. Of this 1.6% was judged to be caused by changes in the patient population and 1.7% due to altered coding practices, i.e. upgraded diagnoses.[109] Although this increase is hardly conclusive, it is at least suggestive.

Financially motivated diagnostic creep will affect the classification systems used by psychiatrists. When finding cases falling under a particular diagnosis is financially rewarded the boundaries of that class will expand, and eventually this expansion will be reflected in the official classification systems. Even where the wording of criteria does not change, the effective boundaries of a category may shrink or expand over time as the criteria come to be interpreted more or less strictly.

Insurance considerations may also alter the boundaries of categories in more direct ways. Once a diagnosis is included in the D.S.M. the diagnostic criteria may be altered between editions so that more or fewer patients fall into the category. The A.P.A. archives have not yet obtained documents relating to the development of D.S.M.-III-R (1987) or D.S.M.-IV (1994). However, a four volume *Sourcebook* has been published which documents many of the decisions made by the various Working Groups that were involved in the development of D.S.M.-IV. The A.P.A. claimed that the D.S.M.-IV was based on objective scientific data, and the *Sourcebook* seeks to make it visible that this was so. As such, the central aim of the *Sourcebook* is to document the literature reviews conducted in revising the D.S.M.-IV.

Occasionally, however, 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."[110] Similarly the Working Group on Post Traumatic Stress Disorder notes that "requiring a minimum duration before a diagnosis of P.T.S.D. could be made might reduce help-seeking behaviour as well as reimbursement for treatment".[111]These examples show that the committees responsible for the D.S.M. continue to be sensitive to insurance considerations.

[108] Kirk and Kutchins, 1988, p.229.
[109] Frank et al. 1996
[110] Rush 1996 p.15
[111] Davidson et al. 1996 p.596

2.4 Conclusion

I have shown that the use of the D.S.M. as a nomenclature for completing insurance forms has affected the D.S.M. On occasion the committees that construct the D.S.M. have included conditions and revised diagnostic criteria to enable more patients to be reimbursed for their treatment. More subtle effects can also occur. Mental health professionals are often motivated to massage the diagnoses of individual patients. I have hypothesised that as the popularity of a diagnosis changes the perceived characteristics of that disorder will also change. If, for example, schizophrenia comes to be diagnosed more often to enable borderline-schizophrenics to gain treatment, then over time schizophrenia will come to be seen as a milder and more heterogeneous condition. I suggest that even when perceived changes do not come to be reflected in the criteria included in the D.S.M. they can affect the ways in which the criteria are interpreted in practice.

3. FEEDBACK EFFECTS IN SCIENCE

In my case studies I have shown that the D.S.M. has been affected by pressures arising from the ways in which it is used. In my first case study I argue that the D.S.M. has been shaped by the use of psychoactive drugs (albeit to a lesser extent than has often been claimed). Some disorders have been distinguished on the basis of their response to different drugs; other disorders have been brought to prominence as a result of niche marketing by pharmaceutical companies. In my second case study I show that the D.S.M. has been shaped by pressures arising from its use as a nomenclature for completing medical insurance forms. In some cases new disorders have been included in the D.S.M. so that patients can be reimbursed for their treatment, in other cases the boundaries of disorders have shifted as a result of insurance considerations. My aim in constructing these case studies has been to furnish us with examples of ways in which the D.S.M. can be affected by pressures stemming from the ways in which it is used in practice. With these case studies in hand, I will now move on to consider how such feedback effects might affect the chances that D.S.M. categories will correspond to natural kinds of disorder.

Addressing this question will require a general account of the epistemic significance of feedback effects in science. The account I will offer depends heavily on a distinction being drawn between theoretical entities and the words and ideas that surround theories. Theoretical entities, such as electrons, have particular properties, for example having such and such a mass and charge. The words and ideas that surround a theory are quite different, and have different characteristics. For example, unlike electrons the word "electron" starts with an "e" and is hard for six-year olds to spell, while the idea of an electron is associated with physicists in white coats. Crucial to the account of feedback that I shall propose is the distinction between applications that use the entities posited by a theory, and applications that use the words and ideas surrounding a theory.

3.1 Using Theoretical Entities Or Processes

Consider an application that works by interacting with theoretical entities. Let's say an application that interacts with electrons. Let's suppose that the application works, and that over time feedback results in changes being made to the theory that enable the application to work even better. I take it that in such a situation our intuition will be to say, first, that because the application works this is evidence that our theory of electrons is at least partially on the right tracks. And, second, that when feedback results in changes being made to the theory this is likely to make the theory even better. Here I will take each of these intuitions in turn and show why things are not quite so simple:

3.1.1 First Intuition. The success of an application that uses theoretical entities gives us reason to think that the theory is at least partially correct.

The claim that we have reason to believe that theories are true if they are useful has been heavily attacked by Larry Laudan.[112] His argument must be blocked if anything is to be salvaged from the first intuition. Laudan claims that many past theories, that we now consider to be false, were found useful in their day. He gives the humoral theory of medicine and the caloric theory of heat as examples. Laudan then employs a version of the pessimistic induction to argue that if past theories were useful but false, theories that are currently useful are also likely to be false.

Philip Kitcher develops one way in which Laudan's challenge can be resisted in his book *The Advancement of Science*.[113] Kitcher claims that Laudan wrongly characterises the scientific realist as claiming that the successful application of any part of a theory supports the claim that the theory as a whole is true. Kitcher claims, however, that any sensible realist would never have wanted to claim that the truth of the idle parts of a theory is supported by the success of the theory as a whole. Kitcher then analyses Laudan's example of a useful and yet false theory, the optical ether theory, and shows that, in so far as optical ether theory was successful, the ether was an idle part of that theory. For example, the ether plays no essential role in the calculations that allow Fresnel's prediction of a bright spot at the centre of the shadow of a circular disc. Laudan's argument is then blocked because he has no examples of cases where the part of a theory that was found useful turned out to be false.

Kitcher's reply to Laudan is along the right lines but needs to be developed further. The truly sensible realist should not even claim that the existence of some particular theoretical entity is directly supported by its use in the successful application of a theory. In general an application does not make use of all the properties of a theoretical entity but only of some of them. As such, a successful application does not directly show that the whole theory is true, nor even that some

[112] Laudan 1981
[113] Kitcher 1993b. pp.140-149

particular theoretical entity exists, but only that something or other with the relevant properties exists.

To illustrate, consider the theory that Father Christmas brings gifts at Christmas. Each year millions of children prove the application of this theory to be highly successful. More often than not, a child who follows the theory and puts out a stocking on Christmas Eve will find it filled in the morning. Laudan's extravagant realist takes the success of this application of Father Christmas theory to support the claim that the total Father Christmas theory is true. The extravagant realist concludes that is there is a jolly fat man with a beard and a red coat, who lives in Lapland with elves, and annually uses a flying sledge pulled by reindeer to deliver presents. Kitcher's more moderate realist notes that some of the theoretical entities of Father Christmas theory are not actually used in the stocking-filling application. When presented with the successful application he does not consider it as evidence for the existence of flying reindeer or elves, and chooses to limit his ontological commitments to Father Christmas. My truly sensible realist goes one step further and notes that many of Father Christmas's properties, his having a red coat, being fat and so on, are not used when he fills stockings. My realist takes the success of the stocking-filling application to be evidence, not for the existence of something with all of Father Christmas's properties (i.e. Father Christmas), but only for the existence of something or other that possesses the necessary properties for stocking-filling.

The truly sensible realist's conclusion may seem disappointingly modest. Often we will want to conclude rather more than that there is something with the properties required to enable our application to work. Fortunately, in some cases it will be possible even for the truly sensible realist to do this. One reason why only modest conclusions can be drawn from the success of the stocking-filling application is that Father Christmas Theory supplies us with no theoretical reasons for thinking that many of Father Christmas's posited properties are linked. There is no reason to suppose that he wears a red cloak *because* he is fat, for example, nor any suggested causal link between his having a beard and being generous. It is because we have no reason to think that the property of being able to fill stockings is causally related to Father Christmas's other distinguishing properties that finding that something filled the stockings gives no reason for thinking that something to also wear red, be fat, have a white beard, and so on.

In many cases, however, there will be reasons for thinking that a posited entity's properties are causally linked. In such cases finding that the entity has the properties required for the application to work can also be evidence that it has those properties that are thought to be causally linked to the properties that have been directly "probed" by the application. As such, in some cases the truly sensible realist will be able to go beyond claims such as "There is reason to think that something has a negative charge" and make claims such as "There is reason to think that there are electrons".

To put the same point another way, applications that depend on interactions with theoretical entities can be thought of as being similar to experiments from an epistemic point of view. Like experiments, applications can enable us to "probe" the properties of theoretical entities. Also like experiments, applications are limited, in that they will generally only probe certain properties. An experiment to measure the speed of light should not also be expected to demonstrate its particulate nature. Similarly, an application that works because electrons have a particular charge may well provide no information regarding their spin. When an application is successful we may conclude only that the theoretical entities or processes have those properties that are required for the application to work.

3.1.2 *Second Intuition: Pressures on a theory that arise from such applications will tend to make the theory more true.*

For feedback to be epistemically virtuous it must also be the case that when a theory is altered in such a way that an application becomes more successful there is a good chance of this producing a truer theory.

Here complications arise. The theory that comes closest to telling us the truth about the world need not be the theory that informs the most successful applications. One of the simplest scenarios whereby this can happen occurs when a true theory is too complicated to apply. To return to an example discussed earlier, in the 1960s some theorists thought that at least twenty or so factors needed to be taken into account before it could be decided which drug to use to treat a psychiatric patient. Now for the sake of argument let us suppose that these early theorists were right and that all these factors are important in determining the best treatment for a patient. Still, it might be the case that such a theory was not useful in practice. Plausibly, over-worked physicians would not usually have enough time to investigate all these factors. Let us suppose that those physicians who attempted to follow the theory were rarely able to gather enough information and so rarely treated anybody. In such a scenario the theory according to which a patient's diagnosis is the only important factor in deciding their treatment might be further from the truth, but might be more useful because its simplicity allows it to be applied far more easily. In such a case feedback stemming from the use of psychoactive drugs would tend to encourage the acceptance of this theory, despite it being further from the truth than its competitor.

The theory that can be most successfully applied need not be the truest theory because theories can have other virtues apart from being true. They can also have properties that make them easier for humans to remember and manipulate, such as being simple, being mathematically tractable, being visualisable, and so on. These properties might be thought of as properties that increase the "cognitive malleability" of a theory. All these properties are important. For us to be able to use a theory the world must not only be at least partly as it describes, but we've got to be able to understand and remember the theory.

The degree to which a theory is cognitively malleable may vary depending on the society that is trying to use it. Theories that are mathematically tractable now may well not have been prior to the development of particular mathematical techniques, and scientists who have been educated differently are likely to find it easier to think in different ways. More distinctly "social" factors can also be important. To return to our earlier example, a theory according to which twenty or so factors are relevant to deciding on a drug treatment may be unacceptably complicated in an over-stretched public hospital, but not in a well-funded private hospital.

Feedback that arises when a theory is applied in a way that depends on the manipulation of theoretical entities or processes will tend to increase the usefulness of the theory. However, this may be achieved either through making the theory more true or through increasing its cognitively malleability. As a result, sometimes feedback will encourage the adoption of a theory that is less true but more useful than its competitor.

It might be thought that whether this is a bad thing depends on whether the aim of science is conceived to be the development of true theories or of useful theories. However, I suggest that even someone who thinks that the aim of science is to produce useful theories should be somewhat worried by the possibility of such feedback. Feedback will tend to encourage the adoption of theories that are currently useful, or have been in the recent past. There is no reason why these theories should also be the most useful in the future. To return to our earlier example, let us suppose that in the 1960s theories according to which many factors needed to be considered to determine appropriate drug treatment were too complicated to be useful. This may have encouraged the adoption of the simpler, but let us suppose less true theory, that claimed that only the patient's diagnosis mattered. This theory was the most useful in the 1960s, but that needn't be so now. Now, social conditions and available technology have changed (for example, computers have become far more common) and perhaps it would be quite easy to apply the theory that claims that twenty factors are relevant in determining an appropriate treatment. Perhaps now the twenty-factor theory would be the most useful, but as this theory has now largely been forgotten it will never get a fair running. Thus feedback can result in the adoption of a less than optimally useful theory.

For someone who thinks that the aim of science is to produce true theories, feedback that leads to the adoption of less true but more useful theories is clearly epistemically speaking a bad thing. The one redeeming feature of such feedback is that in many cases we will at least have a good chance of knowing whether or not it is likely to occur. Feedback that leads to the adoption of less true but more useful theories has a chance of occurring when the true theory is too difficult to use. Plausibly, scientists will generally know whether or not this is a risk. The factors that can make a theory difficult to use are not occult, and are often explicitly recognised by scientists who will frequently state that, for example, they have had to make simplifying assumptions. When scientists experience no such difficulties in applying theories we will have reason to think that all the competing theories are easy enough to apply, and in such cases we have reason to think that no epistemically bad feedback will occur.

To take an example, the use of imipramine treatment to delineate anxiety disorders that are characterised by panic attacks is a straightforward example of feedback that arises from an application that involves manipulating theoretical entities or processes. Here the original theory says that anxiety disorders constitute a fundamentally distinct type of mental disorder. The application is to treat these conditions with specific drugs, and the theoretical processes that are being manipulated are the pathological processes that underlie the disorders. Feedback results in the adoption of a more useful theory according to which anxiety disorders characterised by panic attacks are fundamentally distinct from other anxiety disorders. In this case there is no reason to think that either of the two theories - that anxiety disorders are of a kind, and that disorders characterised by panic attacks are distinct - would be difficult to use and there appears no risk that feedback may here be promoting the adoption of a simpler but less true theory. Thus we have reason to think that this feedback is epistemically virtuous and leads to the adoption of a better theory.

3.2 Using The Words And Ideas Surrounding A Theory

In addition to using the theoretical entities posited by some theory it may also be possible to make use of the ideas and words that surround a theory. To take an example, in the late 19th century a Dr. Cram sold a patent medicine called Fluid Lightning that was claimed to contain liquid electricity.[114] At this time ideas of electricity had become linked with those of progress, excitement, cleanliness, and efficiency. As a result, linking products of all sorts with electricity was a good marketing ploy. Dr Cram made use of the ideas surrounding electricity in order to sell his medicine. However, the commercial success of Dr Cram's medicine can tell us nothing about electricity. Dr Cram didn't manipulate any electricity; there wasn't actually any electricity in his medicine. Rather Dr Cram just manipulated his customers.

In the case of Fluid Lightning there is no suggestion of feedback effects arising that could actually shape the theoretical knowledge surrounding electricity. In other cases, however, the use of ideas surrounding theories can produce feedback effects. In his paper "Late Victorian metrology and its instrumentation", Simon Schaffer describes the problems early workers in electromagnetism had in conceptualising the electromagnetic quantities they employed. While some defined electromagnetic terms, for example "ohm" and "farad", in terms of instruments others thought of them as referring to quantities of electrical fluid. The potential commercial consequences of the definitions were recognised by the participants of the debate. It was anticipated that electricity would prove easier to sell if units were defined in such a way that clients could think of themselves as buying quantifiable amounts of "potted energy". Such considerations played a role in the decision making of key developers of electromagnetic theory. In a letter to Clerk Maxwell, William Thomson wrote:

> When electrotyping, electric light, etc. become commercial, we may perhaps buy a microfarad or a megafarad of electricity...If there is a name given to it, it had better be given to a real, purchasable, tangible object, rather than to a quantity of electricity.[115]

Here we have an example where the anticipated effects of potential clients' ideas are able to influence a scientific theory.

[114] Nye 1990 p.153

[115] Thomson to Maxwell 24 August 1872, Cambridge University Library MSS ADD 7655/II/62. quoted by Schaffer 1992 p.32

The success of practices that make use of the words and ideas surrounding a theory need not be dependent on the theory being even partially true. Practices that make use of words or ideas do not probe the properties of the actual entities or processes and so cannot tell us anything about them. As such, the success of such practices gives us no reason for believing the theory, and there is no reason to expect that any adjustments to the theory that enable it to serve the application better will make the theory truer. This is not to deny that feedback that results from applications that make use of the ideas and words surrounding a theory *may* happen to make the theory more true, but if this happens it will just be by luck.

Complications arise in applying my account when the theoretical entities that an application manipulates are themselves ideas. Such cases arise when, for example, a counsellor uses a psychological theory to manipulate the ideas of a patient. In such cases my distinction between applications that manipulate theoretical entities and those that manipulate the words and ideas surrounding a theory can still be drawn. In this case the theoretical entities posited by the theory are those ideas that the theory claims that the patient has. For example, a psychological theory might claim that people can have repressed memories. The ideas surrounding the theory are the ideas that people have about the theory that are not posited by it. For example, people may think that the theory isn't popular with the advocates of False Memory Syndrome, or that it's fashionable to claim to have repressed memories.

To return to the D.S.M., I suggest that the use of D.S.M. categories for completing insurance forms is an example of an application that just employs the words and ideas surrounding a theory. As a consequence, the feedback that arises from the use of the D.S.M. in completing insurance forms is almost certainly epistemically speaking a bad thing. The basic problem is that the success of the practice of using "schizophrenia", "major depression", and so on to complete insurance forms is largely independent of the actual properties of schizophrenics and depressives. 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 doesn't require that patients actually have the diagnosed disorder, but only that the insurance company believes that they do. Here only ideas need to be manipulated.

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) found that diagnosis predicted only 3% in the variation in length of hospital stay, and concluded that "Diagnosis, even when clearly defined, is a poor indication of resource utilisation".[116] 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 doesn't matter whether or not

[116] McCrone and Phelan 1994 p.1025

those who are so diagnosed are actually schizophrenic. As the success of using the D.S.M. 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.

3.3 Applications That Use Both Theoretical Entities And The Ideas Surrounding A Theory.

Here I have discussed the epistemic significance of pressures arising from applications that just require the manipulation of theoretical entities and those that just require the manipulation of ideas. I suggest that these two sorts of application may be thought of as being at the two extremes of a continuum. Many applications will require the manipulation of both theoretical entities and of people's ideas. A scientist who wants an application to work may just need the world to co-operate, but if he wants to sell it he will also need people to want to buy it. Developing applications for sale will involve the manipulation of theoretical entities and of people's ideas to varying extents. Under some conditions it will be possible to sell something that is claimed to interact with theoretical entities when this is actually completely untrue. Thus Dr Cram was able to sell his Fluid Lightning even though his claim that it contained liquid electricity was a lie. In other cases the easiest way to make buyers believe that an application works will be to ensure that it actually does work. In such cases it will be necessary for an application to actually successfully manipulate theoretical entities for it to be saleable. Feedback arising from applications that depend on the manipulation of both theoretical entities and of ideas can be expected to have mixed effects, sometimes, but not always, it will promote the adoption of truer theories.

To take an example, consider the feedback that occurs as a result of marketing by pharmaceutical companies. Niche marketing by pharmaceutical companies can bring disorders that were previously rarely diagnosed to prominence. Alternatively, when pharmaceutical companies market one drug for the treatment of many disorders this encourages the disorders to be seen as being related. Whether the claims made by pharmaceutical companies actually need to be true in order for psychiatrists to believe them probably depends on the salience of the drug effects being claimed. Some claimed drug effects will be easy for individual psychiatrists to confirm or falsify. If a drug company claims that a compound makes 100% of alcoholics feel sick if they drink alcohol this will easy enough for an individual psychiatrist to check. In such cases marketing can only be expected to be successful in affecting psychiatrists' beliefs if the claims made are true.

On the other hand, some claimed drug effects will be impossible for an individual psychiatrist to check. In such cases a pharmaceutical company will be able to get away with lying far more easily. Claims that a drug is slightly more effective than another, or has a lower chance of causing a rare side-effect, can only be checked by someone who has access to very large numbers of patients. The experience of individual psychiatrists will not be sufficient to enable them to judge whether such claims are true. In *The Creation of Psychopharmacology* (2002) David Healy describes how the use of Randomised Clinical Trials, originally designed to protect patients, can perversely make it easier for drug companies to push ineffective drugs. Randomised Clinical Trials can be used to unearth the slightest treatment effect. They enable drugs to be marketed as "being effective" for, say depression, even when they do so little that their effects might be unnoticed by an individual patient or physician. As a consequence, Healy argues that Randomised Clinical Trials have increased the power of pharmaceutical companies to distort medical practice.

Of course, it is illegal for drug companies to make false claims about their products, but there is evidence that despite this legislation many drug adverts contain misleading or false information.[117] There is a chance that independent investigators may spot false claims made by a pharmaceutical company and publish their findings in medical journals. However, many physicians are more influenced by marketing claims than by papers published in such journals.[118] Thus, when drug companies make claims that are false but not blatantly so, they may be able to convince many physicians. In such cases treatments can come to be believed to have properties that they do not have. Feedback arising from such marketing will epistemically speaking be a bad thing.

3.4 Can Feedback Be Controlled?

Once it has been decided that some feedback arising from the use of the D.S.M. is epistemically undesirable the question arises as to whether, and if so how, such feedback can be controlled. The exact mechanisms via which feedback occurs will vary from case to case, thus no general account of how epistemically undesirable feedback can be prevented can be given. Still, the mechanisms via which feedback operates should not be thought of as being occult, and there is no reason why they can not be discovered in each case and measures be introduced to counter-act them.

To take an example, feedback that arises from the use of D.S.M. categories for completing insurance forms can 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 minimising the proportion of cases in which the psychiatrists will be torn between alternative categories. This route is already being adopted by the creators of Health Resource Groups (H.R.G.s), the British version of Diagnostic Related Groups, which are designed to facilitate financial planning within the N.H.S. Early versions of H.R.G.s 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",[119] it was decided to abandon this distinction. Later

[117] Wilkes et al. 1992
[118] Avorn et al. 1982
[119] N.H.S. Executive p.264

versions of H.R.G.s 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 70s 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 forms were not returned to the insurance company by the patient's employer.

The mechanisms by which feedback occurs can be discovered and it is possible to introduce measures to stop the feedback occurring. Here I have discussed the conditions under which feedback is a bad thing from an epistemic point of view. It should be noted, however, that feedback that is epistemically a bad thing need not necessarily be a bad thing all things considered. Epistemically speaking it is undesirable for psychiatrists to massage their patients' diagnoses. However, such practices do help individual patients to gain treatment and avoid stigma. In cases where an accurate diagnosis would cost the individual patient dearly it is plausible that psychiatrists might be morally justified in performing an epistemic sin and massaging their diagnosis. This point is best illustrated by considering an extreme example: In Hitler's Germany the severely mentally ill were killed. It is extremely plausible that a psychiatrist working in such an environment would be justified in recording false diagnoses for those patients that he believed to be suffering from severe mental illnesses. The general question of what should be done when epistemic requirements conflict with other requirements is too big to be tackled here. However, it is plausible that on occasion feedback that is epistemically bad is not bad all things considered. Thus I do not want to commit myself to the claim that all epistemically bad feedback should be stopped.

4.CONCLUSIONS

In my case studies I have shown that the D.S.M. has been shaped by pressures arising from the ways in which it is applied. I have also developed a general account of feedback in science that sheds light on the epistemic significance of the pressures on the D.S.M. According to this account much of the feedback affecting the D.S.M. (notably that arising from its use for completing insurance forms) is epistemically undesirable. The existence of such feedback makes it less likely that the D.S.M. committees will succeed in developing a classification scheme that describes natural kinds of disorder. Still, 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 D.S.M., however, such measures are not being taken. As we have seen, far from trying to prevent the D.S.M. being affected by insurance considerations the D.S.M. committee knowingly alters the classification scheme for insurance purposes. As a result I conclude that it is unlikely that D.S.M. categories will describe natural kinds of disorder.

CONCLUSIONS

As should by now be clear, the D.S.M. is a classification system of immense practical importance. As such it is important that the D.S.M. should be the best classification of mental disorders possible. Through examining the fundamental assumptions on which the D.S.M. is based, this book aims to contribute to assessing whether the D.S.M. is a satisfactory classification system.

The first half of the book examined the metaphysical assumptions behind the D.S.M. In the first chapter I assessed the account of disease on which the D.S.M. is based. The D.S.M. committee employed an account of disease according to which a disease is a harmful dysfunction. I argued that this account of disease is unsatisfactory. Instead, diseases are conditions that are bad things to have, that are such that the sufferer is unlucky, and that can potentially be medically treated. This account of disease is quite different from the D.S.M. account. However, the D.S.M. account could not have been used in practice. This is because in general we do not know enough about evolutionary biology to know whether or not a condition is a dysfunction. I suggest that in practice the committee assumed that a condition is a dysfunction if it seemed that sufferers are unlucky and if the condition seemed to have a biological or psychological basis. These conditions are so close to my conditions, that those with a disease are unlucky and that diseases are at least potentially medically treatable, that I suggest that little harm has been done to the D.S.M. through the committee explicitly adopting an incorrect account of disease.

The D.S.M. project has assumed that empirical research can tell us how mental disorders ought to be classified, and that the distinctions between mental disorders thus discovered will be theoretically important. In other words the D.S.M. assumes that types of mental disease are natural kinds. In the second chapter I refuted various arguments that have been supposed to show that mental disorders cannot be natural kinds and I developed a generally applicable account of natural kinds. According to this account it is plausible to think that some types of mental disorder will be natural kinds, while other types will not be natural kinds. If I am right, and at least some mental disorders are natural kinds, this is an important conclusion. Natural kinds and natural laws are linked. Thus, if at least some mental disorders are natural kinds, there will be laws, explanations, and sound inductive inferences in psychiatry – in short psychiatry will be a genuine science. In addition, if at least some mental disorders are natural kinds it makes sense to review empirical work in the hope that theoretically important distinctions between mental disorders might be found, and so the approach of the D.S.M. committees is justified.

The second half of the book examined epistemological issues. Even if some types of mental disorder are natural kinds is there any reason to hope that a classification system such as the D.S.M. will ever reflect their natural structure? In the third chapter I examined the epistemic problems that would follow from observation being theory-laden. Most philosophers of science hold that observation is theory-laden because they think that perception itself is theory-laden, that the language of observation reports is theory-laden, and that scientists require a theory to tell them what features of the world are worth investigating. I argued that there is insufficient evidence for it to be possible to judge whether or not perception in psychiatry is theory-laden. I also argued that the problems caused by the theory-ladenness of language can be side-stepped; while observation statements may not be theory-free they can at least be theory-neutral. However, I agreed that classification systems must always draw on some theory or other, as a theory must be used to decide which features of the entities under study are of scientific interest. As such a classification of mental disorders can only be as good as the best psychiatric theories. In so far as we have reason to doubt that the correct theories concerning mental disorders are known, we have reason to doubt that the conditions included in the D.S.M. are natural kinds.

In the fourth chapter I investigated the ways in which the D.S.M. has been shaped by its use by the medical insurance industry and by the pharmaceutical industry. I demonstrated that the D.S.M. has been substantially shaped by insurance considerations, and to a lesser extent affected by the needs of the pharmaceutical industry. I developed an account of feedback in science that makes it clear that the feedback arising from the use of the D.S.M. by insurance companies, in particular, is epistemically a bad thing. In principle it would be possible to introduce measures to limit the effect of such feedback on the D.S.M. However, far from seeking to limit the effects of insurance considerations on the D.S.M., the D.S.M. committees knowingly include categories for insurance purposes. As a result I conclude that it is highly unlikely that D.S.M. categories will come to describe natural kinds of mental disorder in the near future. Unfortunately it turns out that although the D.S.M. is of immense practical importance it is not on track to become the best possible classification of mental disorders.

APPENDIX

Definitions of "Mental Disorder" in the D.S.M.

1.D.S.M.-III (1980)

"... each of the mental disorders is conceptualised 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 important areas of functioning (disability). In addition there is an inference that there is a behavioural, psychological, or biological dysfunction, and that the disturbance is not only in the relationship between the individual and society."

2.D.S.M.-III-R (1987), D.S.M.-IV (1994) AND D.S.M.-IV-R (2000)

"...each of the mental disorders is conceptualised as a clinically significant behavioural or psychological syndrome or pattern that occurs in a individual and that is associated with present distress (e.g. a painful symptom) or disability (i.e. an impairment in one or more important areas of functioning) or with a significantly increased risk of suffering death, pain, disability, or an important loss of freedom. In addition, this syndrome or pattern must not be merely an expectable and culturally sanctioned response to a particular event, for example, the death of a loved one. Whatever its original cause, it must currently be considered a manifestation of a behavioural, psychological, or biological dysfunction in the individual. Neither deviant behaviour (e.g. political, religious or sexual) nor conflicts that are primarily between the individual and society are mental disorders unless the deviance or conflict is a symptom of a dysfunction in the individual, as described above."

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